Essays in Labor Economics: Peer Effects and Labor Market Rigidities

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Abstract

TETYANA SHVYDKO: Essays in Labor Economics: Peer Effects and Labor Market Rigidities
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The first essay of this dissertation, "Interactions at the Workplace: Peer Effects in Earnings", analyzes the impact that the average earnings of coworkers have on a given worker's earnings. We control for worker and firm fixed effects, which capture long-run individual and firm determinants of productivity. We interpret the effect of average coworker earnings net of these fixed effects as a measure of the effect of behavioral interactions at the workplace. This is the first study of peer effects at the workplace to use a large representative sample of workers and firms, constructed from the Longitudinal Employer-Household Dynamics files. We find that individual earnings increase by at least 8 cents for every one dollar increase in coworker average contemporaneous earnings and by at least 14 cents for every one dollar increase in the coworker average permanent component of earnings. Our estimates suggest that behavioral interactions at work might be a widespread phenomenon rather than a specific feature of workplaces where social interactions have been shown to be important.

The labor market is often asserted to be characterized by rigidities that make it difficult for older workers to carry out their desired trajectory from work to retirement. A potentially important source of rigidity is restrictions on hours of work imposed by firms, but such rigidities are difficult to measure directly. The second essay, "Labor Market Rigidities and the Employment Behavior of Older Workers", explores two variables that may serve as proxies for flexibility in hours at the employer level: the share of older workers and the share of young women in the employer's workforce. We use matched worker-firm data to analyze the effects of these variables on the separation propensity of older workers and the incidence of part-time work. The results show that older workers employed in firms with a greater share of older workers and a greater share of young female workers have a lower separation propensity from their employers. These results provide indirect but suggestive evidence of the importance of demand-side labor market rigidities in shaping the employment decisions of older workers.

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Chapter 1

Interactions at the Workplace: Peer Effects in Earnings

1.1 Introduction

There is abundant empirical evidence that members of the same group or social network tend to behave similarly in a range of economic and social outcomes. Positive correlation between the behavior of an individual and the behavior of his peer group can result from interactions among group members, or from the similarity of group members even in the absence of interactions among them. A large empirical literature investigates peer effects in schools, neighborhoods, migration, fertility, health behaviors, crime, and other domains. This paper studies interactions among coworkers at the workplace. Do my coworkers make me more productive? Or is my performance at work purely determined by my individual characteristics and ability, and employer characteristics, e.g., technology, past investment decisions, on-the-job training programs, etc.?

Formally, researchers distinguish between: (i) behavioral (endogenous) interactions, when the *behavior* of other members of the group directly affects the behavior of an individual member of the group, (ii) contextual (exogenous) interactions, when an individual's behavior is affected by exogenous group characteristics, and (iii) correlated effects, which arise when individuals form into groups with similar attributes and behave similarly because they have similar individual characteristics or face similar institutional environments (Manski, 1993).[1] In this analysis, we estimate the impact of mean coworker earnings on an individual's performance at work, as measured by the individual's earnings. We control for worker and firm fixed effects, which capture long run individual and firm determinants of productivity. We interpret the effect of coworker earnings net of these fixed effects as a measure of the effect of 'behavioral' interactions at the workplace.

Previous studies of workplace interactions have used data from a single firm or a very narrowly defined occupation, potentially limiting the generalizability of their results. To the best of our knowledge, this is the first study of peer effects at the workplace using data from a large number of firms. We use a large representative sample of workers and firms constructed from the U.S. Census Bureau's Longitudinal Employer Household Dynamics (LEHD) files, which contain the earnings history and basic demographic characteristics of the entire workforce in a representative sample of firms. The LEHD file system is based on state-level quarterly Unemployment Insurance (UI) administrative reports and has not been widely used by the empirical researchers so far.

We find that individual earnings increase by at least 8 cents for every one dollar increase in coworker average *contemporaneous* earnings and by at least 14 cents for every one dollar increase in the coworker average *permanent* component of earnings. The two models have different interpretations and the specification of the key explanatory variable of interest in the estimation of peer effects or the true model depends on the exact mechanism behind social interactions at work. For example, a worker might become more productive after joining a group of more productive workers, whose productivity is reflected in the *permanent* component of their earnings, when learning at work is an essential component of the production process. Alternatively, in cases of social pressure under the group-based compensation mechanism, a worker is likely to be responsive to the *current* performance of the team members irrespective of their permanent productivity. Contemporaneous interactions are also likely to be more

[1] Throughout the paper we use the terms behavioral effects, peer effects, social interaction effects, group interaction effects, and neighborhood effects to refer to what Manski (1993) calls the endogenous component of interactions. Social interactions are sometimes also called non-market interactions to emphasize the fact that these interactions do not occur via the price mechanism.

important in work groups with high turnover. The two models are equally plausible and it is ultimately not possible to empirically distinguish between them even when the specific firm level data is available (Mass and Moretti, 2008). Finally, it is natural to suspect that these two models of peer interactions can operate simultaneously. We present estimates of peer effects from both models in our analysis.

We also examine how the magnitude of the peer effect varies by firm size. The effect is larger in small firms when peer behavior is measured by contemporaneous earnings of coworkers. This could indicate that peer effects are more important in smaller workplaces, or that the relevant peer group is measured with greater error in larger firms. The effect is independent of firm size when the long-run earnings performance of coworkers is used instead. We find that peer effects are larger at lower levels of contemporaneous earnings. This result supports findings of several case studies which found that peer interactions are an important characteristic of low-skilled occupations (e.g. grocery scanners as in Mas and Moretti, 2008, or fruit pickers as in Bandiera, Barankay, and Rasul, 2007), but not present in high-skilled labor markets (professional golfers as in Guryan, Kroft, and Notowidigdo, 2007). The effect is reversed, i.e. is larger, for permanently higher earners.

Behavioral interactions imply the existence of indirect effects of any interventions on individual behavior at work through coworker networks in addition to their direct effects. That is, there may be a social multiplier (Scheinkman, 2006). In contrast, contextual and correlated effects do not imply the existence of a social multiplier. On the firm side, the analysis of peer effects at work has significant implications for questions such as optimal design of the workplace, choice of the most efficient system of compensation incentives, the evaluation of potential new hires, etc. Peer effects estimates might be of considerable interest to individuals evaluating ways to improve their own productivity (e.g., taking additional individual training or looking for a new position at the firm with a more productive set of coworkers). Previous studies of workplace interactions are case studies and are able to directly address some of these issues as applied to the specific firm under analysis. Our estimate is an average across a large number of heterogeneous work environments, suggesting that behavioral interactions at the workplace might be a widespread phenomenon rather than a specific feature of workplaces where interactions have been shown to be important (e.g. grocery scanners or soft-fruit pickers). Our results help to generalize previous findings as they are representative of the population of individuals and firms and potentially increase their applicability.

The next section briefly reviews the literature on interactions at the workplace. Section 1.3 discusses the conceptual framework behind the use of individual earnings as an outcome measure of individual 'behavior' at work, while Section 1.4 develops the empirical model used in our analysis. The data are described in Section 1.5. Section 1.6 presents and discusses the empirical results. Conclusions and a discussion of future research are given in Section 1.7.

1.2 Previous Research

Empirical studies of social interactions at the workplace are limited compared to the rest of the empirical literature on peer effects, because suitable data are not readily available. Ichino and Maggi (2000) investigate the role of group interactions in shirking at work. Drago and Garvey (1998) study helping efforts within work groups under different compensation schemes. Most of the existing studies of workplace interactions focus on investigating interdependence in coworker productivity. In this section, we briefly summarize these analyses and explain how our research complements and enhances current knowledge on peer interactions at work.

It is rarely the case that each agent's individual contribution to output is both observable by the firm's manager and independent of the contributions of other coworkers.[2] Most production processes involve interactions among workers. Consider assembly line production, with the output of some workers being an input for other workers. Even if individual effort is perfectly observable, a slowdown in the productivity or effort of a particular worker will lead to a fall in productivity of consecutive workers in the line, provided that inputs cannot be easily stored. Such a production process leads to interdependence in productivity among workers due to the

[2]It is well understood that the most direct and effective monetary incentive when individual output is observable and independent of coworker output is an individual performance or piece-rate pay scheme (see, for example, Lazear, 2000; and Bandiera, Barankay and Rasul, 2005).

organizational design of the production process.[3] Consider further the situation in which total output is observable to the employer, but the individual contribution of team members is not easily measurable.[4] This is often referred to as a team-based production technology. Note that it is the cost of monitoring individual contributions that makes it a team production. Thus, even a group of carpenters building a house or a group of cashiers working in the same shift would be considered a team (as in Mas and Moretti, 2008).

If individual effort is not easily observed, and assuming that providing effort is costly, individual team members have an incentive to shirk. If the cost of monitoring precise individual contributions to the final product exceeds the benefit in terms of reduced shirking, employers might use efficiency wages in order to give workers the incentive to avoid shirking. Alternatively, an employer may introduce group-based compensation. Group-based compensation creates conditions for interactions among coworkers to ensure that a sufficient level of individual effort is provided by all group members, because all workers benefit from individual productivity increases.[5] Consequently, workers might choose their productivity or shirking levels strategically in response to those chosen by their colleagues, leading to a 'behavioral' interaction among team members.[6]

Hamilton, Nickerson, and Owan (2003) use weekly productivity reports data from a garment plant, and Mas and Moretti (2008) use scanner data from six branches of a national supermarket chain to investigate endogenous productivity interactions in teams and the mechanisms through which such interactions operate. These studies find that workers respond to

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[3]Guryan, Kroft, and Notowidigdo (2007) call this a 'production complementarity' effect.

[4]In some cases, it might not only be difficult to measure individual contributions by workers performing the same task, but even impossible to assign tasks or jobs to individual workers who perform or fill them. Examples of those include small and medium-sized family-based businesses and technology-based innovative firms as in Chillemi and Gui (1997).

[5]This feature of the incentive structure of group-based compensation was formally conceptualized in a theoretical framework by Kandel and Lazear (1992).

[6]Team output may also benefit from 'collaborative skills' involving communication and leadership in addition to 'technical' abilities used in individual piece rate production (Hamilton, Nickerson, and Owan, 2003) and 'team human capital' or informal but effective customs or norms for successful task performance developed and preserved by team members (Chillemi and Gui, 1997). This may lead to more productive use of already existing technologies and invention of new ones.

the productivity of their team members, and provide evidence that behavioral peer interactions are likely to operate through peer pressure in the form of mutual monitoring and the imposition of team norms. They further find evidence that less productive workers are more responsive to peer influences.

Different types of pay based on group performance, such as profit sharing, team-based incentives, or stock option plans, can be an independent source of coworker interactions. Some firms, which can explicitly measure individual productivity as in Weiss (1987), Hansen (1997), Rees et al. (2003), nevertheless adopt group-based compensation in order to induce additional beneficial incentive effects among their workers through behavioral peer interactions.[7] In their empirical application, Rees et al. (2003) find evidence of strong interdependency in productivity levels of coworkers tied only by group compensation incentives.

Interestingly, Rees et al. (2003) also show that individual productivity is affected by the average productivity of coworkers who do not belong to the same compensation-tied group. They attribute this to the existence of social interaction mechanisms, which extend beyond the obvious formal factors such as team-based production and group-based compensation practices used by the firm. As hypothesized by Rees et al. (2003), "expected returns to individual effort and thus subsequent choice of effort level might vary with the evolution of the permanent component of market demand which in turn can be identified through exchanges of information among coworkers about their sales revenues, resulting in positive correlation between workers productivity levels even without group based production or compensation components" (p. 587). In other words, information transmission and learning from coworkers how to best perform a given task also leads to behavioral peer interactions at work.[8]

Falk and Ichino (2006) designed an experiment, in which subjects stuffed letters into envelopes and were paid *independently* of output. The results showed that average output was

[7]For example, a telephone sales firm analyzed by Rees et al. (2003) is able to observe individual monthly sales of all its workers, but still chooses to use group compensation schemes rather than simple individual pay for performance compensation.

[8]Manski (2000) distinguishes between three sources of behavioral interactions or peer effects: (i) expectations, (ii) preference and (iii) constraint interactions. Observational learning generates expectation interactions if agents form expectations based on observation of actions chosen and outcomes experienced by others.

higher for individuals who worked in the same room, i.e., were able to observe each other, than for individuals who worked alone. These results could be generated *only* if individuals cared about their own performance relative to the performance of their coworkers, because alternative mechanisms of behavioral interactions through production technology (simple individual level task, not much to learn) and compensation incentives (fixed individual pay) were ruled out by the experimental design.[9]

It may well be the case that peer effects of this kind are stronger in actual workplaces because people know each other, in contrast to the subjects of Falk and Ichino experiment, who had never met before. In fact, Bandiera, Barankay, and Rasul (2007), using productivity data from a soft-fruit picking firm enhanced with survey information about friendship networks among the firm's workers, find that the mere presence of friends significantly affects individual worker productivity, but that productivity does not respond to the skill level of non-friend coworkers. Individual productivity is perfectly observed and workers are paid piece rates, so there are no externalities arising from either the production technology or compensation scheme.

The only study of peer effects at the workplace that fails to find evidence of peer effects is a study of playing partners in professional golf (Guryan, Kroft, and Notowidigdo, 2007). The absence of peer effects among professional golfers may mean that workers seek to avoid responding to social incentives when individual financial incentives are strong, and that workers learn with professional experience not to be affected by peers. This result suggests that there is heterogeneity in susceptibility to peer interactions, and that in elite professional labor markets workers might be more likely to avoid the influence of peers, unless they are linked by production technology or group based compensation.

Even though all of the empirical studies of interactions in worker behavior cited above are case studies and each is designed to answer a different question, some generalizations are possible. First, empirical evidence suggests that behavioral peer interactions at the workplace can

[9]Individual productivity can increase in return for recognition by coworkers (as in Hollander, 1990) or as a result of conforming behavior (as in Bernheim, 1994). These are perfect examples of preference generated group interactions at the workplace, which occur when an agent's preference ordering over the alternatives in the choice set depends on the actions chosen by other agents (Manski, 2000).

arise solely as a result of preference, expectations or constraint interactions among coworkers, as classified by Manski (2000). Production technology and pay based on group performance create additional incentives for these interactions. Second, peer effects on average tend to enhance individual productivity, although there is considerable heterogeneity in peer effects across individuals and across labor markets. Third, it seems that low productivity workers are more responsive to the behavior of coworkers (Mas and Moretti, 2008), although there is also some evidence that high productivity workers slow down in the presence of friends (Bandiera, Barankay, Rasul, 2007) or after the introduction of teams (Hamilton, Nickerson, and Owan, 2003).

But, as in all case studies, the specific results are not easily generalizeable across heterogeneous industries, occupations and firms. In contrast to one firm/one occupation studies, this paper estimates peer effects in a large representative sample of firms, thus making our inferences more generalizeable. Earnings is the only readily available and comparable measure of 'behavior' in the workplace in an analysis of a large representative sample of firms. Consequently, this paper should be viewed as analysis of peer effects in earnings.[10] Below, we discuss the implications of studying peer effects in earnings for the estimate of peer effects in productivity.

1.3 Conceptual Framework

This section discusses the theoretical mechanisms behind an empirical analysis of the association between individual earnings and mean earnings of coworkers presented in this paper, and the relevance of using earnings as an outcome or a measure of individual 'behavior' at work. The mechanics of introducing non-market interactions into a standard model is rather straightforward. Complementarities in behaviors will emerge if the individual utility of undertaking an action depends not only on individual characteristics, but also on the characteristics

[10] To the best of our knowledge, only Rees et al. (2003) used individual monthly sales revenue as an outcome measure for their analysis of group interaction effects. Monthly sales revenue is an unambiguous measure of individual productivity in their application because their data come from a telephone sales firm.

and either the expected or the actual actions of the individual's peers. There are several possible sources of such interactions. Some models assume that there are psychological reasons why individuals might care about their own output and pay relative to the output and pay of their coworkers even when they are paid based on individual performance. These reasons include status formation, including first impressions, approval or recognition by others (Hollander, 1990), compliance with norms (Bernheim, 1994), contagious enthusiasm, or aversion to pay inequality within their group of work peers (Fehr and Schmidt, 1999; Charness and Rabin, 2002). Mass and Moretti (2008) suggest naming this broad category of mechanisms *pro-social preferences* defined to encompass all cases where workers lose utility if they act non-cooperatively, regardless of whether or not they are being observed behaving this way by their work peers.

Other models emphasize the information transmission that occurs when a person observes the choices of others (*knowledge spillover* mechanisms in Mas and Moretti's classification). The consequence of any of these models is that a person's behavior is a function of the behavior of the members of his peer group in addition to the usual determinants of individual behavior. In all these cases, peer effects in earnings will reflect behavioral interactions in the workplace even if individual pay is determined strictly by individual performance.

As discussed in the previous section, behavioral peer interactions at the workplace can result purely from the externalities generated by a group based compensation scheme, as in Kandel and Lazear (1992). If individual effort contributions to the production of final output are not easily observable, and assuming that providing effort is costly, free-riding is likely. Each individual has an incentive to spend some effort to make sure that other members of the group do not shirk in case everybody is paid based on the total output produced by the group. Kandel and Lazear (1992) call this action 'peer pressure'.[11] If workers have the means to exercise pressure on each other, then optimal individual effort is larger than in the absence

[11] Mas and Moretti (2008) call all mechanisms that involve a loss in individual utility if observed behaving non-cooperatively by coworkers *social pressure* class of interaction mechanisms, and find evidence of social pressure induced productivity spillovers among supermarket cashiers in the fixed pay setting. Indeed, peer pressure can be operative even in a fixed individual pay setting if the employer, for example, can eventually fire workers who shirk more than average or promote those who shirk less. Thus, a worker has an incentive to react to the behavior of coworkers if he wants to keep his job.

of peer pressure, and so are total output and individual earnings. Note that the nature of peer pressure is social because it depends on coworker efforts. In this type of model peer pressure is the source of positive correlation between individual earnings and earnings of other members of the group whose compensation is jointly determined. In this example, peer effects in earnings reflect behavioral interactions at the workplace.

The ultimate question of interest is indeed peer effects in productivity. In other words, it would be unnecessary to control for average coworker earnings in the model of individual productivity as a function of coworker average productivity if perfectly observed. By using a large sample of firms, we sacrifice the availability of detailed information on technology and payment schemes used by a particular firm, but most importantly direct measures of productivity. If individual effort cannot be measured directly, two situations are possible. In the first case, if people are rewarded based on individual or group performance, then the analysis of social interactions at the workplace using individual earnings as an outcome measure is equivalent to the analysis of peer effects in productivity. In the second case, if individual earnings do not depend on individual or group performance, then the estimate of the behavioral interaction effect using earnings as an outcome measure will be zero, even if coworkers interact behaviorally in ways that affect productivity. In other words, pay for performance is not a necessary condition for behavioral interactions at the workplace, but it is necessary for detecting such interactions while analyzing earnings as an outcome measure.[12] We do not have information on specific compensation schemes used by each firm in our data. Therefore, mixing firms with performance-independent pay and firms with performance-based pay will yield an average estimate of behavioral interactions in earnings lower than that in productivity.

1.4 Empirical Model

Conducting a sound empirical analysis of peer behavior is challenging due to both conceptual and data limitations. Empirical research on endogenous group interactions must tackle

[12] Using the PSID data, Lemieux, Macleod, and Parent (2006) find that the overall incidence of performance based pay was a little more than 30 percent in the late 1970's, but grew to over 40 percent by the late 1990's (close to 50 percent among salaried workers).

the following set of conceptual problems. First, the empirical analysis must separate endogenous peer effects from the effects of confounding factors such as the influences of exogenous characteristics of group members or common institutional environment, or shocks faced by all group members. Second, individuals might self-select into specific groups. The endogeneity of group choice will lead to the estimation of spurious social interaction effects if characteristics that affect both group membership and individual behavior are omitted. Finally, empirical analysis must account for the simultaneous determination of individual agent's behavior and behavior of other members of his reference group. This creates two problems. The first one is the so-called 'reflection problem' (Manski, 1993). If one observes that individual behavior is correlated with the expected average behavior in a group or neighborhood, this may be due to the fact that the group outcome simply reflects the role of exogenous characteristics in influencing individuals, provided that the behavior of each peer is governed by the same relationship. The reflection problem is a source of identification failure in the linear-in-means models. Second, the simultaneity of behavior is a source of endogeneity between individual and group outcomes as every member of the group is each others peer. In other words, there can be peer group effects from the individual under analysis to other group members in the empirical model of individual outcome as a function of average group outcome.

The most recent generation of theoretical and empirical research has made significant progress in developing models and methods to make the estimation of group interaction effects possible.[13] Despite this apparent progress, the choice among these identification strategies crucially depends on the specific data available to the researcher. We rely on the longitudinal structure of our data in our empirical analysis of peer interactions at work.

As discussed in Brock and Durlauf (2001), the availability of panel data on individual and group outcomes permits one to specify the following interaction-based model with fixed effects

[13]These include: (i) use of random assignments into groups to identify peer effects (Sacerdote, 2001; Zimmerman, 2003; Katz, Kling, and Liebman, 2001; Ludwig, Hirschfeld, and Duncan, 2001); (ii) use of peer group background characteristics as instruments for peer group outcomes (Case and Katz, 1991); (iii) use of aggregate statistics such as the variances of group average outcomes (Glaeser, Sacerdote, and Scheinkman, 1996; Graham, 2004) or social multiplier (Glaeser, Sacerdote, and Scheinkman, 2003); and (iv) use of group size (Lee, 2006; Lin, 2005), spatial (Bertrand, Luttmer, and Mullainathan, 2000; Topa, 2001) or time (Ichino and Maggi, 2000) variation to identify peer or neighborhood effects.

for unobserved time invariant factors which affect or determine the behavior of individuals, and fixed effects for unobserved characteristics of the group (or firm in our specific application):

$$Y_{ijt} = c + \alpha X_{it} + \beta \overline{Y}_{-i,jt} + \gamma Z_{jt} + \theta_i + \zeta_j + \epsilon_{ijt}, \tag{1.1}$$

where Y_{ijt} is an outcome of individual i who is employed by firm j at time t, X_{it} is a vector of observed individual characteristics in period t, $\overline{Y}_{-i,jt}$ is the average outcome of *other* employees of firm j in period t, Z_{jt} is a vector of predetermined or exogenous group characteristics of firm j in period t, θ_i is a time-invariant person effect, ζ_j is a time-invariant firm effect for firm j, and ϵ_{ijt} is an error term.[14] Individual fixed effects in this model are identified by multiple observations on a given worker's earnings at a given firm. Firm fixed effects are identified because workers move across firms, so the set of workers observed at a given firm changes over time.

The parameter of interest in this model is β, which measures the magnitude of the peer effect in earnings. Controlling for time invariant individual and firm unobservables in (1.1) ensures that the estimate of peer effects, β, is not contaminated by correlated effects. Firm fixed effects, for example, account for permanent differences between low and high paying firms. This fixed effects approach also eliminates bias due to self-selection into firms based on time invariant individual and firm unobservables, e.g. sorting of more able individuals to more productive firms if reflected in individual and average firm earnings. There are two potential sources of bias remaining in the estimate of β in model (1.1): the first is due to time varying unobservables affecting all workers at a given firm in the same quarter (e.g. demand or technology shocks), the second is due to the simultaneity of coworker behavior. With longitudinal data, the lagged values of the group outcome as in Hanushek et al. (2003) and Ichino and Maggi (2000) or lagged values of exogenous group characteristics as in Lee (2007), Lin (2005), and Ichino and Maggi (2000) are logical candidates for instruments. We use both approaches.

[14] The mean of coworker outcome is the most commonly used parameterization of the peer effects function. Alternatively, individual behavior can be affected by the best or worst performer in a group, and so forth.

The availability of longitudinal matched employer-employee data allows us to estimate a model similar to that in Mas and Moretti (2008) and Guryan, Kroft, and Notowidigdo (2007) by analyzing the mean of *long-run average* earnings of coworkers as well as the mean of individual fixed effects of coworkers from the corresponding individual earnings regressions in place of the mean of *contemporaneous* coworker earnings as the key explanatory variable of interest

$$Y_{ijt} = c' + \alpha' X_{it} + \beta' \overline{Y}_{-i,jt}^{long-run} + \gamma' Z_{jt} + \theta_i + \zeta_j + \epsilon_{ijt}, \tag{1.2}$$

where all the variables are the same as in model (1.1) and $\overline{Y}_{-i,jt}^{long-run}$ is the mean of the long-run coworker earnings. We are able to construct this measure because our dataset contains the earnings history of *every* person employed by our sample of firms.[15] By definition, the endogeneity bias due to common shocks or simultaneity of behavior does not arise in this approach because it regresses the current individual outcome on the average permanent component of the outcome measure of coworkers.

The parameter β' in equation (1.2) represents the effect of the permanent coworkers productivity if reflected in their earnings on worker $i's$ current productivity. The parameter β in equation (1.1) represents the effect of contemporaneous earnings performance of coworkers, rather than their permanent earnings performance. The two models have different interpretations and the specification of the key explanatory variable of interest in the estimation of peer effects or the true model depends on the exact mechanism behind social interactions at work. The two models are equally plausible and it is not possible to empirically distinguish between them, even when the specific firm level data is available (Mass and Moretti, 2008). Finally, it is natural to suspect that these two models of peer interactions can operate simultaneously. Therefore, we reconcile our results as those possibly capturing some combination of a true effect of the permanent coworker performance and a true effect of contemporaneous coworker performance with different measures of the average coworker outcome capturing

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[15] We construct multiple versions of this variable for specifications tests. See detailed discussion on the construction of these variables in the Results Section 1.6.

different mechanisms inducing earnings/productivity spillovers.

There are two sources of identification of peer effects estiates in models (1.1) and (1.2).[16] One is from individuals who stay with the same employer in two consecutive periods (stayers). The other is from individuals who change employers from one period to the next (movers). Stayers may not contribute much to identification if average earnings in firms change slowly over time, which is especially likely to be the case in large firms. The use of stayers in small firms will suffer from the endogeneity bias due to simultaneity of behavior to a greater extent as the average earnings will be computed for smaller groups.

The computational feasibility of estimating (1.1) and (1.2) depends on the number of individual and firm fixed effects to be estimated.[17] Computationally, we use within-individual first differencing to eliminate the individual fixed effects. First differencing generally exacerbates measurement error bias. This happens when the variable measured with error is correlated over time. For stayers, average earnings are much more likely to be correlated across quarters, e.g. due to unobserved firm-specific time varying characteristics. For movers this is much less likely because we compare average earnings of coworkers in one firm with average earnings of coworkers in a different firm. The dataset we use is from administrative unemployment insurance records. This means that quarterly earnings data *per se* do not suffer from the measurement problem to the same extent as self-reported earnings data in survey datasets. On the other hand, average earnings of coworkers are meant to capture the average outcome or behavior of the *relevant* peers in peer effects application. Since we do not observe the relevant peer group, the average earnings of coworkers are indeed measured with error in our case. Thus, using data on movers helps to address this measurement error bias.

Finally, the identifying variation in the approach with contemporaneous coworker average earnings is due to quarterly changes in individual earnings as well as changes in the firm's workforce composition across quarters. The identifying variation in the approach with the

[16]Manski (1993) notes that identification fails in linear in means models due to the reflection problem. Equation (1.1) appears to be linear in means, but as discussed below, the Z variables used in the model are not linear aggregates of the individual level X variables, so our empirical model is technically not linear in means.

[17]See Abowd, Kramarz and Margolis (1999) for an extensive discussion of this issue.

mean of long run average earnings of coworkers is due to the quarter to quarter changes in each firm's workforce composition only, i.e., it is even smaller than in the approach with contemporaneous average earnings. The use of the sample of movers maximizes the variation in the major explanatory variables of interest.

Consequently, we estimate (1.1) and (1.2) in first differences using *only* data on movers to address the above problems.[18] Assuming that individual i is employed by firm j' in period $t-1$ and by firm j in period t, our estimating equations are:

$$\begin{aligned} Y_{ijt} - Y_{ij't-1} = \alpha(X_{it} - X_{it-1}) + \beta(\overline{Y}_{-i,jt} - \overline{Y}_{-i,j't-1}) \\ +\gamma(Z_{jt} - Z_{j't-1}) + (\zeta_j - \zeta_{j'}) + (\epsilon_{ijt} - \epsilon_{ij't-1}), \end{aligned} \tag{1.3}$$

and

$$\begin{aligned} Y_{ijt} - Y_{ij't-1} = \alpha'(X_{it} - X_{it-1}) + \beta'(\overline{Y}^{long-run}_{-i,jt} - \overline{Y}^{long-run}_{-i,j't-1}) \\ +\gamma'(Z_{jt} - Z_{j't-1}) + (\zeta_j - \zeta_{j'}) + (\epsilon_{ijt} - \epsilon_{ij't-1}), \end{aligned} \tag{1.4}$$

where all the variables are defined as before, but all firm-specific measures are computed not only in two different periods, but also at two different firms, j and j'.[19] We include exactly two time periods per person, a quarter before the move and a quarter after the move, which is the minimum amount necessary to difference out individual fixed effects. Note that estimation of (1.3) and (1.4) requires estimation of the full set of J firm fixed effects, where J is the total number of firms in the sample of job movers.[20]

We analyze individual quarterly earnings at the old firm from one full quarter before the

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[18] A brief summary of how our sample movers differ from stayers is provided in the Data Section 1.5 below.

[19] A similar empirical strategy was implement by Ichino and Maggi (2000), who studied the sources of regional shirking differentials in Italy, by Krueger and Summers (1988) and Gibbons and Katz (1992), who analyzed the reasons for inter-industry wage differentials, and Aaronson (1998), who studied the impact of neighborhoods on children's educational outcomes.

[20] We could also estimate our two level fixed effects models (1.1) and (1.2) in differences in means form following the methodology described in Davis (2002). It should be noted though that differences-in-means method is computationally equivalent to estimating (1.3) and (1.4) in case of unbalanced panels. Our panel is unbalanced at the firm level because we observe individuals moving across different firms.

quarter of job change and individual quarterly earnings at the new firm from a full quarter after the quarter of job change. This translates to a period of 3 to 6 months before and after the job change. In other words, our estimate of peer effects is not measuring peer influences based solely on workers who *just* arrived to the new firm.

To be fair, the model behind behavioral interactions for workers who are about to leave the firm might be rather different than for workers who just started at the new firm. If the social interaction function is assumed to depend on the permanent productivity of coworkers, the focal worker is assumed to know the productivity of his coworkers and adjust his behavior accordingly, irrespective of their productivity at a particular point in time. This might be true for workers before the move. On the other hand, if peer function is assumed to depend on contemporaneous coworker behavior, workers are assumed to influence each other only through their performance at a particular point in time. This can be true to a much bigger extent for newly arrived workers.

As for the mechanisms, Mass and Moretti (2008) suggest the following classification: social pressure, pro-social preferences, and knowledge spillovers (see definitions in the Conceptual Framework Section 1.3.). All of these mechanisms can be at play for both situations. Social interactions through knowledge spillovers are probably more important for newly arrived employees, although a tenured worker might still benefit from the knowledge brought in by the new hires during his last quarter of employment at an old firm. Workers can be characterized by pro-social preferences irrespective of their tenure at the firm. Finally, the social pressure mechanism can take place at any firm at any time. Consequently, the question of what we learn about peer effects from a sample of workers who just arrived to the new firm simply reduces to the question of how fast we would expect the social interactions mechanisms to come into play. It certainly depends on specific industry (supermarket cashiers and fruit pickers versus college professors or software engineers), specific technology (team versus individual production) and compensation scheme (fixed rate, piece rate, group based performance pay, etc.) used by a particular firm. That is why a detailed industry specific analysis of peer effects is an interesting topic for future research. To summarize, if one is willing to assume that the group of movers is representative of the general population of employees, in the sense of being characterized by the same behavioral parameters, our results are representative of the entire population of workers.

1.5 Data

In our empirical analysis, we use unique matched longitudinal employer-employee data for the United Sates - the Longitudinal Employer-Household Dynamics (LEHD). The LEHD Infrastructure File system is confidential dataset because it is based on state Unemployment Insurance (UI) administrative files and is accessible only through the network of Census Bureau Regional Data Centers. For the years 1990 - 2004, the data are available from 31 states covering about 80% of U.S. employment, although the period covered varies by state (Abowd, Haltiwanger, and Lane, 2004).

The LEHD is constructed as follows. Employers covered by UI file a quarterly report for each individual who received any covered earnings from the employer in the quarter. An 'employer' in this context is a UI-tax-paying entity, roughly equivalent to an establishment. UI covers about 96% of private non-farm wage-salary employment, with lower coverage of agricultural and government workers, and no coverage of the unincorporated self-employed. These UI records are the basis for the LEHD Employment History Infrastructure Files. These are state-specific files containing information on the quarterly earnings of *each* individual from *each* employer from which the individual has any covered earnings during a specific quarter, the individual's Social Security number, and an identification number for the employer. These data are then merged with the Census Personal Characteristics Files, which contain date and place of birth, sex, and a crude measure of race/ethnicity for each individual in the data. There is no information about education and occupation in the LEHD. Additional establishment-level information such as industry, location, and ownership type is merged in from the Employer Characteristics Infrastructure Files. An extensive discussion of the construction and the content of these files is provided in Abowd et al. (2006).

Here, we use a custom extract rather than the full LEHD. The custom extract provided to us by the Census Bureau contains earnings records for respondents to the Survey of Income and Program Participation (SIPP) (1990 - 2001 panels) as well as for all coworkers of the SIPP respondents. The extract consists of about 450 million person-job quarterly earnings records for about 150 million people employed by about 423,000 firms. The employer-level data in the custom extract is aggregated to the firm level within each state. If a firm owns several establishments in a given state, all of these establishments constitute a single employer or firm. However, if a firm owns establishments in several states, its establishments in one state are a different employer in our data than its establishments in another state. This reflects the fact the UI is administered at the state level.

We constructed the sample of movers used in our empirical analysis as follows.[21] Using the Employment History files of the LEHD custom extract described above, we select 16-69-year-old individuals who changed employers during a quarter and have valid earnings data available for both a quarter before and after the move. Valid earnings data is defined as quarterly earnings not less than full time minimum wage quarterly earnings. We further select only those workers who worked at least one full quarter in the old firm before the move and in the new firm after the move, i.e., with positive earnings *two* quarters before and after the move. Finally, we use only within-state movers.[22]

Our final sample consists of 2,395,640 individuals who move across 246,517 firms sometime during 1990 - 2004 within one of 31 LEHD participating states. We use two quarters of data for each individual, the quarter before changing firms, and the quarter after changing firms. This

[21] The employer identifier used in all the LEHD's files is a state-specific account number from that state's unemployment insurance accounting system used to administer the tax and benefits of the UI system. These employer identifiers can change for a number of reasons, including simple change in legal form or firm mergers (Abowd et al., 2006). If an employer changes firm identifier, but makes no other changes, the worker would appear to have left the original employer firm and might have been allocated to the sample of movers even though his or her employment status should have been classified as one of a stayer. The potential way of dealing with this problem is to track large worker movements between unique employer identifiers. Using the LEHD data for 18 states for the period 1992 - 2001, Benedetto et al. (2004) report that the movement of the clusters of workers that number 5 or more is quite rare, accounting for about 0.2% of all worker movements across firms with different firm identifiers. We have not cleaned our final sample of movers from seemingly spurious changes in employer identifiers. This simply means that our final sample may contain some data on stayers. As discussed previously, the analysis of stayers is not theoretically wrong rather it reduces identifying variation in the major variable of interest and suffers from a measurement error bias to a greater extent than the sample of movers. In other words, the failure to identify and exclude 'spurious' movers results in lower estimate of peer effects coefficient if anything.

[22] We analyze within-state movers because we processed the data on a state-by-state basis. Additional work has to be done to incorporate information on people who change employers across states.

yields a sample of 4,791,280 person-quarters. The availability of the data on basic demographic characteristics of the firm's workforce allows us to construct detailed age/sex/race workforce distribution variables such as fractions of workers of different ages, fractions of females, whites and blacks. The longitudinal nature of our data makes it possible to control for a firm's workforce turnover characteristics such as accession and separation rates and rate of change in total firm employment.[23] We also know the establishment structure of each firm (whether the firm has multiple plants and if so, how many plants). Column 1 in Table 1.1 presents summary statistics of this sample.

Unfortunately, computational constraints prevent estimation of equations (1.3) and (1.4) given the very large number of firms in the sample. Indeed, we would need to include 246,517 firm fixed effects in the estimation of this equation. Instead, we use a random subsample from the full sample of movers. First, we randomly select 0.3% of individuals from the total of 2,395,640 movers from the full sample described above. Then, we identify the firms that employed each individual in the quarter before the individual moved and in the quarter after the move. This yields 10,961 firms. Finally, we select *all* individuals from the full sample of movers who move both to and from one of these 10,961 firms. This ensures that we have multiple movers per firm, which is necessary for the identification of firm fixed effects. Our new sample (sample 2 or random sample) consists of 753,728 person-quarters for 376,864 individuals and 10,961 employers.[24] Descriptive statistics of this sample are provided in Column 2 of Table 1.1. It seems that we are over-sampling movers across larger firms compared to the original full sample of movers (see summary statistics for the firm size and multi plant indicator variables). It also seems that we over-sample high earners and high paying firms (see summary statistics of individual and coworker average earnings). This happens because we are including *all* movers

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[23]The accession rate is defined as the number of workers with positive earnings in quarter t who were not employed in quarter $t-1$ divided by the average number of workers in quarters $t-1$ and t for that employer. The separation rate is defined as the number of workers with positive earnings in quarter $t-1$ who were no longer employed by that employer in quarter t divided by the average number of workers in quarters $t-1$ and t. By definition, the rate of change in employment is equal to the accession rate minus the separation rate. Thus, we control for two measures out of the three in our empirical analysis.

[24]The average number of movers per firm is 19 in the full sample and 69 in the random subsample of job movers.

across a selected sample of firms in the second stage of our sampling procedure, and large firms have more movers and pay higher wages. We explore below whether the peer effect estimate varies by firm size.

Descriptive statistics of sample characteristics in Table 1.1 can also be used for a brief comparison of how movers used in our analysis differ from stayers. Individual quarterly earnings and individual age variables represent sample-based age and earnings characteristics of movers. The mean of contemporaneous earnings of coworkers variable is the earnings characteristic of the sample of stayers in the corresponding samples of firms the movers are moving both to and from. As can be seen from the results in Table 1.1, movers from both the full sample and the random subsample tend to be slightly lower paid than respectively defined stayers. The average age of stayers can be inferred from the age fractions of coworkers variables. Assuming the uniform distribution of workers in each age group, the average age of workers employed by 246,315 firms corresponding to the full sample of movers is about 37 years and the average age of workers employed by 10,961 firms corresponding to the random subsample of movers is about 39 years. In other words, movers in our samples are on average 1 to 2 years younger than respectively defined stayers. Overall, we believe that stayers and movers in our samples are tolerably comparable along age and earnings dimensions for the purpose of our analysis. Finally, the LEHD has information on two more individual level characteristics, sex and race. We do not extend our analysis along the dimensions of sex and race in the current project.

1.6 Results

Table 1.2 provides the first set of estimates of the coefficients of interest.[25] We estimate the association between quarterly earnings and quarterly average earnings of coworkers as in equations (1.3) and (1.4) excluding firm fixed effects, using both the full sample of movers and

[25] All specifications included in the paper contain the following set of additional controls, for some of which coefficient estimates are not shown: the vector of individual characteristics, X_{it}, includes age and age squared; the vector of predetermined firm level characteristics, Z_{jt}, includes firm size, demographic characteristics of the firm's workforce such as fractions of workers by age-sex-race, workforce turnover characteristics, and a multi-plant indicator variable. Each specification contains a full set of calendar quarter fixed effects (60 quarter dummies, for 1990:1 to 2004:4), which control for quarter specific unobserved shocks affecting workers at all firms. We also estimated all specifications using log earnings. The results were qualitatively very similar.

the random subsample for the models with the mean of contemporaneous coworker earnings and the mean of permanent measures of coworker performance as alternative explanatory variables of interest.[26] With this exercise, we try to assess the degree to which compositional differences between the full sample and the random subsample of movers might affect our estimation results.

Specification (1) presents the estimates of peer effects in earnings with the mean of contemporaneous coworker earnings as a key explanatory variable.[27] The random subsample of movers used in the main empirical analysis yields smaller estimates of the peer effect on earnings than the full sample of movers. Individual earnings increase by about 7 cents in the random subsample compared to 17 cents in the full sample for every one dollar increase in average earnings of coworkers.[28]

The next five specifications in Table 1.2 present estimates of peer effects in earnings with different measures of the mean of the permanent component of coworker earnings performance for the two samples of movers. First, we construct the mean of the long-run coworker earnings using coworker quarterly earnings from *all employers* and *all quarters* for each coworker as follows:

$$\overline{Y}_{-i,jt}^{long-run} = \frac{1}{(N_{jt}-1)} \sum_{i' \neq i}^{N_{jt}} \left(\frac{1}{\sum_{j=1}^{J_{i'}} T_{i'j}} \sum_{t=1}^{T_{i'j}} \sum_{j=1}^{J_{i'}} Y_{i'jt} \right) \tag{1.5}$$

where $Y_{i'jt}$ is the quarterly earnings of individual i' during quarter t while employed by employer j, $J_{i'}$ is the total number of employers of individual i' observed in the employment history file for that individual, $T_{i'j}$ is the total number of quarters an individual i' has been

[26] We can easily perform this estimation on the full sample of movers because despite its size, it includes only 15 explanatory variables and 60 quarter dummies.

[27] Formally, the mean of contemporaneous coworker earnings is defined as $\overline{Y}_{-i,jt} = \frac{1}{(N_{jt}-1)} \sum_{i' \neq i}^{N_{jt}} Y_{i'jt}$, where $Y_{i'jt}$ is the quarterly earnings of individual i' during quarter t while employed by an employer j and N_{jt} is the total number of workers employed by firm j during quarter t.

[28] We have further investigated how compositional differences between our samples translate into different estimation results by constructing multiple random samples by gradually increasing the initial percentage of random draws of movers from the full sample. Smaller random subsamples give consistently smaller estimates of peer effects than the full sample of movers in the model with the mean of contemporaneous coworker earnings.

observed with positive earnings from employer j, N_{jt} is the total number of workers employed by firm j during quarter t. In other words, we calculate the average wage for each individual in the dataset from all employers this person is observed ever working for in the entire data set and then take an average across $(N_{jt}-1)$ coworkers of individual i in quarter t while individual i is employed by firm j.

We construct the second version of the mean of the long-run coworker earnings using coworker quarterly earnings from *all quarters with the current employer*, or employer j, only:

$$\overline{Y}_{-i,jt}^{long-run} = \frac{1}{(N_{jt}-1)} \sum_{i' \neq i}^{N_{jt}} \left(\frac{1}{T_{i'j}} \sum_{t=1}^{T_{i'j}} Y_{i'jt} \right) \tag{1.6}$$

Finally, we construct the mean of the long-run coworker earnings using coworker earnings from the *current employer only up to the current quarter* of analysis:

$$\overline{Y}_{-i,jt}^{long-run} = \frac{1}{(N_{jt}-1)} \sum_{i' \neq i}^{N_{jt}} \left(\frac{1}{T_{i'j}} \sum_{t=1}^{T_{i'j}=t} Y_{i'jt} \right) \tag{1.7}$$

We use different versions of this variable in specification tests. The long-run individual earnings averages are meant to capture the permanent component of individual ability and are supposedly more accurate the more information on individual earnings is used, which in our case corresponds to the first measure constructed as in (1.5). On the other hand, this measure does not take into account differences in payment practices across different firms, which is potentially a huge problem in our peer effects estimation because we want peer earnings to reflect peer productivity net of payment scheme differences across different employers. We address this by analyzing long-run averages of coworker earnings while employed by the current employer only. Permanent differences across firms in this case are taken into account once this measure is used in the regression analysis of models (1.3) and (1.4) with the full set of current employer firm fixed effects. The third measure is the noisiest measure of the permanent component of individual earnings performance because it is constructed with the shortest longitudinal component.

We also calculate individual fixed effects or the permanent component of coworker earnings from corresponding individual earnings regressions to adjust individual earnings for the impact of certain observed characteristics of an individual and a firm on individual quarterly earnings.[29] We use the mean of coworker fixed effects $\overline{\theta}_{-i,jt} = \frac{1}{(N_{jt}-1)} \sum_{i' \neq i}^{N_{jt}} \hat{\theta}_{i'}$ instead of the mean of long-run averages of coworker earnings as the main explanatory variable of interest while estimating our empirical model. We construct individual fixed effects, $\theta_{i'}$, as follows. First, we estimate the following specification on pooled earnings history data of all workers in the dataset

$$Y_{i'jt} = \varphi X_{i'jt} + \theta_{i'} + \epsilon_{i'jt} \tag{1.8}$$

and use the estimates of individual fixed effects for each worker, $\hat{\theta}_{i'}$, to construct the mean value of individual fixed effects of individual $i's$ coworkers at firm j in period t, $\overline{\theta}_{-i,jt}$.[30] Matrix X contains individual age and firm size characteristics. It would be ideal to include firm fixed effects in the estimation of (1.8) to control for unobserved permanent differences across firms, e.g., disentangle the difference between high and low paying firms, etc. Unfortunately, it is not computationally feasible in our case given the size of the available data. We construct two types of individual fixed effects: (i) analyzing coworker earnings history data from *all employers* observed in the data for every coworker, (ii) analyzing coworker earnings history data from the *current employer* only.

The estimates of peer effects with five different measures of the average long-run coworker earnings performance at work (Specifications (2) to (6) in Table 1.2) are virtually identical across the two samples of movers, suggesting that these measures of average coworker outcome are not affected by the compositional differences across our samples of movers. We analyze differences in social interactions by firm size later in the paper (see results in Table 1.5).

[29] We control for individual's age to account for the fact that individual earnings might be increasing with age irrespective of a change in individual productivity. We also control for firm size to account for the fact that larger firms are generally paying higher wages to their employees.

[30] More specifically, we estimate (1.8) in differences in means form and then back out individual fixed effects according to $\hat{\theta}_{i'} = \frac{1}{\sum_{j=1}^{J_{i'}} T_{i'j}} (Y_{i'jt} - \hat{\varphi} X_{i'jt})$.

The estimates in Table 1.2 are biased because firm fixed effects are omitted. Table 1.3 presents estimates of peer effects in earnings for the same set of different measures of average coworker outcome as in Table 1.2 with 10,961 firm fixed effects added to the estimation. Note that we are able to perform these estimations on a random subsample of job movers *only* due to computational constraints discussed previously.[31]

The results in Specification (1) of Table 1.3 show that when unobserved time invariant differences across firms are controlled for, the estimated peer effect is about 8 cents per dollar of coworker earnings, compared to 7 cents when firm fixed effects are omitted in the model with contemporaneous coworker outcome. Thus, the bias from omitting firm fixed effects is small in this random subsample. Controlling for firm fixed effects is much more crucial in models with long-run measures of coworker performance. An additional dollar increase in the long-run average earnings of coworkers translates to an additional 13-14 cents increase in individual earnings (Specifications (2) and (4) in Table 1.3). The point estimates of peer effects in earnings are virtually identical when the average coworker individual fixed effects are used instead (Specifications (3) and (5)). These estimates are remarkably similar to those in experimental (Falk and Ichino, 2006) and some case studies (Ichino and Maggi, 2000; Mas and Moretti, 2008), which report estimates of 14 to 18%.

Note that our peer effects estimates get larger when firm fixed effects are controlled for across all specifications in Table 1.3 compared to those in Table 1.2.[32] Larger peer effects estimate in the specification with firm fixed effects technically means that firm fixed effects are negatively correlated with average coworker earnings variable. We explain it as follows. Firm fixed effects in our application help to differentiate between firms with compensation

[31] All specifications in Table 1.3 also contain the estimates of the full set of additional control variables.

[32] Ichino and Maggi (2000) implement identical empirical strategy to estimate peer effects in shirking on a sample of movers across different branches of one firm. Contrary to our results in Tables 1.2 and **??**, the peer effect estimate in shirking reported by these authors is larger in specification without branch fixed effects, and lower in specification with branch fixed effects. Failure to control for time-invariant differences in shirking across firm branches introduces an upward bias to the peer effects estimate in shirking. The story behind it might be something like this branch has cubicles rather than individual offices; therefore everybody at this branch shirks less because they can for example see each other (our own example). Once the branch fixed effects are introduced, the peer effects in shirking capture the effect due to the composition of workers at the branch rather than office structure.

schemes not related to performance from those with performance-based pay. Ideally, we should analyze peer effects in earnings only on a sample of individuals at firms with pay related to performance. If individual earnings do not depend on individual or group performance, then the estimate of the behavioral interaction effect using earnings as an outcome measure will be zero because earnings are not going to be the appropriate measure of work performance or behavior. Unfortunately, we cannot distinguish between firms with performance-based pay and pay not related to performance in the LEHD and mixing data on workers at firms with fixed pay into our sample introduces a measurement error of individual and group behavior leading to downward bias in the peer effects estimate. Firm fixed effects capture the effect of all permanent differences across firms including permanent differences between firms with different compensation schemes. In this situation introduction of firm fixed effects to the estimation leads to an increase in peer effects estimates because they help to disentangle the downward effect of the data from firms with fixed pay.

Finally, the estimate of peer effects in earnings for the last measure of long-run earnings of coworkers in Specification (6) of Table 1.3 is 0.48 and is noticeably larger than the estimates in Specifications (2) - (5). The main explanatory variable in this case is defined as the long-run average coworker earnings from the current employer up to the current quarter or the quarter when the mover under analysis leaves or joins the firm. This average coworker outcome is going to be close in nature to the average contemporaneous coworker outcome in case of a short longitudinal component of coworker earnings, e.g., if the firm is young or if the firm has high workforce turnover. It is going to be much closer to the long-run measures of the average coworker outcomes in Specifications (2) through (5) in older firms with relatively stable workforces. This fact does not necessarily mean that the estimate of Specification (6) has to be in between the estimates of Specification (1) and Specifications (2) to (5) because we know that the Specification (1) estimate is biased with the direction of bias being *a priori* unknown. Another feature, which makes this measure different from other measures of the long-run average group outcomes, is that it does not include information on quarters beyond the current quarter of analysis. In other words, this measure is not contaminated by future changes in individual productivity of each co-worker, which might be irrelevant to peer interactions in the current period.[33] This fact might be the reason behind larger peer effects estimate in Specification (6) than in Specifications (2) to (5). On the other hand, this measure might be quite a noisy measure of coworker long-run average performance in case of a short tenure of each coworker with the current employer and will be subject to the same set of biases as a contemporaneous average outcome. We do not think we should reject the estimate in Specification (6), rather we believe we should treat it as an upper bound on the estimate of peer effects in earnings. This result suggests that it might be interesting to investigate how peer effects in earnings vary by coworker tenure with the firm and with each other in the future research.

As noted before, there are several potential sources of bias remaining in Specification (1) of Table 1.3. The first is due to time varying unobservables affecting all workers at a given firm in the same quarter. If earnings of all workers at the firm change because of a time varying demand or technology shock that has nothing to do with worker interactions, then the estimate of β will be biased upwards. In other words, the estimates will suggest the existence of social interactions at the workplace even if there are none.[34] Second, the average peer outcome is endogenous if there are peer effects from the mover to the stayers due to simultaneity of behavior. Finally, the use of the sample of movers does not eliminate the measurement error bias in the average coworker outcome; it only helps to reduce the exacerbation of the measurement problem bias while estimating our empirical model in first differences. In the case of classical measurement error, the bias is toward zero. Overall, the exact direction of these three different sources of bias in the peer effects estimate in a model with contemporaneous peer outcome is a priori unclear.[35]

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[33] For example, performance at work of each coworker can change in the future because of additional individual training, change in technology employed by the firm, or even the same peer effects from the new subset of coworkers.

[34] As noted above, we include a full set of calendar quarter fixed effects. This controls for quarter-specific shocks which are *common* to all firms, but does not take care of *firm-quarter-specific* demand shocks or technology changes. In principle, we could include *year-firm* interaction dummies to address the issue of year-firm specific shocks because the variation in the firm's average earnings is at the quarterly level. Unfortunately, this implies the inclusion of 14 (years) x 10,961 (firms) additional controls, which is not computationally feasible.

[35] See Ichino and Maggi (2000) for a similar discussion.

The most direct approach to address these biases is IV estimation. The results of our experiments with different instruments for contemporaneous average earnings performance of coworkers are presented in Table 1.4. The means of long-run measures of coworker performance are the logical candidates for instruments because it is reasonable to expect that more productive workers (e.g., those with high permanent components of long-run earnings performance) will tend to have higher contemporaneous performance. The IV-FE estimates with each of the five alternative measures of the long-run average co-worker outcome are presented in Specifications (1) through (5) in Table 1.4. The IV-FE estimates in these specifications are significantly larger than the fixed effects estimate in a model with contemporaneous average peer outcome, suggesting that the fixed effects estimate of 0.08 is actually biased downwards in our application and can be viewed as the most conservative estimate of peer effects in earnings. Note that the IV-FE estimates with the first two measures of the permanent component of co-worker average outcome based on individual earnings data from all employers are almost three times larger than IV-FE estimates with the next two measures based on individual earnings data from the current employer only. We believe that this result could be due to the fact that the first two measures do not account for permanent differences across firms and this fact is picked up with the IV-FE estimates to much bigger extent than with simple FE estimates in Table 1.3. Also note that the IV-FE estimate of β with the fifth measure of the long-run average earnings of coworkers as an instrument is 0.22 and is much closer to the FE estimates in Table 1.3 than when this measure is used as the explanatory variable instead.

We also re-estimated our model with one-quarter lagged values of exogenous firm characteristics, Z_{jt-1}, and several lagged values of contemporaneous peer average earnings, $\overline{Y}_{-i,jt-1}$, $\overline{Y}_{-i,jt-2}$, $\overline{Y}_{-i,jt-3}$, $\overline{Y}_{-i,jt-4}$, as instruments for contemporaneous peer outcome (see Specifications (6) through (10) in Table 1.4) to replicate an IV approach used in Ichino and Maggi (2000), Lin (2005), and Lee (2007). The point estimate of the peer effect instrumented with lagged exogenous characteristics of the firm is quite similar to the fixed effects estimates from Table 1.3, but is very imprecise suggesting that these are weak instruments in our empirical application. The estimates of β with lagged values of peer-average earnings are much more precisely estimated, suggesting that the lagged group outcomes are much more powerful instruments (Specifications (7) - (10)). However, the point estimates of β are very unstable and range from -0.5 to 0.07. This can be explained by the fact that the validity of these instruments is much more questionable. Ideally, we would like to capture only the behavioral component of co-worker interactions with the use of these instruments. However, if there are serially correlated time-varying firm-specific shocks, which affect the behavior of all workers at the firm and which persist over time, the lagged values of group behavior are not valid instruments because they will be correlated with contemporaneous individual earnings through shocks correlated across time periods. Finally, the use of lagged average earnings as an instrument of contemporaneous earnings is theoretically incorrect in firms with high workforce turnover. In other words, lagged average earnings are an entirely false measure of average group behavior of the *relevant* peer group if the firm completely changes its workforce composition from one period to the next. The use of longer lags helps to address the problem of time-varying firm-specific shocks to the extent that the estimates will be biased only by shocks of longer persistence, but makes the question of the relevant peer group of much bigger concern. Recall that the time period in our analysis is a quarter and 2nd to 4th lags translate to 6 to 12 month intervals in our application. Our exercise with lagged average coworker earnings suggests that despite the fact that it is theoretically justifiable, it is inferior to the analysis with long-run measures of the average peer outcome because long-run averages or individual fixed effects used as instruments do not suffer from these problems. To summarize, our preferred IV results are those in Specifications (3) to (5) in Table 1.4. They suggest that contemporaneous peer effects in earnings can be up to 50 cents per additional dollar earned by coworkers and are consistent in size with the largest estimate in Table 1.3 Specification (6).

Overall, based on the results presented in Tables 3 and 4 we conclude that on average peer effects in earnings are at least in a range of 8 to 14 cents for an additional dollar increase in the average coworker earnings performance. Again, since we do not know the exact mechanism that generates peer interactions as we are mixing heterogeneous individuals and workplaces in our analysis, a possible interpretation of our estimates is that they capture some combination of a true effect of permanent productivity and a true effect of contemporaneous coworker effort on individual performance.

Next, we investigate the possibility of non-linearity of peer effects in earnings (Specifications (1) and (2) in Table 1.5). Our results in a model with contemporaneous coworker behavior suggest that peer interactions have diminishing marginal returns, or that the marginal effect of an additional dollar increase in earnings of an average coworker on the earnings performance of individual i is smaller if the average coworker earns more. A one dollar increase in average earnings of coworkers from a base of \$5,000 per quarter causes a 19 cent increase in individual earnings while a one dollar increase in average earnings of coworkers from a base of \$15,000 per quarter causes a 16 cent increase in individual earnings.[36] This result can be explained as follows. More self motivated and productive workers, if reflected in larger earnings, might not be as active in information transmission, teaching on the job, coworker monitoring, or other forms of interaction to the same extent as low earners if they do not expect to gain as much from interacting with peers. This result is in line with recent results by Guryan, Kroft and Notowidigdo (2007), who explain the absence of peer effects among professional golfers by suggesting that workers in high-skilled labor markets might avoid social interactions when they face stronger individual financial incentives. We also estimated the same non-linear specification with one of the long-run measures of coworker performance as a key explanatory variable - the mean of the long-run averages of coworker earnings from the current employer only. The results in Specification (2) are qualitatively different from those in Specification (1) - the effect is increasing at larger levels of the permanent component of coworker productivity. A possible way of reconciling both results is that contemporaneous and long-run measures of average coworker behavior indeed capture different mechanisms behind peer interactions.

Finally, in order to determine how peer effects vary by firm size, we estimated our model with an interaction term between the same two measures of average coworker outcome and firm size. The results in Specification (3) with the mean of contemporaneous coworker outcome show evidence of stronger peer effects among coworkers in smaller firms: an additional dollar increase in earnings of average coworker earnings translates to 13.5 cent increase in individual

[36] The positive effect of peers disappears if an average coworker makes about \$85,000 per quarter.

earnings in a firm with 100 workers, 12.6 cent increase in individual earnings in a firm with 1,000 workers, and only 3.6 cent in a firm with 10,000 workers. This result could indicate that peer effects are more important in smaller workplaces, especially if the mechanism behind peer interactions is of social pressure nature or the ones when individuals experience utility loss when they are observed behaving non-cooperatively. Peer interactions of this nature are likely to be more effective in smaller groups.[37] The more plausible explanation is that there is greater misspecification or mismeasurement of the *relevant* peer group in larger firms while treating all coworkers as peers. In other words, larger firms may consist of many peer groups. While the degree of worker interactions with relevant peers in larger firms may be as strong as in smaller firms, we might be underestimating peer effects in larger firms as we add more noise to the measurement of average peer earnings by including outcomes of 'irrelevant' peers.[38] Introduction of an interaction term of the average peer outcome with the firm size does not make any difference if the mean of the long-run average earnings of coworkers is used. This result might mean that long-run measure of peer outcome might be better at capturing much broader aspects of peer interactions, e.g., knowledge spillovers at the firm level. The results in Specifications (3) and (4) are consistent with those in Table 1.2 showing much lower estimate of peer effects in a sample with larger firms (random subsample) in the model with contemporaneous average coworker outcome and the same estimate in both samples in a model with the long-run average coworker outcomes.

[37]Kandel and Lazear (1992) show that whether individual effort is increasing or decreasing in N formally depends on the shapes of the production function and the peer pressure function. As firm size increases, the direct incentive to supply effort falls. Larger groups may reduce the returns to peer pressure because an individual has to exercise such pressure over a larger group of coworkers. At the same time, larger groups contain more coworkers who can exert pressure.

[38]The size of the downward bias depends on the total sample size as well as the size of the 'relevant' peer group and the size of the misspecified peer group. We confirmed this result by a simulation exercise. We created an artificial sample of individual earnings of 100,000 individuals with true peer effects of 0.2 (or 20%) within a group of 5 individuals each only. The estimate of peer effects decreases as we misspecify the true peer group by adding more outcomes of irrelevant peers in addition to the outcomes of relevant peers to the calculation of average group earnings. The estimate of peer effects becomes significantly not different from zero as we increase the size of the mismeasured peer group up to 30 individuals (average earnings of 5 'relevant' and 25 'irrelevant' peers).

1.7 Conclusion

We find evidence of endogenous interactions at the workplace in a large representative sample of workers and firms. We find that individual earnings increase by at least 8 to 14 cents per dollar earned by coworkers. In our empirical approach, we control for worker and firm fixed effects, which capture long run individual and firm determinants of productivity. We interpret the effect of average coworker earnings net of these fixed effects as a measure of the effect of behavioral interactions at the workplace. Our estimates are quite similar to those in Mas and Moretti (2008), Falk and Ichino (2006), and Ichino and Maggi (2000), who estimate the size of interaction-generated externalities in the range of 14 to18%. The magnitude of our estimate is not trivial. The 8 cent estimate implies that the impact of a 25% increase in average coworker earnings performance on individual performance at work is equivalent to the impact of at least two additional years of work experience and at least one additional year of schooling. Our estimate is an average across a large number of heterogeneous workplaces, suggesting that behavioral interactions at the workplace might be a widespread phenomenon rather than a specific feature of workplaces in which interactions have been shown to be important. We show that peer effects might be stronger in smaller firms. We also show that peer interactions are non-linear in nature.

In this paper, we have assumed that the entire workforce of a firm is the relevant peer group for each individual in the firm. However, it is easy to think of reasons why an individual's work-related peer group might be a subset of the firm's workforce. For example, workers might be more responsive to peer pressure or other forms of interaction with peers of similar age, race, sex, productivity, etc. Finally, our results alongside with diverse empirical evidence on the importance of peer interactions at the workplace by other researchers suggest that detailed industry specific analysis of peer effects in earnings is the logical next step towards investigating the heterogeneity in social interactions across individuals and across labor markets with aggregate data similar to ours.

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Table 1.1. Means and Standard Deviations of Sample Characteristics

(All earnings variables are measured in 2006 dollars, standard deviations in parentheses)

	Full sample	Random subsample
Individual outcome, person-firm-quarter specific:		
Individual quarterly earnings, Y_{ijt}	10,484 (24,539)	12,187 (34,127)
Average group outcomes, person-firm-quarter specific:		
Mean of contemporaneous earnings of coworkers	11,073 (8,941)	12,818 (10,256)
Mean of long-run averages of coworker earnings (all employers for each coworker)	11,004 (7,327)	12,704 (8,747)
Mean of coworker fixed effects (based on earnings data from all employers for each coworker)	-4,044 (7,591)	-3,852 (8,789)
Mean of long-run averages of coworker earnings (earnings from current employer only for each coworker)	11,062 (7,827)	12,792 (9,038)
Mean of coworker fixed effects (based on earnings data from current employer only for each coworker)	-4,257 (8,281)	-4,422 (9,318)
Mean of long-run averages of coworker earnings (earnings from current employer up to the current quarter of analysis only for each coworker)	10,356 (6,432)	11,772 (6,928)
Firm specific average quarterly earnings, one quarter lag	11,097 (9,577)	12,943 (10,835)
Firm specific average quarterly earnings, two quarters lag	10,952 (10,432)	12,788 (11,151)
Firm specific average quarterly earnings, three quarters lag	10,636 (16,717)	12,494 (11,486)
Firm specific average quarterly earnings, four quarters lag	10,176 (9,755)	11,948 (11,250)
Individual characteristics, (X_{it}), person-quarter specific:		
Individual age	35.74 (10.87)	36.71 (10.92)
Predetermined firm characteristics (Z_{jt}), firm-quarter specific:		
Fraction of workers less than 30 years old in the firm's work force	0.31 (0.17)	0.28 (0.16)
Fraction of workers 30-39 years old in the firm's work force	0.27 (0.08)	0.28 (0.07)
Fraction of workers 40-49 years old in the firm's work force	0.23 (0.08)	0.25 (0.08)
Fraction of workers 50-59 years old in the firm's work force	0.13 (0.07)	0.15 (0.07)
Fraction of workers 60+ years old in the firm's work force	0.05 (0.04)	0.05 (0.03)

Table 1.1. Continued

	Full sample	Random subsample
Fraction of females in the firm's work force	0.49	0.51
	(0.23)	(0.20)
Fraction of whites in the firm's work force	0.69	0.66
	(0.21)	(0.17)
Fraction of blacks in the firm's work force	0.12	0.13
	(0.13)	(0.11)
Number of workers (firm size)	4,040	9,855
	(10,196)	(15,362)
Rate of change in employment per quarter	0.01	0.01
Accession rate per quarter	0.16	0.13
	(0.15)	(0.14)
Multi plant indicator	0.51	0.69
	(0.50)	(0.46)
N (person-quarters)	4,791,280	753,728
N (persons)	2,395,640	376,864
N (firms)	246,517	10,961

Table 1.2. Peer Effects in Earnings, Basic Specifications and No Firm Fixed Effects

(dependent variable is individual quarterly earnings, standard errors in parentheses)

Average coworker outcome as an explanatory variable	Full sample	Random subsample
(1) Contemporaneous earnings	0.170***	0.072***
	(0.002)	(0.006)
(2) Long-run average earnings, all employers	0.074***	0.059***
	(0.003)	(0.007)
(3) Individual fixed effects, all employers+	0.074***	0.060***
	(0.003)	(0.007)
(4) Long-run average earnings, current employer	0.072***	0.073***
	(0.003)	(0.007)
(5) Individual fixed effects, current employer+	0.071***	0.072***
	(0.003)	(0.007)
(6) Long-run average earnings, current employer up to current quarter	0.160***	0.175***
	(0.004)	(0.011)
Firm fixed effects	No	No
Other controls	Yes	Yes
N (person-quarters)	4,791,280	753,728
N (persons)	2,395,640	376,864
N (firms)	246,517	10,961

+ Standard errors in specifications with individual fixed effects in all Tables are not adjusted for the fact that individual fixed effects of coworkers, $\hat{\theta}_{i'}$, are the estimated parameters themselves. Given the size of the dataset at hand, the cost of correcting standard errors is too large given that the point estimates of the peer effects coefficients are virtually identical to corresponding specifications with long-run earnings averages.

Table 1.3. Peer Effects in Earnings, Basic Specifications with Firm Fixed Effects

(dependent variable is individual quarterly earnings, standard errors in parentheses)

	(1)	(2)	(3)	(4)	(5)	(6)
	Contemporaneous earnings	Average earnings, all employers	Individual fixed effects, all employers	Average earnings, current employer	Individual fixed effects, current employer	Average earnings, current employer up to current quarter
Average coworker outcome	**0.081*****	**0.139*****	**0.138*****	**0.131*****	**0.132*****	**0.480*****
	(0.009)	(0.019)	(0.019)	(0.019)	(0.019)	(0.032)
Firm fixed effects	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**
Individual characteristics:						
Age	**687.27*****	**687.02*****	**687.33*****	**688.62*****	**687.85*****	**684.34*****
	(155.53)	(155.54)	(155.54)	(155.54)	(155.54)	(155.50)
Age squared	**-8.08*****	**-8.07*****	**-8.07*****	**-8.09*****	**-8.08*****	**-8.05*****
	(1.96)	(1.96)	(1.96)	(1.96)	(1.96)	(1.96)
Predetermined firm characteristics:						
Fraction of workers 30-39 years old	404.50	1,246.56	1,517.50	1,165.09	1,467.20	-1,203.71
	(1,232.56)	(1,229.62)	(1,230.35)	(1,229.64)	(1,230.17)	(1,239.74)
Fraction of workers 40-49 years old	1,088.02	1,509.10	2,033.69	1,413.69	1,928.98	-402.11
	(1,260.88)	(1,259.90)	(1,261.47)	(1,260.09)	(1,260.89)	(1,266.41)
Fraction of workers 50-59 years old	-2,311.49	-2,688.95	-2,012.76	-2,609.10	-2,020.09	**-3,233.96***
	(1,835.23)	(1,837.64)	(1,834.97)	(1,837.28)	(1,834.98)	(1,836.44)
Fraction of workers 60-69 years old	1,049.72	1,162.57	2,022.40	1,205.43	2,108.20	1,500.23
	(2,619.54)	(2,619.65)	(2,622.49)	(2,619.69)	(2,623.32)	(2,619.14)
Fraction of females	**1,781.83***	**1,828.33***	**1,825.24***	**1,745.58***	**1,743.63***	**2,827.57****
	(989.75)	(990.15)	(990.13)	(989.85)	(989.84)	(992.78)
Fraction of whites	-591.19	-1,167.15	-1,174.51	-1,114.22	-1,163.85	-1,821.09
	(1,196.06)	(1,202.60)	(1,202.69)	(1,202.42)	(1,203.02)	(1,199.91)
Fraction of blacks	-784.94	-1,111.26	-1,088.04	-1,140.37	-1,111.25	-394.27
	(1,619.68)	(1,619.95)	(1,619.89)	(1,620.06)	(1,619.97)	(1,619.68)
Number of workers (firm size)	0.008	**0.017***	**0.017***	**0.016***	**0.018***	0.011
	(0.009)	(0.009)	(0.009)	(0.009)	(0.010)	(0.009)
Rate of change in employment	-60.88	-107.45	-108.38	-91.15	-97.40	1.23
	(371.37)	(371.38)	(371.39)	(371.38)	(371.38)	(371.33)
Accession rate	388.59	534.37	558.76	532.09	547.88	133.92
	(537.92)	(537.53)	(537.48)	(537.55)	(537.51)	(538.23)
Multi plant indicator	7.72	25.50	28.78	7.60	16.74	47.83
	(165.13)	(165.18)	(165.19)	(165.15)	(165.16)	(165.13)
Quarter fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
N (person-quarters)	753,728					
N (persons)	376,864					
N (firms)	10,961					

Table 1.4. Peer Effects in Earnings, Instrumental Variable Estimates

(dependent variable is individual quarterly earnings, *conditional* standard errors in parentheses)

Mean of contemporaneous coworker earnings instrumented by	Random subsample
(1) Long-run average earnings, all employers	1.513***
	(0.209)
(2) Individual fixed effects, all employers	1.660***
	(0.229)
(3) Long-run average earnings, current employer	0.516***
	(0.075)
(4) Individual fixed effects, current employer	0.546***
	(0.078)
(5) Long-run average earnings, current employer up to current quarter	0.223***
	(0.015)
(6) Exogenous firm characteristics, one quarter lag	0.178
	(0.158)
(7) Mean of contemporaneous coworker earnings, one quarter lag	0.020
	(0.025)
(8) Mean of contemporaneous coworker earnings, two quarter lag	-0.298***
	(0.045)
(9) Mean of contemporaneous coworker earnings, three quarter lag	-0.521***
	(0.099)
(10) Mean of contemporaneous coworker earnings, four quarter lag	0.072**
	(0.029)
Firm fixed effects	Yes
Other controls	Yes
N (person-quarters)	753,728
N (persons)	376,864
N (firms)	10,961

Table 1.5. Non Linearity of Peer Effects

(dependent variable is individual quarterly earnings, standard errors in parentheses)

Average coworker outcome as an explanatory variable	(1)	(2)	(3)	(4)
Contemporaneous earnings	0.198***		0.136***	
	(0.014)		(0.011)	
(Contemporaneous earnings /1,000) squared	-1.148***			
	(0.108)			
Long-run average earnings, current employer		-0.066		0.131***
		(0.043)		(0.019)
(Long-run average earnings, current employer/1,000) squared		2.554***		
		(0.495)		
Contemporaneous earnings * number of workers (in 1,000)			-0.010***	
			(0.000)	
Long-run average earnings, current employer * number of workers (in 1,000)				-0.0001
				(0.000)
Firm fixed effects	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes
N (person-quarters)	753,728			
N (persons)	376,864			
N (firms)	10,961			

Chapter 2

Labor Market Rigidities and the Employment Behavior of Older Workers

2.1 Introduction

The majority of workers retire by moving directly from full-time year-round employment on a long term job to non-employment. Gradual retirement, partial retirement, bridge jobs, and other less abrupt transitions to retirement are common (Gustman and Steinmeier, 1984, Ruhm, 1990, Blau, 1994, Maestas, 2004), but much less frequent than abrupt and complete exit from employment. Why is retirement typically so abrupt? If most individuals retire as a result of a health shock, then the prevalence of abrupt retirement would be understandable. Deterioration in health is in fact an important cause of retirement, but most changes in employment status at older ages are not associated with a decline in self-reported health (we document this below). In the absence of a health shock, it seems implausible that preferences for leisure would change abruptly at older ages. One indication that abrupt changes in preferences are unlikely to be a major cause of abrupt retirement is that self-employed workers, who have much more discretion over their hours of work than do wage-salary workers, are much more likely to retire gradually (also documented below).

An alternative explanation for the prevalence of abrupt retirement is labor market rigidity. The labor market has been asserted to be characterized by rigidities that make it difficult for older workers to carry out their desired trajectories from work to retirement. The rigidities that are often cited include lack of opportunity for part-time and flexible-hours work, low wages and lack of fringe benefits in the part-time employment opportunities that are available, and lack of training and promotion opportunities for older workers both at their career employers and at potential new employers (Hurd, 1996). Labor market rigidities may limit the employment options of workers of all ages, but older workers will be more affected by rigidities if they have a stronger desire for leisure or flexible work hours than younger workers.

As defined by Hurd (1996, p.12), "labor market rigidities are employment practices and work-related financial arrangements that constrain or limit the volume of work with respect to hours per day, days per week, or weeks per year" with the current employer or when changing employers. "Rigidities also include situations in which the volume of work can be varied, but the change requires a disproportionate sacrifice in compensation, job satisfaction, mental or physical requirements, or location". Many factors could be responsible for making the labor market rigid. For example, older workers face discontinuities in retirement incentives as a result of government policy and labor market institutions. Social Security and Medicare have strictly defined age eligibility criteria that affect employment incentives, particularly for workers who are liquidity-constrained (Rust and Phelan, 1997). The Social Security Earnings Test places a large implicit tax on earnings above a certain threshold prior to the normal retirement age. This has been found to affect employment behavior (Burtless and Moffitt, 1985, Friedberg, 2000). The Employee Retirement and Income Security Act (ERISA) prohibits workers from receiving benefits from a Defined Benefit (DB) pension plan while working at the firm that provides the benefits, before the normal retirement age in the pension plan. In addition, most DB plans link benefits to earnings in the last few years on the job, making it costly for a worker to decrease work hours at the career employer. Older workers who are covered by an employer-provided health insurance plan and have a health problem that requires medical attention may be reluctant to change employers (Scott, Berger, and Garen, 1995).

However, these factors alone cannot fully account for the prevalence of abrupt retirement, because as we document below abrupt retirement is the most common pattern even for individuals who don't appear to face liquidity constraints, are not covered by DB pension plans, and have retiree health insurance. This suggests that other sources of labor market rigidity may be important. On the supply side of the labor market, fixed costs of being employed may make part time employment unattractive to many individuals (Hamermesh and Donald, 2007). On the demand side of the labor market, some features of technology could induce firms to impose constraints on hours of work. If there are fixed costs of hiring, training, and employing a worker, then firms may impose a minimum hours constraint on their workers (Hamermesh, 1993). If production takes place in teams, then the absence of a team member could reduce team productivity. In this case firms might require the presence of workers at specific times, reducing the flexibility of workers in scheduling their hours of work. Other factors could result in reluctance of firms to hire older workers under the same terms as younger workers, but would not result in hours restrictions placed on older workers who age in place. For example, workers could face statistical discrimination in the labor market as a result of the application of group characteristics to all members of the group (Hellerstein, Neumark, and Troske, 1999). The short expected duration of future employment of an older worker reduces the incentive of a firm to train and promote older workers, despite the fact that some older workers may plan to remain employed for a long time (Hutchens, 1988).

Some of these sources of labor demand rigidities are caused by features of the technology of production that may affect all of a firm's workers, not just older workers. But if the preference for leisure increases with age, then the preferences of older workers differ systematically from those of younger workers. Thus the existence of technology-induced rigidities could be manifested in the age structure of a firm's work force: the more important are technology-induced rigidities, the lower is the share of older workers at a firm. There is evidence that production technology differs substantially across firms, even within narrowly defined industries (Doms, Dunne, and Troske, 1997). These differences are hypothesized to arise from variation across firms in managerial ability, expectations of future price and technological change, and past investment decisions (Davis and Haltiwanger, 1999). Thus, while it is difficult to measure technology directly, it may be possible to detect evidence of technology-based rigidities if such rigidities are manifested in differences in the age structure of the work force across firms.

It is important to study rigidities in the labor market and their impact on older workers, because workers who cannot carry out their optimal labor supply trajectory suffer a welfare loss. The economy loses the production and earnings of older workers who would like to work but cannot find a job with the desired hours and conditions and choose retirement instead. In addition, the government loses tax revenue, and the workers switch from contributors to claimants on Social Security. The approaching retirement of the baby boom generation and overall population aging amplify the importance of this issue. These demographic factors have raised concerns about whether labor supply will remain sufficient to meet employers' needs and whether Social Security and Medicare will remain solvent. Most research on employment of older workers adopts a labor supply framework of analysis. It is important to broaden the study of employment at older ages to consider the labor demand side as well.

In this paper, we develop a simple model of the labor market in which, in equilibrium, older workers are disproportionately concentrated in firms with flexible technology, allowing older workers to reduce hours of work gradually. Firms with more rigid technology require longer hours of work, and pay a wage premium to attract workers. Workers at such firms cannot gradually reduce hours of work as they age. In this model, a firm's age structure is an indicator of its technology: a relatively large share of older workers indicates flexible technology. Although technology itself is not observable, we develop a testable hypothesis from the model: the hazard of separation by an older worker is lower in firms with an older age structure than in firms with a younger age structure.

Empirically, we study the effect of both the firm-level and the industry-level age composition of employment on the separation propensity of older workers. We use data on workers from the Survey of Program Participation (SIPP) matched to data on their employers from the Longitudinal Employer-Household Dynamics (LEHD) files (Abowd, Haltiwanger, and Lane, 2004). We use a difference-in-difference approach to analysis, comparing the job exit behavior of older and younger workers in firms with different shares of older workers. Comparing older and younger workers makes it possible to determine whether labor market rigidities affect older workers disproportionately, as we hypothesize. In order to ensure that the firm's work force age composition is not merely picking up the effects of other factors, we control for the worker's demographic characteristics, pension and health insurance coverage, wage rate, wealth, health, industry, occupation, and location. We also control for other firm characteristics, including the number of workers, average earnings, and number of plants. The empirical results show that an older age structure of the work force, both at the firm and industry levels, is associated with a lower separation propensity of older workers. This finding supports our hypothesis, and is robust to many specification checks. We explore a number of alternative explanations for the finding, and find little evidence to support them.

The next section of the paper discusses evidence on labor market rigidities and the age structure of firms' work force. Section 2.3 describes the model. Description of the data and methodology are provided in Section 2.4. Section 2.5 presents the basic estimation results, and section 2.6 discusses alternative estimates and interpretations. Section 2.7 concludes.

2.2 Background and Literature

Here, we illustrate our claim that the majority of workers retire by moving directly from full-time employment to complete retirement. Table 2.1 shows employment transition rates computed from the Health and Retirement Study (HRS) for individuals aged 51-72 who were employed full-time year-round on a long-tenure job (at least five years) in any of the first five survey waves. The first row of the table shows that 17.7% of these individuals were not employed as of the next survey wave (two years later on average). In comparison, 4.0% were employed on a new year-round full-time job, 5.9% were employed part-time or part-year with the same employer, and 2.7% were employed part-time or part-year with a new employer. Thus of the total of 30.3% who changed employment status between survey waves, the majority (58.4%) made a complete exit from employment.[1]

As mentioned above, deterioration in health is clearly a major cause of retirement, but most changes in employment status at older ages are not associated with a decline in health. Table 2.1 shows wave-to-wave employment transition rates by the associated wave-to-wave change in self-reported health status. The exit rate from employment conditional on health declining from "good" to "bad" is twice as large as the exit rate conditional on remaining in

[1]Employment status is defined here by the five mutually exclusive and exhaustive categories shown in Table 2.1. The HRS is a biannual survey, but a detailed employment history is collected, so it is possible to compute transition rates for shorter intervals. Annual transition rates show a very similar pattern.

good health. However, comparing the sample sizes in the last column, it is clear that most exits from employment are not associated with a decline in self-reported health. Thus 67.7% of exits from employment were by individuals whose health remained good, compared to only 13.7% whose health declined from good to bad. Other measures of health show similar results (Blau and Gilleskie, 2001).

As noted above, self-employed individuals are much more likely to retire gradually than are otherwise similar wage-salary employees. Self-employment offers greater flexibility in hours to accommodate changing tastes for leisure, thus facilitating gradual retirement (Quinn, 1980). Karoly and Zissimopoulos (2004) report that workers age 45 and older represented 38% of the workforce in total, but made up 54% of the self-employed in 2002. Karoly and Zissimopoulos also find that while average hours worked per week was similar for self-employed workers and employees, 59% of the self-employed worked full time compared to 74% of wage and salary workers. The data in Table 2.1 show that the two-year transition rate from a full-time year-round long-tenure job to part-time employment (on the same job or a new job) was 7.3% for wage-salary workers and 16.5% for the self-employed. This clearly suggests that wage-salary workers face a constraint on hours of work imposed by their employers.[2]

Finally, as noted above, workers who are liquidity-constrained, are covered by DB pensions, and do not have Employer-Provided Retiree Health Insurance (EPRHI) are more likely to face incentives to avoid gradual reduction in hours of work. The last three rows in Table 2.1 show that workers who are covered by DB pensions, do not have EPRHI, and are in the lower half of the distribution of net worth are in fact more likely to retire abruptly (70.8% of all changes in employment status) than are workers who are not covered by a DB pension, do have EPRHI, and are in the upper half of the distribution of net worth (55.6%). Nevertheless, even among workers who, by these criteria, are relatively unlikely to face institutional or liquidity constraints on hours of work, the majority retire abruptly.

[2] In support of this interpretation, we find that the standard deviation of hours worked per week among the self-employed is 20, compared to only 13 among wage-salary workers. Workers with a strong preference for gradual retirement might be more likely to move to self-employment at older ages in order to indulge this preference. However, Karoly and Zissimopoulos (2004) report that only about one third of older self-employed workers entered self-employment after age 50.

Direct evidence on technological sources of labor market rigidity is scarce. When asked in surveys, many older workers who are employed full time state that they could not reduce the number of hours they work at their current employer (Hurd, 1996). Abraham and Houseman (2005) report that the fraction of older working Americans who plan to reduce their work hours or change the type of work around retirement age is almost equal to the fraction that plan to retire fully, but the former are only about half as likely as the latter to actually follow through on their plans.

Hutchens and Grace-Martin (2006) surveyed 950 establishments with at least 20 employees and two white collar employees aged 55+, and posed questions about phased retirement policy. 82% of the establishments report that they have a phased retirement policy. Most of the policies described by the establishments were informal and discretionary, and fewer than half of the establishments with a phased retirement policy reported that any older employees had actually shifted from full time to part time work in the three years prior to the survey. The survey did not inquire about any conditions that might be associated with phased retirement, such as wage cuts and pension eligibility.

In order to provide some additional evidence on flexibility in hours of work in relation to worker age, we examine data from the May 2001 Current Population Survey (CPS) Supplement on Work Schedules and Work at Home. We analyze responses to the question "Do you have flexible hours that allow you to vary or make changes in the time you begin and end work?" Figure 2.1 shows the age profile of responses to this question for workers aged 30-70. The proportion reporting flexible hours is roughly constant at around .35-.37 from age 30 to 58, and then increases sharply to over .50 by age 70. This age pattern persists after controlling for demographic characteristics of the worker, and detailed occupation, industry, and class of worker dummies in a regression equation (see Figure 2.1). This evidence clearly indicates that older workers are more likely to have flexible work schedules, but it does not directly address the issue of age structure and hours flexibility. To get at this issue, we computed measures of age structure at the three-digit industry level from the 1990 U.S. Census of Population (described in more detail below) and merged them with the CPS data. Controlling for all of the variables mentioned above (including 40 worker-age dummies and 51 two-digit industry dummies), we find that the fraction of workers aged 60-64 in the three-digit industry has a positive and statistically significant effect on the probability that a worker has flexible hours. The same finding holds for the fraction of workers aged 65-69 and 70-74. A one standard deviation increase in these fractions is associated with a 1-2 percentage point increase in the probability of flexible hours. This suggestive evidence directly supports our contention that age structure and flexible hours will be positively associated. We next discuss a model that generates this association.

2.3 Conceptual Framework

We illustrate the logic of our conceptualization of technological sources of labor market rigidity and their impact on the employment behavior of older workers using a two-sector general equilibrium model of the labor market.[3] The idea of the model is simple: there are two sectors of the labor market, differentiated by the technology employed. One sector has a "flexible" technology: firms care about the total number of labor hours employed, but are indifferent to the number of hours worked by individual workers. Workers in this sector can reduce hours of work as they age, if they so desire. The other sector has a "rigid" technology: firms in this sector care about hours of work per worker, and as a result they impose a minimum hours-of-work constraint. Workers employed in this sector cannot reduce their hours of work as they grow older unless they shift to an employer in the flexible sector or withdraw from the labor force. There is no direct cost of changing sectors, but in equilibrium firms in the rigid sector pay a higher wage than in the flexible sector in order to induce workers to work the number of hours demanded. Thus, leaving the rigid sector does entail an opportunity cost. There are many homogeneous firms in each sector, and the type of technology employed by a firm is fixed.

If the preference for leisure increases with age, some workers who preferred the high-wage

[3]See Hutchens and Grace-Martin (2006) for a related partial equilibrium model, based on fixed costs of employment to employers. Hamermesh and Donald (2007) propose a model of fixed time costs of employment to workers as an explanation for abrupt retirement. Both of these models may help explain abrupt retirement, but neither model has implications for the age structure of employment at the firm level. Hence our approach is complementary with these alternative models.

rigid sector while young will shift to the lower-wage flexible sector when they are older. Thus the flexible sector will have a higher share of older workers than the rigid sector. Workers who experience an increase in the preference for leisure can reduce hours of work without leaving their employer if they are in the flexible sector, but not if they are in the rigid sector. So the hazard rate of exit from a firm will be higher for firms with a younger age structure of employment.

A formal model is presented in the Appendix, using a specific parameterization. There, we show that an equilibrium in which the share of older workers is greater in the flexible sector than in the rigid sector exists.[4] We also show that the separation hazard of older workers is higher in the rigid sector than in the flexible sector as long as preferences for leisure increase rapidly enough with age. We cannot test the first prediction, because we do not have data on technology, so we focus in the empirical section on testing the second prediction: older workers in firms with a larger share of older workers have a lower hazard rate of exit from the firm.

An important question is whether there are other mechanisms that, even in the absence of rigid technology, would result in an association between the age structure of a firm's workforce and the exit rate of its workers. If so, this would limit our ability to draw inferences about labor market rigidity based on the empirical association between age structure and turnover propensity. In a one-sector version of our model in which there is no rigid-technology sector, there is no association between a firm's age structure and the exit propensity of its workers. However, this does not rule out the possibility that other types of models could yield such an association. For example, suppose the age profile of wages is steeper in some sectors than in others. If older workers are concentrated in sectors in which the relative wage rate of older workers is high, this could lead to a lower exit rate of older workers from firms with a higher share of older workers. This suggests that it is important to control for a worker's wage rate in a model of separation. We do this and we also control for average earnings at the worker's firm.

Alternatively, suppose workers prefer to work with co-workers of the same age group. Then

[4] Other types of equilibrium exist as well, depending on specific parameter values. The equilibrium described in the text is the one of interest for our purposes.

an older worker who, by chance or design, finds himself in a firm with a large share of older workers might be less inclined to separate from the firm than an older worker in a firm with a smaller share of older workers. This would yield an association between age structure and separation propensity that has nothing to do with technology-based rigidity.[5] To deal with this and other possible sources of association between age structure and separation propensity that are unrelated to technology, we use an alternative proxy for a firm's technological flexibility: the share of female workers under the age of 30 in the firm's workforce. Women under the age of 30 are in their prime childbearing years, and are much more likely to occupy part-time and flexible-hours jobs than are other workers.[6] If older workers in firms with a larger share of young female workers are less likely to separate from the firm than older workers in firms with a smaller share of young female workers, this would be hard to explain by mechanisms other than technology-based hours inflexibility.

2.4 Methods

2.4.1 Empirical Specification

Our empirical specification can be viewed as an approximation to the employment decision rule of a worker. Life cycle models of the employment behavior of older workers imply that the employment decision in a given period depends on health, demographic characteristics, the wage offer, net worth, potential Social Security and pension benefits, and health insurance coverage (Rust and Phelan, 1997; Blau and Gilleskie, 2006; Van der Klaauw and Wolpin, 2005).[7] We augment this list with a measure of the age composition of employment at the

[5]Leonard and Levine (2006) studied employee turnover in 800 workplaces owned and operated by a single firm. They focus on the effects of workplace diversity along the dimensions of age, race, and gender, and do not estimate the effect of the share of older workers on turnover. Their results indicate that a change in age diversity at a workplace had no effect on turnover.

[6]Tabulations from the March 2005 Current Population Survey show that 21% of working women under age 30 worked 20 or fewer hours per week, compared to 9% of other workers; and 18% of working women under age 30 worked 21-34 hours per week, compared to 11% of other workers.

[7]It is straightforward to include the wage rate, because the sample consists of workers. We cannot easily include pension and Social Security benefits, since these are observed only for individuals who begin to collect benefits during the sample period. Thus, we omit these benefits and re-interpret our specification as a

individual's firm. As noted above, taking the difference between the employment behavior of older and younger workers makes it possible to determine whether labor market rigidities disproportionately affect older workers. A simple illustration of our empirical specification is

$$Pr(S_{ijt} = 1 | S_{ijt-1} = 0) = F(X_{ijt}\beta + \alpha A_{it} + \gamma R_{ij} + \delta A_{it} R_{ij}) \tag{2.1}$$

where $S_{ijt} = 1$ if individual i employed at firm j at the beginning of period t separates from the firm during period t, and equals 0 otherwise; X is a vector of individual and firm characteristics; $A_{it} = 1$ if the individual is classified as an older worker in period t; and R_{ij} is the proportion of older workers in the work force of firm j. This is a hazard model of the risk of separation, and is estimated as a logit.

The coefficient of interest is δ: the difference between the effect of the proportion of older workers on the separation propensity of older and younger workers. The main effect of age on employment behavior is captured by α. The main effect of the age composition of the firm's work force γ captures any effects of workforce age composition on employment behavior that are independent of the worker's own age. For example, firms with relatively few older workers may tend to be younger, and firm age may affect the separation propensity of all workers at the firm. The interaction effect δ captures any differences in the effects of the firm's age composition on older workers relative to younger workers. Controlling for pension and health insurance coverage, occupation, industry, and the wage rate (all included in X), we interpret differential effects of a firm's workforce age composition on older versus younger workers as an indication that labor market rigidities affect the employment decisions of older workers differentially.

2.4.2 Data

We merge data on individuals from the U.S. Survey of Income and Program Participation (SIPP), 1990 - 2001 panels, with data on their employers from the Longitudinal Employer

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quasi-reduced form, with effects of anticipated future pension and Social Security benefits captured by their determinants, such as experience, job tenure, and earnings.

Household Dynamics (LEHD) files. The SIPP collects detailed information on employment, demographic characteristics, and receipt of income from public programs. Sample members are interviewed every four months for $2\frac{1}{2}$ to 4 years. Each interview wave records employment information separately for each of the four months since the previous interview, so a monthly record of employment, hours of work, earnings, industry, occupation, class of worker, and health insurance coverage for each job can be constructed. The SIPP topical modules, administered once or more per panel, record information on annual income, assets, health, retirement accounts, pension coverage, and employment history prior to the sampling period. The SIPP collects employment data for up to two jobs held during a given month. If an individual holds two jobs in a given month, we analyze behavior only on the main job, which we define to be the one with greater work hours per week. If hours per week are equal, we select the job which has been in progress longer. The unit of analysis is a person-month. We focus on workers aged 45-69. We exclude younger workers because their behavior is likely to be influenced by factors such as human capital investment, family formation, and so forth that are not relevant for older workers. Thus we compare the behavior of workers in the typical age range of retirement (late 50s to late 60s) to the behavior of mature workers who are not yet approaching typical retirement ages (45-mid 50s).

The LEHD Infrastructure File system is based on state Unemployment Insurance (UI) administrative files, with data available to us from 31 states covering about 80% of the U.S. work force for the years 1990 - 2004, although the period covered varies by state (Abowd, Haltiwanger, and Lane, 2004). Employers covered by UI file a quarterly report for each individual who received any covered earnings from the employer in the quarter. An employer in this context is a UI-tax-paying entity. If a firm owns several establishments in a given state, all of these establishments would constitute a single employer. If a firm owns establishments in several states, its establishments in one state are a different employer in the LEHD data than its establishments in another state. This reflects the fact that UI is administered and largely financed by states. Thus an employer in this context is in general neither a firm nor an establishment. The data include the number of establishments per employer. UI covers about 96% of private nonfarm wage-salary employment, with lower coverage of agricultural and government workers, and no coverage of the unincorporated self-employed. The UI records contain information on the quarterly earnings of each individual from each employer for which he has any covered earnings during the quarter, the individual's Social Security number, and an identification number for the employer. These data are merged by the Census Bureau with the Census Personal Characteristics File, which contains date and place of birth, sex, and a crude measure of race/ethnicity. About 96% of workers in the LEHD data files have this basic demographic data merged in; for the remaining 4% it is imputed, as described in LEHD Program (2002). The Social Security numbers are then replaced by a scrambled worker identification number, to protect confidentiality. Additional employer information such as industry, location, and ownership type is merged in from the Employer Characteristics Infrastructure Files. An extensive discussion of the construction and the content of these files is provided in Abowd *et al*. (2006).

The key to our empirical analysis is matching workers in the SIPP sample to their employer or employers in the LEHD data. The Census Bureau provided us with an extract of the LEHD data, containing data for all the workers surveyed in the 1990 - 2001 SIPP panels who appeared in any LEHD record. For a given SIPP sample member, the LEHD file contains a record for *every* available quarter for *every* employer that paid any UI-covered earnings to the worker from 1990 (or later, if the LEHD records for the state in which the individual was employed begin after 1990) through 2004. The LEHD record for a given employer in a given quarter contains a stable firm identifier, the employer characteristics described above, and earnings and basic demographic data on the SIPP worker and on *all other workers who were paid any UI-covered earnings by the employer in that quarter*. Thus we have a census of the entire workforce of a given employer in a given quarter, which allows us to construct measures of the age distribution of the firm's workforce.

We match SIPP and LEHD records as follows. If an individual reports in the SIPP that he held only one job during a given calendar quarter, and if there is only one employer record in the LEHD for the individual for that quarter, we match the employer record in the LEHD to the job in the SIPP for that quarter. If the LEHD records two different employers for an individual in a given calendar quarter, and the two employers have different industry codes, we match by industry to the industry code for the main job in the SIPP.[8] If the same industry codes are reported for the two LEHD employers, we check whether either job was matched to an LEHD employer in an earlier quarter. If so, this identifies the job-employer correspondence in the current quarter as well, since the employer identifier does not change over time.

Table 2.2 presents summary statistics for two samples used in our analysis. The larger sample described in the first column contains SIPP individuals aged 45-69 who were employed at the beginning of a given month and who resided or reported working in one of the LEHD-covered states. The smaller sample described in the second column consists of those observations from the first column that were actually matched to an LEHD firm. The percentage of all SIPP person-months in our sample that is matched to an LEHD record is 52%. Failure to match occurs for several reasons. First, the LEHD file system is based on UI records and thus contains data only for workers who were employed in the UI-covered sector as wage-salary employees. Second, only about 80% of the SIPP sample members have a Social Security number available. The Social Security number is the basis for the confidential worker identifier that makes a link to the LEHD possible. Third, many states joined the LEHD program after 1990, so there are no data for such states for the early part of the SIPP sample. Finally, for person-months in which an individual held two jobs in the same industry, and neither job was matched to an LEHD employer in an earlier quarter, a match is not possible.

As can be seen from Table 2.2, the two samples are very similar in terms of sample means and standard deviations. The variable separated this month is an indicator for whether the individual left his or her job in the calendar month. This is the main dependent variable in our analysis. The mean separation rate is about 20% smaller in the matched sample. This is likely due to the fact that it is more difficult to match short and unstable jobs, for all of the reasons discussed in the previous paragraph. Figure 2.2 depicts the monthly separation rate by single year of age for the samples of potential and actual matches. The separation rate increases noticeably beginning around age 57, and there are large spikes at ages 62 and 65, as

[8]The SIPP provides three digit 1990 Census industry codes, while the LEHD provides six digit codes based on the 1997 North American Industry Classification System (NAICS). A crosswalk available from the Census Bureau web page http://www.census.gov/hhes/www/ioindex/indcswk2k.pdf was used for matching.

expected given typical retirement patterns in the U.S.

The key explanatory variable in our analysis is the employer-specific fraction of workers aged 65-69. This is our preferred proxy for the flexibility of the technology used by the employer. We experimented with proxies based on other age ranges (60-64, 60-69) and found that the results were generally similar but less precise than with our preferred measure. We use the employer-specific fraction of older workers averaged across all observed quarters for a given employer. This provides a relatively stable measure of age structure that is not subject to transitory quarter-to-quarter variation. We also control for the industry-specific age distribution of employment. We compute the industry-specific fraction of older workers using the 1990 Census Microdata file, rather than the SIPP data, in order to obtain large enough samples for each three-digit industry. We merge the industry-specific age composition to the estimation sample based on a worker's self-reported three-digit industry in the SIPP. The mean employer-level fraction of workers aged 65-69 in the matched sample is 0.018, and the mean of the industry-level fraction is also 0.018. The standard deviation of the employer-level fraction is more than three times larger than the standard deviation of the industry-level fraction. This suggests that much of the variation in the employer-level age composition is within industry. We verified this by using the LEHD data to regress the fraction of workers aged 65-69 in a firm on a full set of four-digit Standard Industrial Classification (SIC) industry dummies, a set of 10 firm size dummies, and the other firm characteristics available in the LEHD. The R^2 for this regression is 0.074, indicating that most of the variation in the employer-level age structure is within industry.

2.5 Results

To illustrate the basic patterns of interest, we first estimated a logit hazard model of separation using a set of single-year age dummies, the fraction of 65-69 year old workers (abbreviated as share65-69 henceforth) at the individual's employer, and interactions of these variables, with no other control variables. Figure 2.3 depicts the pattern of the predicted monthly separation hazard rate for two different values of the share65-69: half a standard deviation below the sample mean (0.003) and half a standard deviation above the mean (0.033). The separation rate is predicted for each individual and then averaged at each age. The results in Figure 2.3 suggest that the separation propensity of workers is lower at ages 62-65 when the share65-69 is higher. Differences at younger ages are small. This is the pattern predicted by our model.

Next, we added the following set of control variables to the model: gender, race, marital status, education, family income other than the worker's earnings, wealth, self-reported health and disability status, the hourly wage rate, two-digit industry dummies, occupation dummies, class of worker, job tenure and work experience, pension plan characteristics, health insurance coverage, size of the employer (number of workers), the demographic characteristics and earnings distribution of the employer's workforce, accession rate, ownership type, a multi-plant indicator, the employer's age[9], region and time. This specification also controls for the industry-level share65-69 and its interactions with single-year age dummies, in addition to the employer-level share65-69 and age interactions. Figure 2.4 presents the average predicted separation propensity by age based on this specification, for the same two values of the employer-level age composition variable as in Figure 2.3. In this specification, a lower separation rate at ages 62-65 in employers with a larger share65-69 remains noticeable even after controlling for many other factors that are likely to influence employment behavior. These results suggest an association between the share of older workers in a firm and the separation propensity of older workers.

Table 2.3 provides estimates of the main coefficients of interest in a more parsimonious specification, in which dummies for five year age groups are used instead of single year age dummies (the omitted age category is 45-49). First, we estimate the model with the industry-level share65-69 only (specification 1). Since technology differs across industries, we might expect to find that industry-level differences in the age composition of employment are associated with

[9]Firm age is equal to the number of quarters an employer is observed in the LEHD. Firm age is left censored if an employer appears in the first quarter of the LEHD coverage period. A dummy variable indicating whether the firm's age is left censored is included in the model.

differences in employment behavior of older versus younger workers. This specification contains all of the worker characteristics described above, but omits all employer characteristics, so it can be estimated on sample 1 from Table 2.2: the sample of SIPP person-months that could be potentially matched to the available LEHD extract. The coefficient estimates on the interaction between dummies for workers aged 55-59, 60-64, and 65-69 (the most common age ranges of retirement) and the industry fraction aged 65-69 are negative, significantly different from zero, and much larger than the interactions for the younger age groups. This is exactly the pattern we hypothesized, although it is for the industry-specific age composition rather than the employer-level age composition.

Next, we estimate exactly the same specification using sample 2: observations that were matched to the LEHD. Comparing specifications 1 and 2 allows us to determine whether the effect of industry-level age-structure is sensitive to sample composition. The main results from column 1 are unaffected by the change in the estimation sample.[10] Specification 3 adds all of the employer characteristics described above other than the age distribution (each averaged over all available quarterly observations for a given employer). Comparing specifications 2 and 3 allows us to investigate whether employer characteristics other than the age distribution affect the impact of the industry-level age distribution. As can be seen, the results in columns 2 and 3 are very similar.

Specification 4 replaces the industry-specific share65-69 and its interactions with their employer-level counterparts. The estimated effects of the employer-level age composition and age interactions are smaller than those of the industry-level age composition. This is partly a result of the larger standard deviation of the firm-level share65-69 (0.030) compared to the industry-level share (0.009), documented in Table 2.2. In order to provide a useful metric for comparing the effects of the industry and firm age65-69 shares, consider the impact of a one standard deviation increase in each. In specification 3, a one standard deviation increase in the industry-specific share65-69 is predicted to reduce the log odds of separation of a worker aged 60-64 by 0.13 (0.221 * 0.009 - 13.097 * 0.009). In specification 4, the corresponding increase

[10]The coefficient estimate on the age50-54 interaction becomes positive and significantly different from zero. This is not predicted by our theory, but is not inconsistent with the theory.

in the firm-level share65-69 is predicted to reduce the log odds of separation by 0.08 (1.402 * 0.030 - 3.998 * 0.030). Thus, the impact of the employer-level measure is smaller than the impact of the industry-level measure when they are compared appropriately.[11] The estimates in column 4 are not precise enough to distinguish between the effects of the share65-69 on each of the fiveyear age groups; only the difference between ages 45-49 (the reference category) and 60-64 is significantly different from zero. Nevertheless, the pattern of the interaction coefficient estimates is consistent with our prediction: the negative effect of the share65-69 is larger at older ages.

Next, we present estimates from a specification that includes both the employer and industry share65-69 and their interactions with age group dummies (specification 5). The coefficient estimates for the full set of control variables for this specification are provided in Table 2.7.[12] The main finding here is that the effects of the employer-level share65-69 are very similar in specifications 4 and 5. A one standard deviation increase in the employer-specific share65-69 is predicted to reduce the log odds of separation of a worker aged 60-64 by 0.06 (1.362 * 0.030 - 3.311 * 0.030). Controlling for the industry-level share65-69 hardly matters and the effects of the industry-level share65-69 are very similar in specifications 3 and 5. Figure 2.5 depicts the predicted monthly separation rate by age for the same pair of fraction values used in the previous simulations, based on our estimates from this specification. Comparing Figures 4 and 5 shows that the more parsimonious specification of age effects does not substantially distort the age pattern. Figure 2.5 shows that workers aged 60-64 have a lower propensity to separate from employers with a greater share65-69, relative to their younger counterparts. The coefficient estimate on the interaction at ages 60-64 is significantly different from zero, although it is not significantly different from the interactions for the other age groups.

[11] The industry age structure will pick up the effects of any variables that vary within two-digit industry and are correlated with the age structure. This may explain why the industry age structure seems to have a large impact than the employer age structure.

[12] We do not discuss the results for other variables in depth, because they are not the main focus of the analysis. There are no surprises in these results: work-limiting disability has a large positive effect on separation, employer-provided health insurance has a large negative effect, and so forth.

Finally, we re-estimated the model controlling for three-digit industry fixed effects (specification 6) instead of the two-digit industry fixed effects used in previous specifications. The industry fixed effects control for all industry-level factors that could be associated with the separation propensity, including observed factors such as the industry-specific age structure used in specification 5, and other unobserved factors. As can be seen, the effects of the employer-level age composition are quite robust.

2.6 Alternative Outcomes and Specifications

The longitudinal structure of the SIPP data allows us to examine the destination of job separations. We define a separation as leading to a change of employers if the respondent starts a new job within 30 days after separating from the previous employer. The residual category of no change of employer pools separations resulting in unemployment, withdrawal from the labor force, and separations of undetermined destination. Slightly more than 20% of monthly job separations are followed by a change of employers within 30 days of the separation (see Table 2.2). The first two columns in Table 2.4 present selected estimates from a multinomial logit model in which the outcomes are (1) separate and change employers, (2) separate without starting a new job within 30 days, and (3) no separation (the reference category). The results are striking: a larger share65-69 reduces separations by older workers to non-employment, but has no impact on employer-to-employer separations. Many of the separations to non-employment are retirements, suggesting that a more flexible technology allows workers to retire gradually on the job rather than switching employers.

Another way to disaggregate separations is by the proximate cause: employer-initiated (laid off, fired, plant closed) versus worker-initiated (quit, retired). The second pair of columns in Table 2.4 contains estimates of a multinomial logit model in which the outcomes are (1) employer-initiated separation, (2) worker-initiated separation, and (3) no separation. A larger share65-69 at both the firm and industry levels reduces the likelihood of an older-worker-initiated separation, but the industry-level effect is much larger than the firm-level effect, and the latter is not significantly different from zero. Our model predicts a positive association between an older age structure and worker-initiated separations, so these findings support our hypothesis. The results also show some association between age structure and employer-initiated separations, although the pattern of effects is not easy to characterize. Our model has nothing to say about employer-initiated separations.

A potential problem with using the fraction of older workers as a proxy for technology flexibility is that the age structure of an employer's work force may be determined in part by the age structure of separations from the employer. This could result in spurious correlation between the age structure of employment and the separation propensity of older workers. We avoid transitory sources of spurious correlation by using the average age structure of the employer rather than age structure in the current quarter. Nevertheless, if there are persistent unobserved differences across employers, the long run average age structure of an employer's workforce could be influenced by worker turnover. As an alternative specification we use the fraction of female workers less than 30 years old in the employer's workforce as a proxy for technology flexibility. Firms with flexible technology will attract any type of worker who might value flexible hours of work and part-time schedules, in particular females of childbearing age.[13] Thus, we expect older workers to have a lower propensity to separate from firms with a larger share of young female workers. Table 2.5 reports estimates of the coefficients of interest corresponding to specifications 4, 5 and 6 from Table 2.3. The coefficient estimates on the interaction terms at ages 55-59, 60-64, and 65-69 are negative and most are significantly different from zero. Thus older workers have a systematically lower probability of separating from firms with a larger share of young female workers, compared to older workers at firms with a smaller share of young female workers. This suggests that our results using the fraction of older workers as a proxy for flexible technology are not driven by spurious correlation.[14]

Finally, Table 2.6 presents estimates of our main specification separately for males and

[13] The mean age of childbearing was 27.4 years in the U.S. in 2000.

[14] We also estimated a specification that included both the share65-69 and the share of younger female workers (including both the industry and employer-level measures of each share). The effect of the share65-69 was very similar in this specification to the effect shown in Table 2.3 (column 5), and the effect of the fraction of young women was very similar in this specification to the effect shown in Table 2.5. This suggests that the two measures may capture different dimensions of an employer's flexibility.

females, separately for the periods 1990 - 1995 and 1996 - 2003 (more precisely, the 1990-1993 versus 1996-2001 SIPP panels), and using the quarter-specific employer-level age structure instead of the average age structure. The age structure of an employer's workforce has a much stronger effect on the separation propensity of older men compared to older women. Women may have a much stronger demand than men for flexible hours during the childbearing years, but it seems that men have a stronger preference for flexibility at older ages. The pattern of coefficient estimates is similar across periods, but the age structure interaction effects are substantially larger in the more recent period. Finally, the age structure interaction effects using the time-varying measure of age structure are much smaller than the corresponding estimates using the employer-average age distribution. This is not surprising given the greater volatility in the quarter-specific age structure of employment.[15]

2.7 Conclusion

This study analyzes the association between the age structure of employment in a firm and the propensity of older workers to separate from the firm. The empirical results show a lower separation propensity of older workers, relative to their younger counterparts, in firms with a larger share of older workers. This evidence is consistent with the hypothesis that technology-driven labor market rigidities are manifested in the age structure of employment, and are an important determinant of employment decisions of older workers. Although we have no direct measure of technology-induced labor market rigidities, we argue that the share of older workers at a firm is a useful proxy for the flexibility of technology at the firm. We control for a rich set of worker and firm characteristics that affect separation decisions and that could be correlated with a firm's age structure. This reduces the likelihood that our results are driven by some alternative source of correlation between age structure and turnover behavior. We also show that the relationship holds when an alternative proxy for technological flexibility is used: the share of young women in a firm's work force. Nevertheless, given the absence of a direct measure of technology, the results presented here are best viewed as suggestive of

[15] About one quarter of the total variance in the quarter-specific share65-69 is transitory.

the possible importance of labor market rigidities affecting older workers, but clearly not as definitive evidence.

Labor market rigidity is one of several complementary explanations proposed for the prevalence of abrupt retirement. Our results, and evidence presented by Hurd (1996) and Hutchens and Grace-Martin (2006), suggest that labor market rigidity is a plausible explanation. Hamermesh and Donald (2007) present evidence that fixed time costs of employment faced by workers is another plausible explanation. Rust and Phelan (1997) and others have shown that Social Security and Medicare policy provide strong incentives for abrupt retirement by liquidity-constrained workers. The U.S. population will be aging rapidly in the next two decades, and it is generally believed that increasing the employment rate of older individuals will be a necessary part of the adjustment to this major demographic change. Consequently, it is important to explore all of the possible impediments to increased employment at older ages, including labor market rigidity.

To conclude, some additional limitations of our study are worth mentioning. The approach used here imposes relatively little structure on the data, but the estimates do not provide an easily interpretable measure of the magnitude of the impact of technology-driven labor market rigidities on older workers. We reported above that a one standard deviation increase in the share65-69 would result in a 6% to 8% decline in the log odds of job separation of workers aged 60-64. There is no obvious way to interpret the magnitude of this effect in terms of its implications for economic well being. This estimate also doesn't allow us to distinguish between specific sources of demand-side labor market rigidities, such as team production versus fixed costs of employment. Finally, an important point made by Hurd (1996) is that we do not observe the wage and compensation that workers would have had if they had done something different from what they were observed doing. For example, what would the worker have earned if he had reduced his hours of work on the same job instead of remaining at full time hours, or if he remained full time rather than retiring? Firm-level data by themselves do not overcome this selection bias. Hence, an important area for future research is to estimate structural models that help to address the problems described above, at the cost of additional assumptions. The quantitative analysis of specific sources of labor market rigidities and their effects on employment behavior could be of considerable value in evaluating different types of policy interventions aimed at increasing labor force participation at older ages.

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Table 2.1. Employment Transition Rates by Selected Worker Characteristics

	Not employed	FT-FY same job	FT-FY new job	PT or PY, same job	PT or PY, new job	Sample size
All	17.7	69.6	4.0	5.9	2.7	14,489
Health in wave *t*, wave *t+1*						
good, good	15.1	72.0	4.2	5.8	2.9	11,503
good, bad	30.1	58.2	2.7	7.3	1.6	1,168
bad, good	22.4	66.2	4.2	5.1	2.0	731
bad, bad	29.1	59.3	2.7	6.6	2.3	1,080
Class of worker						
Wage-salary	18.7	70.5	3.5	4.6	2.7	12,358
Self-employed	12.2	64.6	6.6	13.5	3.0	2,130
Pension, Retiree Health Insurance, and Wealth Status						
Does not have DB pension; has EPRHI, wealth > median	18.2	67.4	3.9	7.3	3.2	1,000
Has DB pension, does not have EPRHI, wealth <= median	16.7	76.4	2.7	2.6	1.6	1,009
Others	17.7	69.3	4.1	6.1	2.8	12,296

Source: Health and Retirement Study.

Notes: Sample: HRS cohort born 1931-1941 or married to someone in that birth cohort; age 51-72 at the date of survey; first six survey waves (1992-2002). Good health = self-reported excellent, very good, or good health; bad health = fair or poor health. Wealth is deflated by the CPI. Full time (FT) = 35+ hours per week. Part time (PT) = 1-34 hours per week. Full Year (FY) = 36+ weeks worked per year. Part Year (PY) = 1-35 weeks worked per year. Long tenure = 5+ years with employer. DB = Defined Benefit. EPRHI = Employer Provided Retiree Health Insurance. The wealth distribution is measured at wave *t*. The survey is bi-annual, so the average length of time between waves is two years.

Table 2.2. Means and Standard Deviations of Selected Sample Characteristics

(standard deviations in parentheses)

		SIPP sample of potential matches	Sample of actual SIPP/LEHD matches
Age, (years)		52.65 (5.80)	52.57 (5.75)
Five-year age groups, (fractions)	Age 45-49	0.37	0.37
	Age 50-54	0.29	0.29
	Age 55-59	0.20	0.20
	Age 60-64	0.11	0.11
	Age 65-69	0.03	0.03
Gender, (fractions)	Males	0.50	0.50
	Females	0.50	0.50
Race, (fractions)	White	0.87	0.89
	Black	0.10	0.08
	Other	0.04	0.03
Marital status, (fractions)	Single	0.29	0.29
	Married	0.71	0.71
Education, (years)		13.45 (2.99)	13.52 (2.92)
Monthly income other than the individual's earnings, ($)		1404 (1740)	1402 (1698)
Wealth, ($ thousands)		111 (932)	123 (1270)
Wage rate, ($ per hour)		9.55 (7.93)	9.85 (7.98)
Initial experience, (years)		22.74 (14.76)	23.98 (14.13)
Tenure, (months)		141.45 (123.22)	143.84 (122.33)
Pension plan coverage, (fraction)		0.48	0.52
Defined benefit pension plans, (fraction)		0.31	0.32
Health status, (fraction in good health)		0.91	0.91
Disabled, (fraction)		0.09	0.08
Health insurance in own name, (fraction)		0.75	0.78
Employer provided health insurance, (fraction of those with HI)		0.79	0.83

Table 2.2. Continued

	SIPP sample of potential matches	Sample of actual SIPP/LEHD matches
Industry-specific fraction of 65-69 year old workers	0.019 (0.010)	0.018 (0.009)
Industry-specific fraction of female workers less than 30 years old	0.127 (0.075)	0.129 (0.075)
Average *employer*-specific fraction of 65-69 year old workers		0.018 (0.030)
Average *employer*-specific fraction of female workers less than 30 years old		0.106 (0.089)
Separated this month, (fraction)	0.012 (0.111)	0.010 (0.099)
Involuntary separations, (fraction of total separations)	0.39	0.37
Separations leading to change of employer within 30 days, (fraction of total separations)	0.23	0.22
Number of person-months	907,282	473,034
Number of individuals	42,687	22,372

Source: Survey of Income and Program Participation and Longitudinal Employer-Employee Dynamics Files.

Notes: Dollar amounts are deflated by the Consumer Price Index, base year 1982-84.

Table 2.3. Selected Coefficient Estimates from Logit Models of Monthly Job Separation

(standard errors in parentheses)

	(1)	(2)	(3)	(4)	(5)	(6)
Worker's age group						
Age50-54	0.068 (0.066)	-0.082 (0.105)	-0.088 (0.106)	0.088 (0.080)	-0.075 (0.106)	0.094 (0.080)
Age55-59	**0.176** (0.098)	**0.287** (0.153)	**0.268** (0.153)	0.186 (0.136)	**0.268** (0.154)	0.199 (0.136)
Age60-64	**0.648** (0.133)	**0.662** (0.207)	**0.657** (0.207)	**0.483** (0.191)	**0.659** (0.208)	**0.497** (0.192)
Age65-69	**0.853** (0.179)	**1.111** (0.277)	**1.096** (0.278)	**0.770** (0.252)	**1.073** (0.279)	**0.822** (0.254)
Industry-specific fraction aged 65-69	1.301 (1.723)	-0.587 (2.816)	0.221 (2.848)		-0.455 (2.904)	
Age50-54 * *industry*-specific fraction	-1.251 (2.341)	**7.145** (3.989)	**7.419** (3.976)		**9.367** (4.087)	
Age55-59 * *industry*-specific fraction	**-7.445** (2.550)	**-7.908** (4.180)	-6.078 (4.155)		-5.240 (4.308)	
Age60-64 * *industry*-specific fraction	**-19.206** (2.734)	**-13.602** (4.732)	**-13.097** (4.786)		**-10.768** (4.849)	
Age65-69 * *industry*-specific fraction	**-15.252** (3.575)	**-16.943** (5.742)	**-16.256** (5.755)		**-15.451** (5.816)	
Employer-specific fraction aged 65-69				1.402 (1.331)	1.362 (1.363)	2.052 (1.336)
Age50-54 * *employer*-specific fraction				-2.333 (1.798)	**-3.369** (1.884)	-2.778 (1.787)
Age55-59 * *employer*-specific fraction				-2.177 (1.726)	-1.690 (1.774)	-2.606 (1.723)
Age60-64 * *employer*-specific fraction				**-3.998** (1.814)	**-3.311** (1.796)	**-4.474** (1.818)
Age65-69 * *employer*-specific fraction				-1.863 (1.449)	-1.496 (1.466)	**-2.476** (1.448)
N(person-months)	907,282	473,034	473,034	473,034	473,034	471,104
N(individuals)	42,687	22,372	22,372	22,372	22,372	22,296

Source: Survey of Income and Program Participation and Longitudinal Employer-Employee Dynamics Files.

Notes: All specifications include the additional variables described in the text. Coefficient estimates on the other variables are shown in the Appendix for the specification in column 5. The estimates in column 1 use the sample of SIPP observations potentially matchable to the LEHD. The other columns use the sample of SIPP observations that were actually matched to the LEHD. Columns 3-6 include employer characteristics other than the age distribution (firm-average earnings, ownership type, a multi-plant indicator, and total employment, each averaged over all available quarterly observations for a given firm). The specification in column 6 uses three digit industry dummies instead of two digit dummies. Coefficient estimates in bold are significantly different from zero at the 10% level.

Table 2.4. Multinomial Logit Models of Monthly Job Separation, by Destination and Cause of Separation

(standard errors in parentheses)

	Destination of separation		Reason for separation	
	New employer		Employer-initiated	
Worker's age group				
Age50-54	-0.060	(0.204)	0.076	(0.174)
Age55-59	-0.151	(0.335)	0.134	(0.250)
Age60-64	-0.029	(0.490)	0.433	(0.356)
Age65-69	-0.956	(0.904)	0.107	(0.470)
Industry-specific fraction aged 65-69	-8.793	(5.379)	**-8.100**	(4.794)
Age50-54 * *industry*-specific fraction	7.555	(7.617)	**15.451**	(6.364)
Age55-59 * *industry*-specific fraction	10.870	(9.400)	**14.649**	(6.278)
Age60-64 * *industry*-specific fraction	-7.779	(13.031)	1.322	(8.308)
Age65-69 * *industry*-specific fraction	8.867	(23.490)	9.045	(9.617)
Employer-specific fraction aged 65-69	0.132	(2.991)	**3.698**	(1.700)
Age50-54 * *employer*-specific fraction	0.115	(3.661)	**-7.572**	(2.747)
Age55-59 * *employer*-specific fraction	-1.036	(3.822)	-2.732	(2.307)
Age60-64 * *employer*-specific fraction	0.297	(4.554)	**-4.819**	(2.284)
Age65-69 * *employer*-specific fraction	-2.587	(5.058)	**-3.688**	(2.230)
	Non-employment		Worker-initiated	
Worker's age group				
Age50-54	-0.036	(0.128)	-0.166	(0.137)
Age55-59	**0.409**	(0.177)	**0.410**	(0.199)
Age60-64	**0.790**	(0.233)	**0.758**	(0.258)
Age65-69	**1.256**	(0.304)	**1.445**	(0.342)
Industry-specific fraction aged 65-69	1.724	(3.523)	5.823	(3.774)
Age50-54 * *industry*-specific fraction	**9.274**	(4.881)	5.234	(5.305)
Age55-59 * *industry*-specific fraction	**-8.846**	(5.008)	**-20.548**	(6.056)
Age60-64 * *industry*-specific fraction	**-11.234**	(5.270)	**-18.820**	(6.006)
Age65-69 * *industry*-specific fraction	**-15.897**	(6.271)	**-28.292**	(7.186)
Employer-specific fraction aged 65-69	1.893	(1.538)	-0.228	(2.098)
Age50-54 * *employer*-specific fraction	**-4.694**	(2.154)	-0.742	(2.653)
Age55-59 * *employer*-specific fraction	-2.276	(2.023)	-0.780	(2.750)
Age60-64 * *employer*-specific fraction	**-4.247**	(1.966)	-2.034	(2.623)
Age65-69 * *employer*-specific fraction	-1.916	(1.632)	0.003	(2.230)
N(person-months)	473,034		473,034	
N(individuals)	22,372		22,372	

Source: Survey of Income and Program Participation and Longitudinal Employer-Employee Dynamics Files.
Notes: All specifications include the additional variables described in the text. The specifications reported here correspond to the specification in column 5 of Table 3. The first pair of columns shows estimates from a multinomial logit model in which the outcomes are started a job with a new employer within 30 days of separation, remained non-employed 30 days after separation, and did not separate. The second pair of columns shows estimates from a multinomial logit model in which the outcomes are involuntary separation (laid off, fired, plant closed), voluntary separation (quit, retired), and no separation. Coefficient estimates in bold are significantly different from zero at the 10% level.

Table 2.5. Selected Estimates from Logit Models with the Fraction of Young Females

(the fraction of female workers less than 30 years old is used as alternative proxy for technology flexibility, standard errors in parentheses)

	(1)	(2)	(3)
Worker's age group			
Age50-54	0.073 (0.090)	0.048 (0.101)	0.073 (0.091)
Age55-59	**0.254** (0.141)	**0.299** (0.150)	**0.268** (0.142)
Age60-64	**0.580** (0.196)	**0.555** (0.203)	**0.599** (0.198)
Age65-69	**0.971** (0.260)	**0.999** (0.271)	**1.027** (0.262)
Industry-specific fraction females < 30		0.699 (0.455)	
Age50-54 * *industry*-specific fraction		0.307 (0.601)	
Age55-59 * *industry*-specific fraction		-0.588 (0.660)	
Age60-64 * *industry*-specific fraction		0.369 (0.716)	
Age65-69 * *industry*-specific fraction		-0.184 (0.968)	
Employer-specific fraction females < 30	**0.553** (0.292)	0.441 (0.330)	**0.516** (0.307)
Age50-54 * *employer*-specific fraction	-0.143 (0.382)	-0.277 (0.494)	-0.145 (0.382)
Age55-59 * *employer*-specific fraction	**-0.779** (0.429)	-0.474 (0.543)	**-0.854** (0.435)
Age60-64 * *employer*-specific fraction	**-1.360** (0.467)	**-1.550** (0.588)	**-1.465** (0.468)
Age65-69 * *employer*-specific fraction	**-1.724** (0.650)	**-1.629** (0.802)	**-1.868** (0.657)
N(person-months)	473,034	473,034	471,104
N(individuals)	22,372	22,372	22,296

Source: Survey of Income and Program Participation and Longitudinal Employer-Employee Dynamics Files.

Notes: All specifications include the additional variables described in the text. The specifications reported here correspond to the specifications in columns 4-6 of Table 3. Coefficient estimates in bold are significantly different from zero at the 10% level.

Table 2.6. Selected Coefficient Estimates from Alternative Logit Specifications

(standard errors in parentheses)

	(1)	(2)	(3)	(4)	(5)
	Females	Males	1990-1995	1996-2003	quarter-specific fraction
Worker's age group					
Age50-54	-0.115	-0.048	-0.267	0.032	-0.086
	(0.146)	(0.156)	(0.189)	(0.130)	(0.106)
Age55-59	0.239	0.206	0.008	**0.390**	**0.269**
	(0.218)	(0.223)	(0.255)	(0.194)	(0.154)
Age60-64	**0.611**	**0.578**	0.333	**0.764**	**0.661**
	(0.295)	(0.296)	(0.337)	(0.264)	(0.208)
Age65-69	**1.097**	**0.918**	**1.113**	**1.035**	**1.101**
	(0.388)	(0.408)	(0.457)	(0.358)	(0.282)
Industry-specific fraction aged 65-69	-1.442	-0.963	-2.694	0.468	0.072
	(3.959)	(4.379)	(4.885)	(3.618)	(2.899)
Age50-54 * *industry*-specific fraction	**11.081**	8.485	**12.027**	7.680	**7.782**
	(5.492)	(6.339)	(6.913)	(5.138)	(4.096)
Age55-59 * *industry*-specific fraction	-1.224	-5.580	-3.889	-4.908	-5.225
	(6.006)	(6.443)	(7.048)	(5.474)	(4.217)
Age60-64 * *industry*-specific fraction	-1.427	**-16.061**	-10.716	-6.899	**-12.425**
	(6.825)	(6.970)	(7.218)	(6.460)	(4.817)
Age65-69 * *industry*-specific fraction	**-17.448**	-11.319	-11.864	**-15.379**	**-15.585**
	(7.994)	(8.286)	(10.418)	(7.265)	(5.998)
Employer-specific fraction aged 65-69	-1.063	**4.074**	-0.789	3.081	0.419
	(1.594)	(2.115)	(1.937)	(1.903)	(0.965)
Age50-54 * *employer*-specific fraction	-0.123	**-8.422**	-1.020	**-5.354**	-1.019
	(2.188)	(3.585)	(2.907)	(2.488)	(1.516)
Age55-59 * *employer*-specific fraction	0.423	**-4.834**	-1.724	-2.612	-1.575
	(2.395)	(2.623)	(3.106)	(2.358)	(1.363)
Age60-64 * *employer*-specific fraction	-0.329	**-7.122**	-0.980	**-5.639**	-0.911
	(2.041)	(3.244)	(2.450)	(2.734)	(1.248)
Age65-69 * *employer*-specific fraction	1.160	-4.048	-1.026	-3.190	-0.483
	(1.679)	(2.538)	(2.620)	(1.981)	(1.228)
N(person-months)	236,815	236,219	176,692	296,342	455,359
N(individuals)	11,212	11,160	10,128	13,288	22,356

Source: Survey of Income and Program Participation and Longitudinal Employer-Employee Dynamics Files.

Notes: All specifications include the additional variables described in the text. The specifications reported here correspond to the specification in column 5 of Table 3. The specification in column 5 uses employer-quarter-specific measures of the fraction aged 65-69 instead of the employer-specific fraction averaged over all quarters. Coefficient estimates in bold are significantly different from zero at the 10% level.

Table 2.7. Full Set of Parameter Estimates of the Monthly Job Separation Hazard

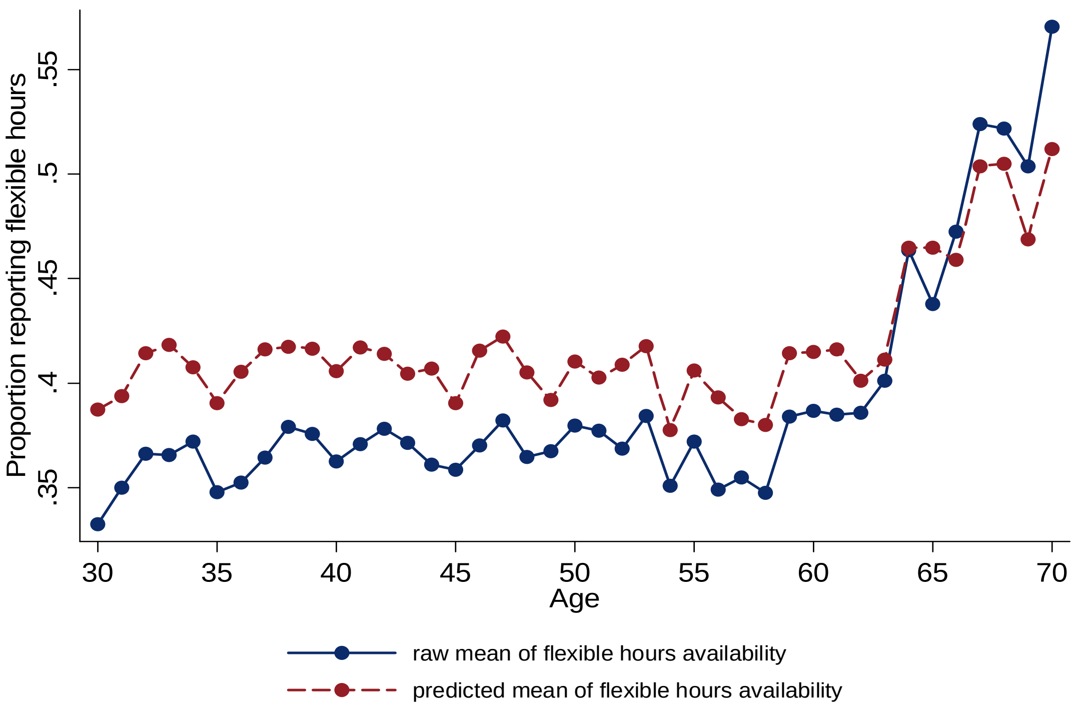
	Coefficient	Robust St. Err.
Age50_54	-0.075	(0.106)
Age55_59	**0.268**	(0.154)
Age60_64	**0.659**	(0.208)
Age65_69	**1.073**	(0.279)
Industry-level fraction 65-69	-0.455	(2.904)
Age50_54 * industry fraction 65-69	**9.367**	(4.087)
Age55_59 * industry fraction 65-69	5.240	(4.308)
Age60_64 * industry fraction 65-69	**-10.768**	(4.849)
Age65_69 * industry fraction 65-69	**-15.451**	(5.816)
Employer-level fraction 65-69	1.362	(1.363)
Age50_54 * employer fraction 65-69	**-3.369**	(1.884)
Age55_59 * employer fraction 65-69	-1.690	(1.774)
Age60_64 * employer fraction 65-69	**-3.311**	(1.796)
Age65_69 * employer fraction 65-69	-1.496	(1.466)
Age	**-2.332**	(0.869)
Age squared	**0.042**	(0.016)
Age cubed	**0.0003**	(0.0001)
Male	-0.016	(0.041)
Black	**-0.157**	(0.064)
American Indian	0.099	(0.130)
Asian	-0.154	(0.098)
Married, Spouse Absent	0.060	(0.153)
Widowed	0.101	(0.073)
Divorced	**0.153**	(0.044)
Separated	0.063	(0.099)
Never married	**0.141**	(0.071)
Education	**0.011**	(0.007)
Real income of other household members	1.438	(1.049)
Total household wealth	-0.001	(0.002)
Indicator: Wealth imputed	**-0.421**	(0.099)
Real wage	**0.006**	(0.003)
Indicator: Wage imputed	**1.464**	(0.074)
Tenure	**-0.005**	(0.001)
Tenure squared	**0.000**	(0.000)
First quarter of tenure	**0.148**	(0.051)
First year of tenure	**0.174**	(0.059)
Year 2-5 of tenure	0.060	(0.052)
Initial experience	**-0.006**	(0.002)
Indicator: Experience imputed	-0.038	(0.066)
Pension plan indicator	**-0.266**	(0.102)
DB pension plan indicator	**0.183**	(0.082)
Employer contributions indicator	-0.029	(0.083)
Indicator: Pension information imputed	**1.975**	(0.047)
Disabled	**0.404**	(0.045)
Bad health	-0.012	(0.047)
Indicator: Self-reported health imputed	**-0.624**	(0.073)
Health insurance, own name	**-0.275**	(0.051)
Health insurance, others name	**0.101**	(0.046)
Employer provided health insurance	**-0.375**	(0.048)
Industry:		
Mining	0.393	(0.249)
Construction	0.237	(0.177)
Non-durables	0.202	(0.173)
Durables	0.157	(0.172)
Transportation	-0.072	(0.184)
Public utilities	**0.342**	(0.189)
Wholesale trade	0.154	(0.174)
Retail trade	0.004	(0.168)
Finance	0.022	(0.171)
Repair services	0.228	(0.169)
Personal services	0.083	(0.181)
Recreation services	0.055	(0.189)
Health services	-0.070	(0.174)
Educational services	0.043	(0.182)
Other services	0.118	(0.172)
Public administration	0.061	(0.198)

Table 2.7. Continued

	Coefficient	Robust St. Err.
Occupation:		
Executives	-0.062	(0.060)
Professionals	**-0.244**	(0.101)
Technicians	-0.030	(0.065)
Sales	0.025	(0.055)
Administrative support	**-0.748**	(0.311)
Private household	0.071	(0.125)
Protective service	**-0.205**	(0.068)
Farming, forestry and fishing	0.134	(0.153)
Craft and repair	0.016	(0.065)
Machine operators	0.003	(0.074)
Transportation and material moving	0.033	(0.083)
Handlers, helpers, and laborers	0.052	(0.095)
Class of worker:		
Private non-profit	**-0.142**	(0.070)
Federal government	**-0.213**	(0.107)
State government	-0.013	(0.116)
Local government	-0.140	(0.114)
Armed forces	**-1.641**	(0.443)
Family business	**-2.212**	(0.731)
Other employer characteristics:		
Firm size <= 5 workers	**-0.405**	(0.087)
Firm size 6-10 workers	**-0.224**	(0.084)
Firm size 11-25 workers	**-0.140**	(0.070)
Firm size 26-50 workers	0.012	(0.067)
Firm size 51-75 workers	0.016	(0.077)
Firm size 76-100 workers	-0.067	(0.083)
Firm size 101-200 workers	0.005	(0.059)
Firm size 201-500 workers	0.008	(0.053)
Firm size 500-1000 workers	**-0.126**	(0.058)
Average number of workers	**-0.040**	(0.019)
Fraction of females in the firm's work force	0.073	(0.092)
Fraction of whites in the firm's work force	0.021	(0.113)
Fraction of blacks in the firm's work force	0.004	(0.177)
Average earnings at the firm	**-4.926**	(2.918)
90th percentile of average earnings	0.801	(0.871)
75th percentile of average earnings	-0.499	(2.090)
50th percentile of average earnings	3.595	(3.645)
25th percentile of average earnings	2.718	(4.692)
10th percentile of average earnings	1.108	(3.278)
Average accession rate	**0.996**	(0.138)
Multi-plant dummy	-0.030	(0.040)
Firm age	0.000	(0.002)
Firm age censored dummy	0.067	(0.056)
State government firm	-0.148	(0.215)
Local government firm	-0.238	(0.193)
Private sector firm	-0.095	(0.170)
State of employment set of 31 dummies	Yes	
Metropolitan area indicator	0.056	(0.040)
Time trend	**0.003**	(0.001)
Constant	-28.122	(25.221)

Notes: Initial experience is equal to the total number of months of work experience as of the beginning of the SIPP coverage. The quarter-specific accession rate is defined as the number of workers with positive earnings in quarter t who were not employed in quarter t-1 divided by the average number of workers in quarters t-1 and t. All workforce demographic, size, earnings and turnover characteristics are further averaged over all quarters of data available for an employer. Firm age is equal to the number of quarters an employer is observed in the LEHD. Firm age is left censored if an employer appears in the first quarter of the LEHD coverage period. A dummy variable indicating whether the firm's age is left censored is included in the model. Coefficient estimates in bold are significantly different from zero at the 10% level.

Figure 2.1. Raw and Predicted Means of Flexible Hours Availability by Age



Source: May 2001 Current Population Survey Supplement on Work Schedules and Work at Home.

Note: The predicted mean is based on a regression equation with controls for demographic characteristics, single year-of-age dummies, and detailed industry, occupation, and class of worker controls. The predicted mean holds constant all of the regressors other than the age dummies.

Figure 2.2. Average Monthly Separation Rates by Single Year of Age

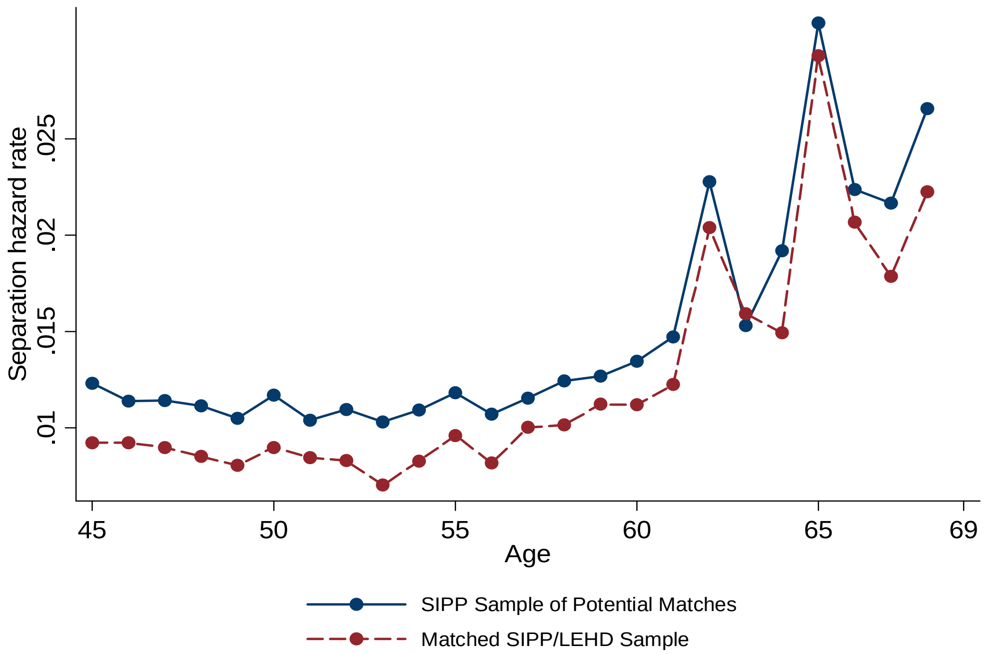


Figure 2.3. Results of Simulation One

Predicted Monthly Separation Rate
by Single Year of Age and Employer-Specific Fraction of 65-69 Year Old Workers,
No Other Controls

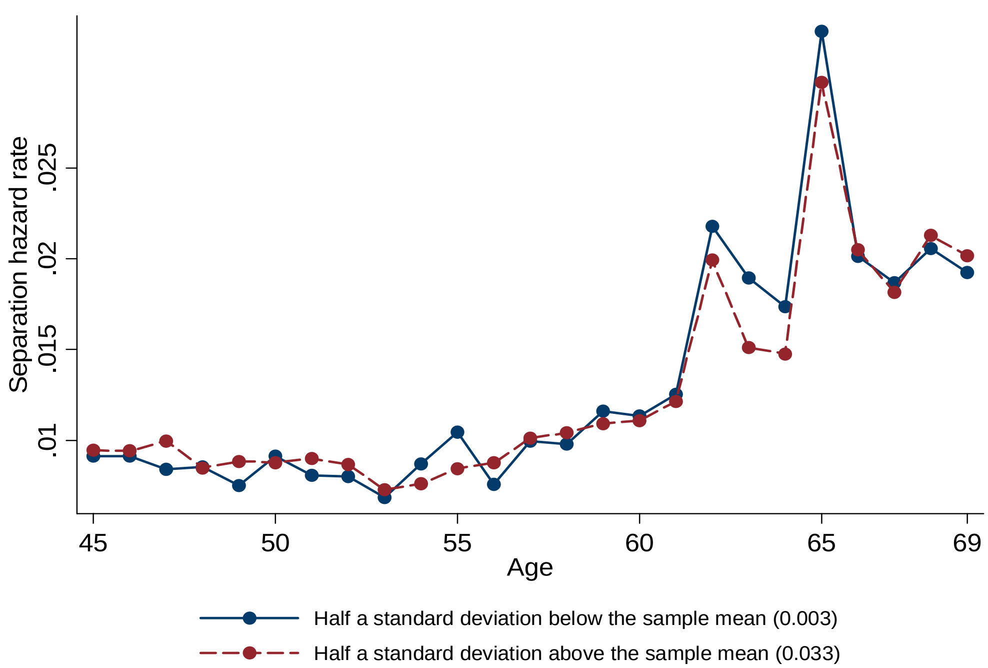


Figure 2.4. Results of Simulation Two

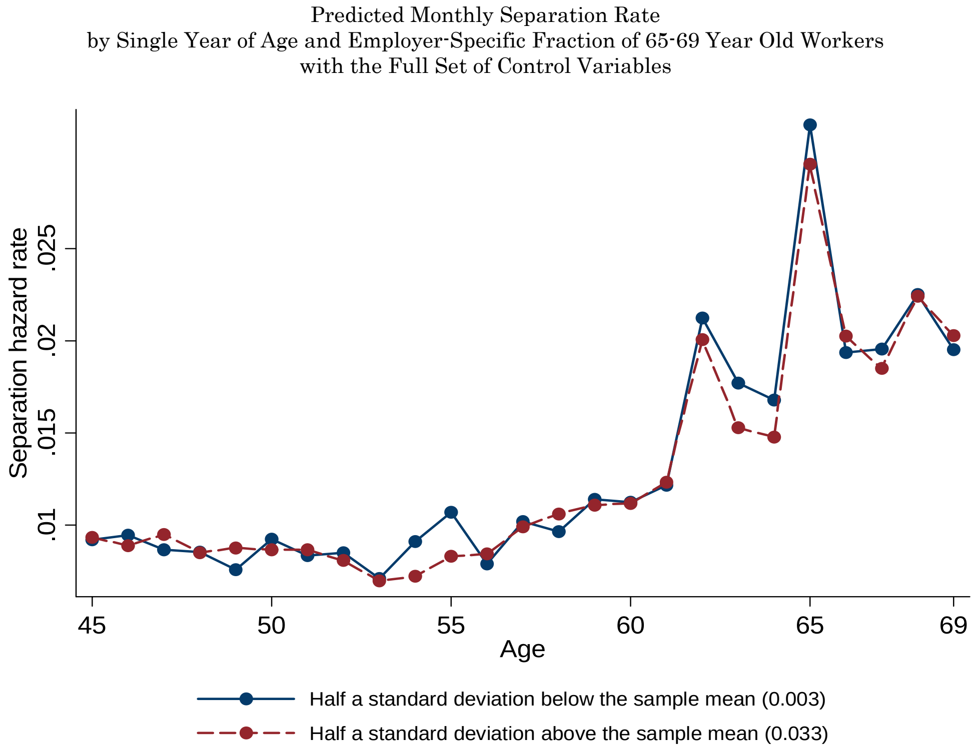
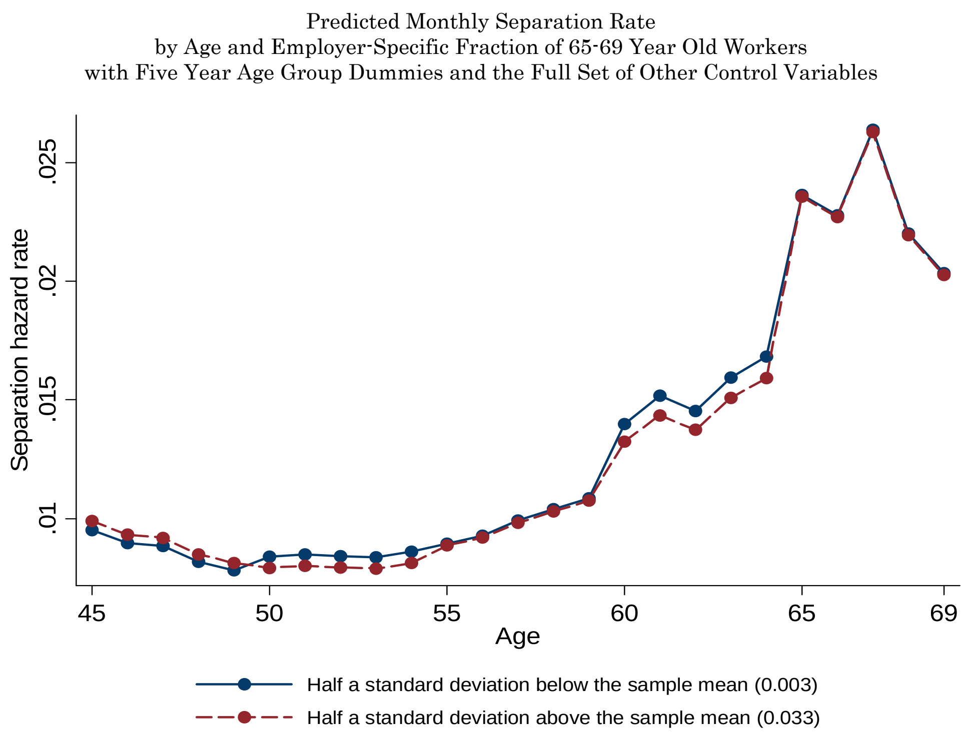


Figure 2.5. Results of Simulation Three



Appendix A

A Model of Labor Market Rigidity

There are two types of firms that differ by the technology employed. Type A firms use a technology that does not have any features associated with labor market rigidity, while type B firms use a technology that has at least one such feature. We use the example of team production here. For simplicity, we ignore non-labor inputs. The type A technology is standard: $Q_A = F^A(L_A)$ where Q is output and L is total hours of labor input. We assume that the marginal product of labor (MPL) is a continuous smoothly declining function of L_A. Thus a type A firm is indifferent to the number of hours worked by any particular worker. The type B production function is $Q_B = F^B(L_B * (min\{L_F, L_2, ..., L_N\})^\theta)$, where L_i is the number of hours worked by the i^{th} worker, there are N workers employed by the firm, and $L_B = \sum L_i$. In this technology, there is a productivity bonus of $\theta \geq 0$ for every hour in which all members of the "team" of N workers are present (assuming, for example, all workers begin the workday at the same time). We take N to be a parameter of the technology: team size must be no smaller than N in order to realize any gains from team production, and (in this simple example) there is no additional gain to a team size greater than N (see Coles and Treble, 1996, for a similar approach). If $\theta = 0$, then the production function is of the standard non-team type, and there will not be any labor market rigidity (the constraint of hiring N workers in this case is not binding). If $\theta > 0$, then the labor input for a type B firm has a fixed coefficients component in which the MPL $= 0$ for that component unless all team members increase hours worked jointly. Hence if $\theta > 0$, a type B firm has an incentive to require all workers to work the same number of hours. We assume that type B firms respond to this incentive by requiring all workers to work the same number of hours, denoted L_{iB}. The type B production function can then be rewritten as $Q = F^B(L_B L_{iB}^\theta) = F^B(N L_{iB} L_{iB}^\theta) = F^B(N L_{iB}^{1+\theta})$, where total labor input $L_B = N L_{iB}$, the number of workers multiplied by hours per worker.

Time is continuous and firms live forever. A given firm is endowed with either type A or type B technology, and cannot change its type. We assume a steady state environment with a stable population age distribution and no changes in technology. Taking the price of output and the hourly wage rate in sector A, W_A, as given, a type A firm chooses the total number of labor hours demanded, L_A^D, to maximize profit. Taking output price, team size N, and the hourly wage rate in sector B, W_B, as given, a type B firm chooses the number of hours demanded per worker, L_{iB}^D, to maximize profit, with the resulting total number of labor hours demanded by a type B firm given by $L_B^D = NL_{iB}^D$. We assume homogeneous firms within sector, with the number of firms per sector normalized to one.

Individuals enter the labor market at age $t = 0$ and live until age T. The utility function is $U(C, 1-L, \delta)$, where C is consumption, 1 is total available hours at a given instant, L is hours of work, $1 - L$ is hours of leisure, and $\delta > 0$ is a parameter such that the marginal utility of leisure is increasing in δ. Individuals are heterogeneous in leisure preferences. Upon entering the labor market, an individual draws an initial value of δ, δ_0, from the continuous cumulative distribution function $G_0(\delta)$. Shocks to the value of δ arrive randomly according to a stochastic process. For simplicity, we assume that an individual can experience at most one preference shock in his lifetime, and all preference shocks are positive, i.e. increasing the preference for leisure. These assumptions are not essential, but they simplify the exposition considerably. Let $\lambda(t)$ denote the instantaneous hazard rate of the preference shock process, and let $F(x)$ denote the continuous CDF of the distribution of shocks, defined on $x \geq 0$. We assume that the size of the shock is independent of the age of arrival of the shock. The only restriction placed on $\lambda(t)$ is that it is non-decreasing with age. There is no access to the capital market, so consumption is given by $C = WL + Z$, where Z is nonwage income, assumed for simplicity to be the same for all workers. Workers are homogeneous in productivity, both across workers at a given age, and over age for a given worker. There is no cost to a worker of changing sectors. Workers choose whether to work ($L = 0$ or $L > 0$), the sector (A or B) conditional on working, and in sector A the number of hours of work, to maximize utility, taking W_A, W_B, L_{iB}, and Z as given.

There are at least two qualitatively different types of equilibrium in this model. In one type of equilibrium, the hours of work required by firms in sector B, L^D_{iB}, is *less* than the optimal hours of work of the *marginal* worker. The *marginal* worker has a preference for leisure, δ, that leaves him indifferent between working in sector A or sector B, given W_A, W_B, L_{iB}, and Z. Let $d^*(W_A, W_B, L_{iB}, Z)$ denote the value of δ that makes an individual indifferent between the two sectors. In this case, workers with a relatively weak preference for leisure, $\delta < d^*$, choose the flexible sector (A), and workers with a stronger preference for leisure choose the rigid sector (B). In this type of equilibrium, the constraint imposed by technological rigidity is that it *limits* the number of hours worked, rather than requiring too many hours. This type of equilibrium is perfectly legitimate, but is not relevant for our purposes.

Hence, we focus on the type of equilibrium of interest by assuming that in equilibrium, $L_A(W_A, Z, d^*) < L^D_{iB}$, where $L_A()$ is the labor supply function of a worker in sector A.[1] In this case, the marginal worker prefers fewer hours of work than the hours required in sector B, hence the constraint imposed by technological rigidity is *excess* hours. In this type of equilibrium, individuals with a strong preference for leisure choose not to work, individuals with a weaker preference for leisure choose to work in Sector A, and individuals with the weakest preference for leisure choose sector B. It is then straightforward to show that the value of δ that makes an individual indifferent between working in sectors A and B, d^*, is defined by $V^A(W_A, Z, d^*) = V^B(W_B, Z, d^*, L_{iB})$, where V^A and V^B are indirect utility functions.[2] And there exists a reservation value of δ, $\overline{d} > d^*$, such that a worker is indifferent between sector A and non-employment if $V^A(W_A, Z, \overline{d}) = V^R(Z, \overline{d})$, where V^R represents the utility of retirement (R).

The assumption of a stable population age structure implies that d^* and $\overline{d}$ are constant over time. Thus an individual with $\delta_t < d^*$ chooses sector B at age t, an individual with $d^* \leq \delta_t < \overline{d}$ chooses sector A at age t, and an individual with $\overline{d} \leq \delta_t$ chooses retirement at age t. The value of δ_t for a given worker is equal to the worker's initial draw, δ_0, if he has not received a preference shock by age t; and is equal to $\delta_0 + x$ if he has received a preference

[1] We parameterized the model and solved it numerically, since an analytic solution does not exist. Both types of equilibrium were found to exist, for alternative parameter values.

[2] It is also straightforward to demonstrate that in this type of equilibrium, $W_B > W_A$.

shock by age t, where $x > 0$ is the value of the shock drawn from distribution F. Normalizing the size of the population to one, total hours of labor supplied to sector B is

$$L_B^S = \int_0^T L_{iB} G_t(d^*) dt,$$

where G_t is the distribution of δ at age t, and the dependence of the reservation value d^* on wages is implicit. Total hours of labor supplied to sector A for given wage rates is

$$L_A^S = \int_0^T \int_{d^*}^{\overline{d}} L_A(W_A, Z, \delta) dG_t(\delta) dt.$$

The model is closed by the assumption of market clearing: the quantity of labor supplied equals the quantity of labor demanded in each sector: $L_A^S = L_A^D, L_B^S = L_B^D$. These two conditions determine the equilibrium values of W_A and W_B, which in turn determine the threshold values d^* and $\overline{d}$, and hours of work required per worker in sector B, L_{iB}.

To derive predictions in the most straightforward way, suppose that: (a) the distribution $G_0(\delta)$ is uniform on $(\delta_{min}, \delta_{max})$, with $\delta_{max} - \delta_{min} = 1$; (b) the distribution $F(x)$ is uniform on $(0, 1)$; and (c) the age distribution is uniform on $(0, T)$. Let t^* represent an arbitrary age that divides older workers from younger workers. Finally, let $Q(t)$ denote the probability that an individual has *not* received a preference shock by age t:

$$Q(t) = exp\{-\int_0^t \lambda(u) d(u)\}.$$

Given the assumption that the magnitude of the preference shock, x, is independent of the initial value of δ and of the age at which the shock arrives, the probability that a worker chooses sector B at age t is given by

$$Pr(B_t) = G_t(d^*) = Pr(\delta_t < d^*)$$

$$= Pr(\delta_0 < d^*)[Pr(no\ shock\ by\ t) + Pr(shock\ by\ t, \delta_0 + x < d^*)]$$

$$= G_0(d^*)[Q(t) + (1 - Q(t)) \int_{\delta_{min}}^{d^*} F(d^* - \delta_0) g_0(\delta_0) d\delta_0]$$

$$= (d^* - \delta_{min})[Q(t) + (1 - Q(t))\frac{1}{2}(d^* - \delta_{min})^2],$$

where the last equality exploits the functional form assumptions described above. The share of all individuals in sector B, which is constant over time, is then given by

$$Pr(B) = \int_0^T G_t(d^*)dt = \frac{(d^* - \delta_{min})[H(T) + (T - H(T))\frac{1}{2}(d^* - \delta_{min})^2]}{T},$$

where

$$H(T) = \int_0^T Q(t)dt = \int_0^T exp\{-\int_0^t \lambda(u)d(u)\}dt.$$

Using similar logic, the share of *older* individuals in sector B is

$$Pr(B|\ t > t^*) = \frac{(d^* - \delta_{min})[(H(T) - H(t^*))(1 - \frac{1}{2}(d^* - \delta_{min})^2) + (T - t^*)\frac{1}{2}(d^* - \delta_{min})^2]}{T},$$

where

$$H(t^*) = \int_0^{t^*} Q(t)dt = \int_0^{t^*} exp\{-\int_0^t \lambda(u)d(u)\}dt.$$

The fraction of the labor force in sector B that is old is given by

$$\frac{Pr(B|\ t > t^*)}{Pr(B)} = \frac{(H(T) - H(t^*))(1 - \frac{1}{2}(d^* - \delta_{min})^2) + (T - t^*)\frac{1}{2}(d^* - \delta_{min})^2}{H(T) + (T - H(T))\frac{1}{2}(d^* - \delta_{min})^2}.$$

Similar derivations for sector A yield the fraction of the labor force in sector A that is old as

$$\frac{Pr(A|\ t > t^*)}{Pr(A)} = \frac{(H(T) - H(t^*))(1 - (d^* - \delta_{min})^2) + (T - t^*)(d^* - \delta_{min})^2}{H(T) + (T - H(T))(d^* - \delta_{min})^2}.$$

Comparing these fractions, it is easy to show that the fraction of older workers in sector A is greater than the fraction of older workers in sector B if

$$\frac{H(t^*)}{H(T)} > \frac{t^*}{T}.$$

To show that this condition holds, consider each side of the inequality as a function of t^*. From the expressions for $H(t^*)$ and $H(T)$ it is easy to see that both sides of the inequality are equal to zero for $t^* = 0$ and both sides are equal to one for $t^* = T$. The right hand side is linear and increasing in t^*. The left hand side is concave and increasing in t^* under the assumption that $\lambda(t)$ is increasing in t. The only way for both sides of the expression to be equal at the endpoints when the left hand side is increasing and concave and the right hand side is increasing and linear is for the left hand side to be strictly greater than the right hand side everywhere except at the endpoints. This proves the claim that for $0 < t^* < T$,

$$\frac{Pr(A|\ t > t^*)}{Pr(A)} > \frac{Pr(B|\ t > t^*)}{Pr(B)}.$$

The next question is whether the hazard rate for exit from sector B exceeds the hazard rate for exit from sector A. We argue in the text that this will be true because changes in leisure preferences can be accommodated in sector A, while in sector B the only way to accommodate a change in preferences is to leave the sector. We assumed above that an individual experiences at most one preference shock in his lifetime. This greatly simplified the proof that the share of older workers in the flexible sector (A) exceeds the share of older workers in the rigid sector (B). Under this assumption, the hazard rate for exit from sector B at age $t > t^*$, denoted $\gamma_B(t)$, is the probability of receiving a large enough preference shock at age t, conditional on not having received a shock prior to t. Under the functional form assumptions described above, this is given by

$$\gamma_B(t) = \frac{\lambda(t)(1 - \frac{1}{2}(d^* - \delta_{min}))T}{(T - t^*)Q(t)}.$$

The hazard rate for exit from sector A, $\gamma_A(t)$, is equal to the weighted average of the hazard rate conditional on having started life in sector A and not having received a shock by t, and the hazard rate conditional on having started life in sector B and having already received a shock by t that resulted in a switch from B to A. Under the assumption of at most one shock in a lifetime, the latter hazard rate is zero. Under the functional form assumptions described above,

$$\gamma_A(t) = \frac{\lambda(t)Q(t)(\overline{d} - d^*)(1 - \frac{1}{2}(\overline{d} - d^*))T}{Q(t) + (1 - Q(t))(d^* - \delta_{min})^2(T - t^*)}.$$

Then $\gamma_B(t) > \gamma_A(t)$ if

$$Q(t)(1-\frac{1}{2}(d^*-\delta_{min}))+(1-Q(t))(1-\frac{1}{2}(d^*-\delta_{min}))(d^*-\delta_{min})^2 > Q(t)(1-\frac{1}{2}(\overline{d}-d^*))(\overline{d}-d^*)^2.$$

This cannot be shown to be true in general. It is more likely to hold if $Q(t)$ is small (the probability of a shock by age t^* is large); $(d^* - \delta_{min})$ is small (sector B is relatively small); and $(\overline{d} - d^*)$ is large (sector A is relatively large). We conjecture that as the number of shocks allowed per lifetime increases, the ambiguity will be resolved, but we have not been able to prove this.

Finally, if there is no rigid sector, then differences in the share of older workers across firms are determined solely by random variation in the arrival rate of preference shocks to the workers in different firms. These differences have no implications for the hazard rate of exit of older workers from employment. Thus, in a version of the model with no variation in technology, there is no reason to expect the share of older workers to be associated with the hazard rate of exit from a firm. In the limit, as firm size grows large, all firms would have the same age structure.