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ABSTRACT

We show that in labor market models with adverse selection, otherwise observationally equivalent workers will experience less wage growth following a period in which they change jobs than following a period in which they do not. We find little or no evidence to support this prediction. In most specifications the coefficient has the opposite sign, sometimes statistically significantly so. When consistent with the prediction, the estimated effects are small and statistically insignificant. We consistently reject large effects in the predicted direction. We argue informally that our results are also problematic for a broader class of models of competitive labor markets.

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An individual's career usually spans several, or even many, firms. An extensive literature in labor economics studies the possible mechanisms explaining this job-to-job mobility. One key class of explanations includes the role of adverse selection. These explanations posit that if an individual's employer observes his ability more accurately than the rest of the market, then moving provides a negative signal about ability. As has been established, if adverse selection explains who leaves their job, this may affect overall labor market mobility and whether workers are matched to the jobs where they will be most productive. Alternatively, workers may leave their jobs when they identify, or are identified by, another firm in which they will be more productive. This may occur through employee learning over time, or if outside firms observe signals of quality that incumbent firms do not. Given these explanations have different implications for the efficiency of worker-firm matches, it is important to understand the dominant driver of mobility.

We present a test for whether adverse selection is an important force in the labor market. As alluded to above, in labor market models with adverse selection, mobile workers have worse unobservables on average than observationally equivalent workers who remain with their current employer. Therefore, mobility provides the market with negative information about a worker while stability is a positive signal. This implies our prediction that *following a move*, the wages of movers should rise less rapidly than those of similar workers who remain with their employer. We emphasize this is not a prediction about wage changes at the time the worker moves or remains with the employer. Indeed, movers will generally require a compensating differential for the adverse signal that accompanies a move, as well as the lower subsequent wage growth.

Our prediction is also *not* about wage growth on a particular job. Thus, both workers who stay and leave will make subsequent mobility decisions. The cumulative effects of these subsequent decisions lead to higher wage growth for those who initially stayed than for those who initially left.

In a sense our prediction is tautological. By definition, adverse selection[1]

[1] We distinguish between adverse selection models and asymmetric information models in which raiders sometimes have private information not available to incumbents. Lazear

in the market for currently or recently employed workers means that moving signals low quality. We recognize it may be possible to construct models in which revealing oneself to be low ability leads to subsequent wage growth. However, such models appear to us to be *post hoc.* Since there is a large, and possibly infinite, set of potential models of adverse selection in the labor market, we cannot prove that all variants must have the property we test. Instead we argue somewhat informally that the class of models to which it applies is broad and describe three particular formulations for which our claim can be established formally.

As described in the opening paragraph, not all models share this prediction, and it is against these that our test will have power. If outside firms have information that incumbent firms lack, mobility could be a positive signal. Or workers could move to firms with which they are better matched and, once matched, invest in skills that are particularly useful at their new firm. These models would suggest faster subsequent wage growth for movers than for workers who do not move, which is the opposite of our prediction. Thus, if we find movers have slower subsequent wage growth than workers who do not move, this suggests the dominant mechanism explaining mobility is adverse selection, rather than these alternative models. Lack of support for our prediction would suggest these alternative models may be the more important drivers of mobility.

We use the National Longitudinal Survey of Youth, 1979, to test the prediction regarding future wage growth. We find little or no evidence to support this prediction. In most cases, we find very small differences between movers and stayers in subsequent wage growth, and point estimates tend to show slightly *larger* future gains for movers. Using both matching models and simple models with regression controls, we consistently find that in the four years following a move/stay decision, wage growth is higher for those who originally

(1986) shows that with asymmetric information of this type, workers who leave their jobs may be positively selected. This class of models includes Golan (2005) and Pinkston (2009). There is also a literature on adverse selection that focuses on how firms' wage policies affect the unobserved quality of their workers (Pallais 2014, Weiss 1980), which we also do not address.

moved than for those who stayed although the magnitude is small (1 to 2 percent) and not always statistically significant.[2] Our results using a ten-year horizon are similar. Only when we limit ourselves to a one-year horizon do we find any evidence in support of the adverse selection hypothesis, and again, any measured effects are small. Thus our results provide little or no support for the importance of adverse selection in the market for currently and recently employed workers, at least in the case where raiding firms bid competitively.

It is important to recognize both the strengths and limitations of our results. First, adverse selection may be operating in the market, without being the dominant driver of mobility. In these cases, movers may have faster subsequent wage growth than workers who do not move, but these effects are partially offset by adverse selection. Our strategy does not allow us to identify these attenuating effects of adverse selection. However, if subsequent wage growth is higher for movers than for stayers, this suggests adverse selection is not the dominant force behind mobility. For example, skill might have a general and a match-specific component. Firms might recruit workers for whom they receive a positive signal about match quality even while knowing that, on average, they will attract workers with lower than average general quality, given observables.[3] Alternatively, workers who have acquired a set of skills at one job may move to another job at which they can use those skills more productively and acquire new ones. There might still be an element of adverse selection in who moves, but it might be fully obscured by the more important "career ladder" consideration.

Second, consistent with the entire literature on adverse selection in the labor market, we assume that raiding firms act competitively. Adverse selection

[2] Note that this does not include the instantaneous effect of the move/stay decision but does include the effects of any subsequent mobility.

[3] Consider a modified Burdett matching model in which productivity is the sum of a general component observed only by the incumbent firm and a match-specific component that can be observed immediately by a single potential raider. The raider would know that, on average, it would successfully hire workers with low general productivity, but would be willing to offer a higher wage to workers with whom they know they are well matched. As the variance of the common component of productivity goes to 0, we recover the Burdett model in which, provided workers receive a constant share of their VMP, future wage growth is independent of recent mobility conditional on current wage.

models without this assumption may not yield our prediction, as suggested by our example above with match-specific productivity (which made the raiding market uncompetitive). As a result, if we fail to find support for the prediction, we cannot completely rule out the existence of adverse selection. However, as our match-specific productivity example also suggests, at least in some cases we can rule out that adverse selection is the dominant mechanism explaining mobility.

Our findings stand in marked contrast with much, but not all, of the existing empirical literature. The seminal paper, Gibbons and Katz (1991), argues that workers displaced in layoffs are likely to be more adversely selected than those displaced in plant closings. Consistent with this prediction, they find that the former suffer larger wage losses. However, we note that regardless of whether outside firms can observe a worker's productivity, their prediction follows more generally if wages are compressed within the firm (e.g. Frank, 1984), and firms are free to layoff workers in any order they prefer. Indeed Acemoglu and Pischke (1998) explicitly use a model in which adverse selection is the source of wage compression within the firm and find results consistent with wage compression.

In contrast, Schoenberg (2007) follows much of the learning literature by assuming the researcher directly observes a measure of ability not seen by the market, in this case the score on the Armed Forces Qualifying Test administered as part of the National Longitudinal Survey of Youth 1979. Adverse selection models imply that turnover should be negatively related to unobserved ability. Further, in some cases, adverse selection implies wages of incumbent workers should be more responsive than those of new workers to the AFQT. She finds little support for either prediction, with the possible exception of college graduates.

Our approach is in some ways closest to Kahn (2013) although we reach quite different conclusions. She looks at wage variation among movers and among stayers. Intuitively, if raiding firms have no information, then all raiding firms should make the same offers. So greater asymmetry leads to more compressed offers. More generally, increasing the variance of productivity should raise the variance of wages among movers less than among stayers.[4] She uses business-cycle induced variation in the variance of productivity to test this latter prediction and a measure of contact with outside firms to test the former and finds strong evidence of asymmetric learning. An important difference between our approaches is that due to its complexity, her model, like most in this literature, is limited to two periods and therefore cannot address post-mobility wage changes.

Next, we review the theoretical literature on adverse selection models of turnover, showing they share certain characteristics giving rise to our prediction, despite important variation in the precise modeling decisions. Our main challenge is that, with the exception of Greenwald's (1986) three-period model, all existing models are limited to two periods and therefore cannot formally generate our prediction. We therefore show that it holds in Greenwald, a steady-state model in a companion paper (Cavounidis and Lang, 2015) and a multi-period model, the details of which we relegate to an appendix. The remainder of the paper follows the usual data/methods/results/conclusion format.

1 Theory

Almost all existing models of adverse selection in the labor market are limited to two periods. The difficulty is that, in general, wage offers should depend on the worker's entire mobility history and, possibly, whether he stays in the current period. Thus, in period t, there are potentially 2^{t-2} histories of whether the worker changed or remained with their employer, and 2^{t-1} wages (or distributions in the case of Li, 2013) and more if layoffs are permitted. Needless to say, the problem rapidly becomes unwieldy as the number of periods increases. In this section, we argue somewhat informally that our prediction arises from a general class of models, including several well-known two-period models of

[4] Although the intuition behind this result is strong, it does not clearly apply to all models of adverse selection. We do not, for example, believe that it applies to the model of Li (2013), described in greater detail below.

adverse selection. We then show formally that our prediction arises from three variants of multi-period adverse selection models.

1.1 The 'general' argument

There is a broad class of labor turnover and adverse selection models in which competition for workers makes the expected profitability of new hires zero, but firms earn quasi-rents on incumbent workers. We argue, somewhat informally, that under a set of plausible auxiliary assumptions, all such models should have a similar and simple prediction: of two otherwise observationally equivalent workers earning the same wage, a worker who recently started a new job should experience lower subsequent wage growth.

The intuition is relatively straightforward. In adverse selection models, the current employer or 'incumbent' is able to pay the worker less than his value of marginal product (VMP). The employer thus earns quasi-rents on its private information about worker quality. Outside 'raiders' are in competition. They bid up wages to the expected value of marginal product of a new hire plus a premium, reflecting the expected surplus on workers subsequently remaining with the firm. Therefore, if two workers are otherwise observationally equivalent and earn the same wage, the one who has just changed jobs will, on average, be less productive than the one who has not changed jobs recently. Moreover, in the presence of adverse selection, the market infers that the worker who has changed jobs is less productive than it had thought while the worker who remained in her job is more productive. Therefore, if workers are eventually paid their VMP, the worker who has recently changed jobs should subsequently experience slower wage growth than the one who has not.

1.1.1 Regularities in two-period adverse selection models

In this section, we review several prominent adverse selection models of the labor market. We show they all share two regularities. First, workers who move are paid their expected VMP, and so raiders make zero expected profits. Second, workers who do not move earn no more than their VMP and some earn less. Almost all adverse selection models of turnover rely on a simple two-period model in which incumbent firms learn the productivity of their workers at the end of the first period. Most commonly, raiders have no information.

In the equilibrium of Greenwald's two-period model incumbents match the outside offer for workers they wish to retain and do not counteroffer to other workers. Workers who do not receive a counteroffer quit as do some other workers who quit randomly. In equilibrium, the wage in the second-hand market equals the average productivity of workers in that market. The wage of workers who stay with their employer is less than or equal to their VMP.

The base model in Acemoglu and Pischke (1998) has a similar structure. In an extension of the model, incumbents offer higher wages to more able workers in order to reduce the probability of a random quit. In this case, leavers earn less than stayers, but outside firms still make zero-expected profit. Incumbents make positive profit on (almost) all the workers they retain.

Gibbons and Katz (1991) use a structure similar to that in Greenwald with two significant departures. First, incumbents may lay off workers. If they do, the market observes that the workers have been laid off and pays them their average product. Raiders make offers to workers who have not been laid off. The incumbent firm decides whether to match. If it does not, the worker leaves. If it does match, some workers depart randomly anyway. The equilibrium is identical to Greenwald once we restrict the distribution of ability to those not laid off. So regardless of whether they quit or are laid off, workers who move are paid their expected productivity. Those who remain with the incumbent firm are paid less than their productivity.

Several models assume raiding firms observe some signal of the worker's productivity. Schoenberg (2007) allows raiders some information about worker productivity in the form of observable education and an imperfect signal of ability. She also assumes that incumbents can match raiders' offers but also allows incumbents to pay a premium above the outside offer in order to reduce the risk of a random quit. Kahn (2013) assumes the incumbent observes the raiders' signal of the worker's productivity and chooses a wage to maximize expected profit, taking into account the raider's equilibrium wage offer given the signal, and the effect of the wage offer on the risk of a random quit. Despite these differences, as in all the two-period models discussed thus far, raiders make zero expected profit, and incumbents pay the workers they retain less than their marginal product.

The papers discussed so far generate an equilibrium with endogenous turnover either by assuming some exogenous turnover directly or a taste shock that causes some workers to quit even if they receive a premium at their current firm. In contrast, in Li (2013) raiders and incumbents make simultaneous offers, and workers accept the highest offer they receive. In equilibrium, raiders randomize their offers and make zero expected profit. Incumbents' offers are increasing in worker productivity but below the productivity of all workers except the least able.

With the exception of Schoenberg (2007) and Kahn (2013), these models assume that raiders can, at most, observe a worker's employment history, specifically whether he has quit or been laid off. There is an important set of adverse selection models beginning with Waldman (1984) in which raiders can observe a worker's task assignment.[5] These models focus on how adverse selection affects task assignment and so they allow the equilibrium to unravel to one without job-to-job mobility (although there may be layoffs if the worst workers are sufficiently unproductive). Without firm-specific capital, these models would also unravel to one in which all workers are assigned to the lowest-skill task. With firm-specific capital, workers are underpaid at the incumbent firm but raiders' offers equal workers' expected productivity in the tasks to which they would be assigned if they accepted the raiders' offer.

In sum, in the two-period models, we have two regularities:

R1. Workers who change jobs are paid their expected VMP.

R2. Workers who remain with the incumbent firm earn no more than their VMP and some earn strictly less.

R1 is not surprising. It must hold in every model in which there is sufficient competition among potential raiders. If wages exceeded expected VMP, raiders

[5] See also Bernhardt (1995) and Waldman (1990).

would incur losses and would not want to make offers. If wages were less than expected VMP, competing raiders would bid them up.

R2 is also not surprising. Without some commitment mechanism, firms will prefer to fire workers rather than pay them more than their VMP. A number of papers (e.g. Harris and Holmstrom 1982; Lazear 1979) do, however, assume a commitment mechanism exists. If there is a wedge between a worker's productivity at the incumbent and her expected VMP elsewhere, possibly created by private information, match-specific productivity or firm-specific capital, we generally expect the firm to reap some of the (quasi-) rents.

1.1.2 'General' multiperiod adverse selection models: features

In this section we argue that multiperiod models will in general share the regularities of two-period models, regardless of the model's details:

1. *Zero expected profit at time of hire*: Clearly we would not expect firms to raid if doing so would be unprofitable. If poaching led to positive profits, in a market-clearing model, firms would increase their wage offers to out-compete other raiders. The expected discounted present value of future VMP and wages at the hiring firm should be equal

$$\sum_{t=0}^{\infty}\left(\delta^t VMP_t \Pi_{j=0}^{t} p_j\right) = \sum_{t=0}^{\infty}\left(\delta^t w_t \Pi_{j=0}^{t} p_j\right) \tag{1}$$

   where δ is the discount factor, p_j is the probability that the worker remains with the firm in period j given that he has remained with the firm through period $j-1$ and p_0 equals 1. VMP is the expected value of marginal product given the worker is still with the firm.

2. *Informational rents:* Firms earn quasi-rents on their incumbent workers over at least some period. This appears to us to be an essential feature of adverse selection models.

$$\sum_{t=t'}^{\infty}\left(\delta^t VMP_t \Pi_{j=t'}^{t} p_j\right) > \sum_{t=t'}^{\infty}\left(\delta^t w_t \Pi_{j=t'}^{t} p_j\right) \tag{2}$$

for some $t' > 0$. Note that $p_{t'}$ is 1 when evaluated at t'.

Combining (1) and (2), leads to our first important regularity.

Proposition 1 *New workers are initially overpaid.*

Proof. Rewrite (1) as

$$\begin{aligned} & \sum_{t=0}^{t'} \left(\delta^t VMP_t \Pi_{j=0}^{t} p_j\right) + \Pi_{j=0}^{t'} p_j \sum_{t=t'}^{\infty} \left(\delta^t VMP_t \Pi_{j=t'}^{t} p_j\right) \\ = & \sum_{t=0}^{t'} \left(\delta^t w_t \Pi_{j=0}^{t} p_j\right) + \Pi_{j=0}^{t'} p_j \sum_{t=t'}^{\infty} \left(\delta^t w_t \Pi_{j=t'}^{t} p_j\right) \end{aligned} \tag{3}$$

Rearranging terms

$$\begin{aligned} & \sum_{t=0}^{t'} \left(\delta^t VMP_t \Pi_{j=0}^{t} p_j\right) - \sum_{t=0}^{t'} \left(\delta^t w_t \Pi_{j=0}^{t} p_j\right) \\ = & \Pi_{j=0}^{t'} p_j \left(\sum_{t=t'}^{\infty} \left(\delta^t w_t \Pi_{j=t'}^{t} p_j\right) - \sum_{t=t'}^{\infty} \left(\delta^t VMP_t \Pi_{j=t'}^{t} p_j\right) \right) \\ < & \ 0 \end{aligned} \tag{4}$$

where the last inequality follows from (2). ■

Note that this result fails in the special case of the second period of a two-period model since there is no $t' > 2$. For the same reason, it will fail in the last period of any model in which the number of employment periods is finite. It holds in the first period of two-period models but is largely ignored because it leads to the conclusion that, in the absence of other offsetting factors, wages fall from the first to the second period. Of course, the decline can be avoided by assuming that VMP increases sufficiently quickly between periods.

Our third assumption/regularity is somewhat more speculative. We first state the assumption and then argue its plausibility.

3. *The market eventually learns the worker's type:* The raiding market's belief about each worker's productivity has a martingale point estimate.

> The argument is essentially identical to Farber and Gibbons (1996). Moreover, provided productivity is bounded, the martingale is bounded. Therefore we know from Doob's Martingale Convergence Theorem that the market's point estimate of each worker's productivity converges almost surely.

In other words, the posterior beliefs about v, the VMP, satisfy:

$$\lim_{t\to\infty} E[v|\Omega_t] \to \hat{v}_\infty \ a.s.$$

We now argue that beliefs must converge to a point. Suppose the market beliefs for a worker with a given history are nondegenerate but their expectation has converged. Then either there is no additional mobility, every worker quits or the average productivity of quitters equals the average productivity of workers with that history. We rule out the first two possibilities by fiat. They would not happen in any existing model. In the last case, raiding firms would offer workers the expected productivity for workers with their history.[6] Since the workers who leave are, on average, average, the incumbent would make no informational rents and would only be willing to pay workers up to their value of marginal product. But if that were the case, all workers with productivity below the average would quit, and since not all workers quit, on average, quitting workers would not have the expected productivity for workers with their history, a contradiction. Therefore, the raiding market's beliefs should converge to a degenerate distribution. As the truth must be in the support of the limiting distribution if it was in the prior's, beliefs must converge on the truth.

Of course, in reality, lifetimes are not infinite. We require the market to learn the worker's type before his retirement.[7] This is plausible if a) there is

[6] Note that under the assumption, they would not earn informational rents after hiring a worker.

[7] The technical problem that arises is that, in those adverse selection models in which workers move randomly, we can never know whether a worker who followed a particular mobility path is a worker of the type that should follow that path or one who has moved randomly in one or more periods. Of course, with enough data, this should cease to be a problem, but in finite time it will never be eliminated.

an element of symmetric learning that accompanies the asymmetric learning and b) the time to retirement is long.

The importance of this third regularity for our purposes is that if the market knows a worker's productivity then his wage will equal his productivity. This yields our result that of two observationally equivalent workers who are earning the same wage, wage growth should be lower for the worker who just changed jobs. We showed that the worker who changed jobs is less productive. Since their wages are equal in period t, and wages eventually equal productivity, wage growth must be lower for the worker who moved.

More generally, our results will go through if the wages of older workers are close to their VMP. We note that there are a variety of ways of obtaining a similar result. For example, if firms cannot commit not to fire overpaid workers, then over the long run wage can never exceed VMP. But workers will quit when their value of leisure exceeds their wage. Therefore incumbent firms should pay the worker at least his value of leisure. But, under suitable regularity conditions, as the value of leisure approaches VMP, this means that the wage also approaches VMP. A different version of our principal result goes through if retirement age is not (too) sensitive to wages.

1.1.3 'General' multiperiod adverse selection models: implications

Assumptions:

A1. $E\left(VMP_{it}|mover_t\right) \leq w_{it}$

A2. $E\left(VMP_{it}|stayer_t\right) \geq w_{it}$

A3. At least one of the two inequalities above is strict

A4. $\exists t^* | \forall t \in [t^*, T]\ w_t = VMP_t$ where T represents retirement age.

A5. $E\left(VMP_{it'} - VMP_{it}|mover\right) = E\left(VMP_{it'} - VMP_{it}|stayer\right), \forall t' > t.$

The first four assumptions were addressed in the previous section. The last assumption says that job assignment does not affect future productivity growth. This assumption would be violated if, for example, mobility is driven by career factors so that, as they acquired skills, workers moved up a job ladder and invested in new skills more heavily after moving. We note that adverse selection models typically assume no productivity growth.

We now turn to our main result.

Proposition 2 *Suppose $w_{mt} = w_{st}$, where m denotes the mover and s the stayer. Under assumptions (A1)-(A5)*

$$E(w_{mt^*}) - w_{mt} < E(w_{st^*}) - w_{st} \tag{5}$$

Proof. From A1 and A2

$$E(VMP_{mt}) \leq w_{mt} = w_{st} \leq E(VMP_{st}) \tag{6}$$

with at least one strict inequality (A3) so that

$$E(VMP_{mt}) < E(VMP_{st}). \tag{7}$$

We also have from A5

$$\begin{aligned} E(VMP_{mt^*}) - E(VMP_{mt}) &= E(VMP_{mt^*} - VMP_{mt}) && (8) \\ &= E(VMP_{st^*} - VMP_{st}) && (9) \\ &= E(VMP_{st^*}) - E(VMP_{st}). && (10) \end{aligned}$$

Now from A4

$$\begin{aligned} E(VMP_{mt^*}) - E(VMP_{mt}) &= E(w_{mt^*}) - E(VMP_{mt}) && (11) \\ &\geq E(w_{mt^*}) - w_{mt} && (12) \end{aligned}$$

where the second inequality uses A1. Similarly, from A4 and A2

$$\begin{aligned} E(VMP_{st^*}) - E(VMP_{st}) &= E(w_{st^*}) - E(VMP_{st}) && (13) \\ &\leq E(w_{st^*}) - w_{st} && (14) \end{aligned}$$

Combining (8), (10), (12) and (14) gives (5). ■

The proposition is quite powerful. The prediction of higher future wage growth depends only on equal current wages and equal expected productivity growth. Therefore, in bringing the theory to the data, we need control only for factors such as experience that we expect to predict future productivity growth although in practice we will control for other observables.

1.2 Three multiperiod adverse selection models

As discussed, multiperiod adverse selection models become unwieldy. To address this problem, we argue in the previous section that the regularities of two-period adverse selection models are shared by multiperiod models. We then show that a 'general' multiperiod model with these regularities yields our prediction. In this section, we present three specific multiperiod adverse selection models that use different approaches to solve the problem of untractability. While each is problematic, we hope that the models and intuitive arguments will help convince readers of the generality of our argument.

We present the results of Greenwald's three-period model, which has the property that there is no period with more than two histories and only four different equilibrium wages. But, of course, it is only three periods. Our second model allows for an arbitrarily large, possibly infinite, number of periods but allows for only two types of worker. This model assumes that as soon as two firms have observed a worker's type, they Bertrand compete for her services. In this way, firms need only know the timing of a worker's first move. Finally, in the third model, we present the results of Cavounidis and Lang (2015) who restrict the market's memory to a single period so that wage offers are conditioned only on whether the worker moved last period. In contrast to the general argument we made earlier, in this model convergence to the truth fails because the market also forgets, and thus information is lost. Nevertheless, our principal prediction holds.

1.2.1 Greenwald (1986) three-period model

Greenwald develops a three-period model with a continuum of types. Workers are hired competitively in period 1. At the end of the period, firms observe the productivity of the workers they hired. Raiding firms make offers. Incumbent firms may make counteroffers. Most workers accept the offer that maximizes their discounted earnings over the next two periods, but a fraction quits randomly. All firms observe which workers have remained with the incumbent employer and which workers have moved. At the end of the second period, firms that have successfully raided other firms observe the productivity of those raided workers. The prior employer either forgets the worker's productivity or cannot make her an offer. All other firms make offers. The incumbent employer can make a counteroffer. Workers accept the higher offer except for a fraction that quits randomly.

Greenwald proves the following. In the third period, the wage received by a worker who remained with her original employer for two periods is independent of whether she stays or quits in the third period. We denote this wage by w_3^s where the s denotes staying with the first-period employer in the second period. Similarly, the wage received by a worker who changed jobs between periods 1 and 2 is independent of whether she remains with her new employer or quits again in period 3. We denote this wage by w_3^q.

Greenwald further shows that

$$w_3^s \geq w_3^q \tag{15}$$

and that

$$w_2^q \geq w_2^s \tag{16}$$

where these refer to wages paid to quitters and stayers in the third and second periods. The inequalities are strict in all but the case where workers are completely myopic.

It follows that

$$w_3^s - w_2^s > w_3^q - w_2^q. \tag{17}$$

Wages rise faster following a stay than following a quit. As Greenwald explains, quitters must be compensated in the second period for the adverse signal that quitting provides while stayers are rewarded in the third period for the positive signal.

1.2.2 Bertrand competition between informed employers

We assume that there are two types of worker with productivities v_h and v_l, where $v_h > v_l$. Workers are hired competitively at the beginning of the first period. At the end of this period, the incumbent firm learns the worker's type. Raiding firms make offers. Incumbent firms make counteroffers. If at any time t, a worker has never left the original incumbent firm, raiders observe this fact and use this information when making their offers to which the incumbent firm may counteroffer. If a worker leaves in any period, at the end of the next period both the original incumbent and the successful raider know the worker's type. They Bertrand compete for her services and therefore offer her v_i. Other firms do not make offers.[8] Workers observe the outside offers and counteroffer and choose the one that maximizes the present value of their lifetime earnings.

We show in the appendix that the following is the unique equilibrium: Low-productivity workers exit their initial firm in finite time. The path of raiding offers is given by

$$\begin{aligned} q_{t+1} &= q_t(1+r) - rv_l \; if \;\; q_t(1+r) - rv_l < v_h \\ q_t &= v_h, \; otherwise. \end{aligned} \tag{18}$$

where q_t denotes the wage received by quitters in period t and r is the discount rate. In other words, raiding offers rise steadily because high-productivity workers become an increasing share of the pool of incumbent workers. This continues until the raiding offer would exceed the productivity of the high-productivity types, at which point only high-productivity types remain with the incumbent and raiders offer v_h.

In every period after the initial hire the incumbent firm offers v_l to its

[8] Or could always offer v_l.

low-productivity workers. It offers high-productivity workers

$$
\begin{aligned}
s_t &= \frac{v_h + v_l r}{1+r},\ if\ \ t < T \\
s_T &= q_T \\
s_t &= v_h,\ if\ \ t > T.
\end{aligned}
\tag{19}
$$

where T denotes the last period in which a low-productivity worker quits. Thus, in T and all prior periods it offers a constant wage $s < v_h$ to its high-productivity workers. In period T, in which the last low-productivity worker quits, we have $s_{t<T} < s_T < v_h$. Thereafter high-productivity workers are paid v_h.

The model confirms our general intuition. The assumption of Bertrand competition means that successful raiders earn no subsequent rents. Consequently, quitters are paid their expected productivity and in all subsequent periods are paid their actual productivity. Consequently, their average wages neither rise nor fall subsequent to moving. In most periods, workers who continue not to move also receive the same average wage, but this wage is below the productivity of the high-productivity workers. But workers who stayed and now quit are paid their average VMP which exceeds their average wage with the incumbent. In addition, in two periods (in a knife-edge case, one period), the wage paid by the incumbent to high-productivity workers also rises.

1.2.3 Steady-state with limited memory

Cavounidis and Lang (2015) derive the steady-state equilibrium of a model with two types of workers (high and low productivity). The market only observes whether the worker quit or remained with her previous firm last period. The assumption of limited memory makes the model tractable. They assume that a small fraction of workers quits exogenously and that the incumbent firm and worker Nash bargain over the wage with the outside offer as the worker's threat point. There is symmetric information between the incumbent and the worker because incumbent firms learn the worker's productivity at the end of the first period, before engaging in bargaining.

They show that low-productivity workers who remain with their firm earn the lowest equilibrium wage but receive a compensating differential when they quit. Subsequent to quitting, these workers take a wage cut regardless of whether they remain with their new firm or quit again. High-productivity workers who stayed with their firm take a pay cut if they exogenously leave. Subsequent to leaving they take a further pay cut if they quit again and may or may not take a second pay cut if they remain with their new firm. This depends on the precise parameters of the model. On the one hand, the negative signal of having moved lowers their outside option. On the other, the firm now knows that the worker is good and is therefore willing to bargain to a higher wage. They show the net result is that, on average, wages rise following a period in which the worker stays with the firm and fall following a period in which they quit.

2 Data

We test the model's main prediction using the National Longitudinal Survey of Youth 1979, a nationally representative sample of 12,686 individuals who were 14-22 years old at the time they were first surveyed in 1979, with oversamples of blacks, Hispanics and poor whites. These individuals are surveyed annually through 1994, and biennially afterwards. We exclude the sample of individuals who were serving in the military at the time of the sample selection in 1978.

Testing the model's prediction requires the following variables for an observation in period $t:$ wage (in $t,\ t-t'$) , whether the respondent is at a new job (in $t-t'$), total job mobility (through $t-t'-1$), and total weeks of potential experience (in $t-t'$), for $t'=1,4,10$. The lags of these variables are created using the value of the variable at the time of the previous interviews.

We define potential experience as the number of weeks between the current period and the individual's long-term transition to the labor market. Following Farber and Gibbons (1996), we assume that an individual has made a long-term transition to the labor market when he or she has spent three consecutive years primarily working, after at least a year spent not primarily working. An individual is defined to be primarily working in a given year if he or she spends more than 26 weeks working, and averages more than 30 hours per week over the working weeks. After 1993, individuals were interviewed every two years, instead of every year. Thus, it is not possible to identify long-term transitions to the labor market using the already constructed variables indicating weeks and hours worked since the last interview.

To identify long-term transitions to the labor market, we identify 52-week periods starting in every week. For each individual, we calculate the weeks and hours worked over these 52-week periods using the weekly arrays from the NLSY. We assume that an individual has made a long-term transition to the labor market when there are three consecutive 52-week periods in which the individual is primarily working, following a 52-week period in which the individual is not primarily working. The week in which the individual makes a long-term transition to the labor market is denoted as the week since January 1, 1978.[9] To obtain weeks of potential experience, we calculate the number of weeks between the interview date and January 1, 1978, and then subtract the week number at which the individual made a long-term transition to the labor market.

We focus on the wage, hours worked, and transitions from the current job.[10] We are able to identify whether a worker changes jobs using the NLSY-constructed variable tracking employers across interviews. A worker is coded as being at a new job if the current employer is different from each of the five most recent employers listed at the last interview. A worker is coded as staying at the current job if the current employer is the same as the current employer

[9] Due to the computational intensity of this procedure, it was performed over the high-performance shared computing cluster operated by Boston University.

[10] In some survey years the CPS job is identified in addition to the five most recent jobs. However, the CPS job is always identical to the most recent/current job (job #1)).

at the time of the last interview.[11] Workers are coded as neither movers nor stayers in the year they make their long-run transition to the labor market.

Controlling for total job mobility helps to ensure we compare observationally identical workers up to the period before the move. For individuals in a new job in $t-t'$ we control for the total number of jobs up until $t-t'-1$. The NLSY contains a measure of the total number of jobs an individual had ever reported at the time of each interview. A drawback of this measure is that it includes very part-time jobs that are held at the same time as the principal job. Mobility in the principal job is the relevant measure for our test.

An alternative measure of total job mobility is the number of times the individual is at a new current job. This is equivalent to adding the number of times $newjob = 1$ for each respondent. Because it only captures jobs at the time of the interview, this measure will likely underestimate the amount of mobility.[12] This is particularly problematic when individuals are only interviewed every two years. We estimate our specifications using this alternative measure for robustness (results with this alternative measure are shown in the appendix).

Both methods for measuring total mobility rely on the number of times the individual had been interviewed. We thus restrict the sample to individuals who have been interviewed in every year through t.

The empirical strategy, discussed in detail below, controls for the values of variables in $t-t'$ for $t' = 1, 4, 10$. Through 1994, for $t' = 1$ we define the lagged variable as the one-year lag because individuals are surveyed annually. However, starting in 1996 individuals are interviewed every two years. We define the one-period lag in 1996 as the value from 1994, since this is the date of the last interview. The two-period lag is 1993. Starting in 1998, we define

[11] This construction excludes respondents whose current employer differs from the current employer identified last period, but is the same as the third most recent employer last period. Despite a gap in employment, this employer presumably still has incumbent-like information about the employee and so is not treated as a new firm. This individual is coded as neither a mover nor a stayer, and so is dropped from the analysis.

[12] For example, an individual may have a different job at the time of the interview in May, 1980 than when interviewed in May, 1981. However, she may have had a different job from January, 1981 to April, 1981 and this job would not be captured.

the one-period lag as the value from two years ago, and the two-period lag as the value from four years ago. For $t' = 4$ and 10, we define the value in $t - t'$ as the value from four or ten years ago, respectively, for all survey years.[13] Controlling for year fixed effects mitigates concerns about different definitions of lags in different years.

The years of the observations in our sample range from 1981 to 2010. Because we define long-run transitions to the labor market as following a year of primarily not working, the earliest an individual could have entered the labor market is 1979. In the 1979 interview, which is the first interview, individuals are asked for weekly employment data from 1978. As mentioned, we do not identify whether the individual is at a new job until the individual's second year in the labor market, and thus the earliest such year is 1980. Because our specifications rely on whether the individual was at a new job in the last period, 1981 is the first year we observe individuals in the data. While we cannot identify whether the individual was at a new job in 1979, we can identify her wages since she had made her long-run transition to the labor market. This allows us to identify the second lag of wage in 1981.

We limit the sample in a way similar to Kahn (2013). We require that in periods t and $t - t'$ the individual is not self employed or employed in a family business, not enrolled in school, earns an hourly wage of at least one dollar and less than or equal to 500 dollars (in 1999 dollars), works at least 35 hours per week, and is currently working at the first listed job and not at the second through fifth listed jobs.[14] These restrictions result in a sample of 7,347 individuals with consecutive survey responses. There are 36,737 individual/year observations in which the individual had not moved in the previous period, and 14,118 individual/year observations in which the individual had moved to a new job in the previous period.

Table 1 shows that, unsurprisingly, there are differences between those who

[13] When $t' = 4$, values in $t - t' - 1$ are those from five years ago for respondents through 1998, and six years ago for respondents starting in 2000.

[14] We require these conditions are also true in $t - t' - 1$ in the specifications controlling for wage in $t - t' - 1$ (described below). Wages are converted to 1999 dollars using the CPI-Urban series.

were in a new job last period and those who were not. Respondents in a new job last period had fewer weeks of total experience, lower wages this period and last period, more total jobs, and they were less likely to be white-collar workers.[15]

While it is not the focus of our study, for comparability to other studies, we estimate the contemporaneous effect of moving on wages. Topel and Ward (1992) find the contemporaneous effects of a job transition are largest when occuring in the first 2.5 years of experience. Similar to Topel and Ward (1992), we regress $ln(wage_t) - ln(wage_{t-1})$ on $newjob_t$. While Topel and Ward focus their analysis on white males, we include both white and nonwhite males and females, and include controls for male, Hispanic, and Black. We also control for total mobility up through t, weeks experience, and age, and implement the sample restrictions described above.

When limiting the sample to those with between 0 and 2.5 years of experience (0 and 130 weeks), the coefficient on $newjob_t$ is .048, statistically significant at the .01 level. Limiting the sample to those with less than five years of experience, the coefficient on $newjob_t$ is .02, and has a p-value of .032. When using the alternative definition of mobility, rather than the total jobs from NLSY, the coefficients are no longer statistically significant although the magnitudes are still positive (although less so). Limiting the sample to those with less than 7.5 years of experience and less than 10 years of experience, the coefficients on $newjob_t$ are not statistically significant. These results suggest consistency with the standard finding that at very early stages of the career, there is a positive, contemporaneous effect of moving.

[15] Similarly, we estimate the cross-section relation between current wage and a quartic in total jobs up through period t, weeks experience, and demographics (not lagged wages). We limit the sample in a similar way as the main regression studying one-period wage growth. The coefficients on total jobs are jointly significant, and the magnitudes suggest that greater mobility is associated with higher wages, up through approximately 15 jobs. The mean number of jobs is 7.8.

Table 1: Summary Statistics, by New Job Last Period

	New Job_{t-1}	
	0	1
Hourly Wage_t	17.08	14.05***
	[13.72]	[11.23]
Hourly Wage_{t-1}	16.37	13.09***
	[13.]	[10.53]
Weeks Experience_{t-1}	614.92	486.71***
	[375.6]	[373.45]
Total Jobs_{t-2}	7.41	8.74***
	[4.57]	[5.69]
White Collar_{t-2}	0.59	0.49***
	[.49]	[.5]
Blue Collar_{t-2}	0.4	0.43***
	[.49]	[.49]
Age in 1979	17.69	17.41***
	[2.29]	[2.27]
Male	0.58	0.58
	[.49]	[.49]
Black	0.12	0.13***
	[.33]	[.34]
Hispanic	0.06	0.06
	[.23]	[.23]
Observations	36,737	14,118

Notes: *** p<0.01, ** p<0.05, * p<0.1. Asterisks denote whether the difference in the average for movers and stayers is statistically significant. See text for variable definition and construction. The proportion of respondents who were white collar and blue collar does not add to one because some respondents have a value of zero for both white and blue collar (their occupation was defined as neither white nor blue collar, or it was not reported).

3 Empirical Strategy

Our theory refers to the wages in period t of individuals who have the same wage in period $t - t'$, but differ in whether they were at a new job in $t - t'$. Therefore, in principle we could test the adverse selection hypothesis by simply regressing future wages on a lagged wage and whether the worker moved in the period for which we measure the lagged wage (e.g. regress the wage in period t on the wage in period $t - t'$ and whether the worker had a new job in period $t - t'$). We are, however, concerned that the hypothesis should not be tested without consideration of other elements of the labor market, in particular that we expect less experienced workers to acquire human capital more rapidly and therefore to have faster wage growth. In addition, since the market's inference about the worker should depend on her entire work history, we allow the wage to depend on the number of prior moves. This will also allow us to control for the possibility that some occupations are simply higher turnover than others.

Thus, we begin by comparing individuals who were, and were not, at a new job in $t - t'$, and their wage growth from $t - t'$ to t. If wages or wage growth is sticky, then firms may have to wait more than one year after the hire before they are able to pay the mover less than the stayer. Further, models which involve match-specific quality or firm-specific capital may involve larger wage growth for the mover over time, but not in the first year after the move. As a result, we estimate the following regression for $t' = 1, 4, 10$, which includes year fixed effects, standard errors clustered at the individual level, and observations weighted by the sampling weights of the survey (normalized so that the sum of the weights across all years is equal to the total number of observations across all years):[16]

[16] Results from unweighted regressions are very similar. We also estimate the specifications with only heteroskedasticity-robust standard errors (without clustering standard errors at the individual-level). Results are in general quite similar. While the unclustered standard errors are larger in several specifications (mostly in the specifications looking at one- and ten-year wage growth), they do not yield differences in whether the coefficient is statistically significant at the 1%, 5%, or 10% significance levels.

$$Ln(Wage_{it}) = \beta_0 + \beta_1 NewJob_{i,t-t'} + \sum_{q=1}^{4}(\phi_q WeeksExperience^q_{i,t-t'} \quad (20)$$
$$+ \kappa_q(Ln(Wage_{i,t-t'}))^q + \lambda_q TotalJobs^q_{i,t-t'-1}) + X\delta + \gamma_t + \epsilon_{it}$$

Similarly, some types of workers may simply be on faster wage growth paths than others. If this is correlated with worker mobility, our results may be misleading. To better address this concern, we focus on specifications in which we also control for the wage in $t - t' - 1$. We thus estimate the specification:

$$Ln(Wage_{it}) = \beta_0 + \beta_1 NewJob_{i,t-t'} + \sum_{q=1}^{4}(\phi_q WeeksExperience^q_{i,t-t'} \quad (21)$$
$$+ \beta_3(Ln(Wage_{i,t-t'}))^q + \beta_4 TotalJobs^q_{i,t-t'-1}$$
$$+ \beta_5(Ln(Wage_{i,t-t'-1}))^q) + X\delta + \gamma_t + \epsilon_{it}$$

We test whether the results are heterogeneous by sex, potential experience, number of prior moves, and education level. An additional specification includes age as well as indicators for male, Hispanic, and Black as explanatory variables in the main specification.

Following Gibbons and Katz (1991), we also test whether adverse selection is more important for white-collar workers. These workers are less likely to be bound by collective bargaining agreements, giving their firms more discretion over worker mobility. We look at whether the worker was white collar in $t - t' - 1$, since this will affect whether the worker was at a new job in $t - t'$. Interestingly, we see in table 1 that white collar workers are less likely to be at a new job. We define white collar and blue collar based on Gibbons and Katz (1991).[17]

[17] Agricultural and private household workers are coded as neither blue nor white collar.

Finally, to determine whether adverse selection is more prominent among higher (or lower) wage workers, we interact $NewJob_{i,t-t'}$ with $Ln(Wage_{i,t-t'})$.

Because movers and stayers are different, and because the linearity assumptions underlying the regression model may be problematic, we present results from a nearest-neighbor matching procedure, implemented using the *nnmatch* routine in STATA. Each respondent who was at a new job in $t-t'$ is matched to one other respondent who was not at a new job in $t-t'$, based on the values of $WeeksExperience_{i,t-t'}$, $Ln(Wage_{i,t-t'})$, $TotalJobs_{i,t-t'-1}$, and year.[18] We specify exact matching for survey year.

The closest match is determined based on the following distance metric: $D = (|X_m - X_n|' S^{-1} |X_m - X_n|)^{1/2}$, where X is an $N \times K$ matrix of the matching variables and S is the $K \times K$ diagonal matrix of the inverse sample standard errors of the K matching variables. If there were two equally good matches then both were used. While each observation is matched to another, not every observation is itself used as a match. The procedure corrects for bias arising from differences in the covariates within a matched pair, using the adjustment suggested by Abadie and Imbens (2011). The procedure further allows for heteroskedastic standard errors by conducting a second matching process, among those in the same treatment group. This allows for a comparison of outcomes for observations with approximately the same values of the matching variables. This correction yields smaller standard errors, and so our main results show the more conservative unadjusted standard errors.[19]

4 Results

The first column of table 2 shows that if two workers made the same wage last period but one was at a new job while the other was not, the one-period wage

One individual listed armed forces as an occupation, which is coded as neither blue nor white collar. In addition, a number of respondents are coded as neither blue nor white collar because of missing occupational data. Details in the appendix.

[18] For robustness, we also implement the procedure matching on $Ln(Wage_{i,t-t'-1})$.

[19] Because of the large number of individuals in the data set, this matching routine is highly computationally intensive, and was run on a shared computing cluster.

growth of the mover is not statistically significantly different. The coefficient is a fairly precisely estimated 0. Column 2 shows this result does not change when controlling for demographic characteristics. Columns 3 and 4 show that when controlling for the second lag of wage, wages of the mover are approximately 1 log point larger than the wages of the worker who did not move, and we can reject the hypothesis that this coefficient, while small, equals 0. Columns (5) through (7) also show no evidence of adverse selection when restricting the sample to various subgroups, including males, blue-collar workers, and workers with less than or equal to the 25th percentile of $WeeksExperience_{t-1}$ (269 weeks, or approximately 5 years). Column (8) shows no evidence of adverse selection once allowing for heterogeneity based on the previous period's wage. Results from the matching estimation are similar to the principal results (column 9).[20] In all cases we can reject an adverse effect of more than one percent.

There is also no evidence of adverse selection when estimating the regression separately for individuals (and also only for males) with less than a high school diploma, a high school diploma, some college, and at least a bachelor's degree. While the effects including the second lag of wage are not significant for any group except those with a high school diploma, the coefficients are most negative for individuals with at least a bachelor's degree and males without a high school diploma (Appendix Table A1).[21]

Finally, we interact $NewJob_{i,t-t'}$ with the quartic in total job mobility. Appendix Table A4 shows these interaction terms are statistically significant, but the combined effect of being in a new job last period is zero or positive up through approximately 25 jobs (the mean number of jobs up through $t-2$ is 7.3).

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[20] The results from the principal specification are very similar when we use the alternative measure of job mobility, $TotalMoves_{t-2}$, and when we do not weight the observations by the sampling weights of the survey (not shown). When we implement the matching procedure also matching on the second lag of wage, the magnitude is slightly smaller than the regression estimate and is not statistically significant.

[21] These specifications are estimated both including and not including the second lag of wage, though restricting the sample to be the same in either specification.

Table 2: Relation between Job Mobility and One-Year Wage Growth

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	Matching
$NewJob_{t-1}$	-0.0003	-0.001	0.010**	0.010**	0.009	0.012*	0.015*	0.020	-0.0004
	[0.004]	[0.004]	[0.004]	[0.004]	[0.006]	[0.006]	[0.008]	[0.026]	[.004]
$NewJob_{t-1}$*Ln($Wage_{t-1}$)								-0.004	
								[0.010]	
Observations	50,855	50,855	40,514	40,514	22,804	18,221	10,128	40,514	50,855
R-squared	0.742	0.743	0.771	0.772	0.766	0.697	0.648	0.772	
Subgroup	All	All	All	All	Male	Blue $Collar_{t-2}$	$Experience_{t-1}$ ≤25th percentile	All	All
Include Demographics	No	Yes	No	Yes	Yes	Yes	Yes	Yes	
Include Ln($Wage_{t-2}$)	No	No	Yes	Yes	Yes	Yes	Yes	Yes	

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors, clustered at the individual level, in brackets. Observations weighted by the sampling weights of the survey. Each regression includes year fixed effects, and a quartic in $weeksexperience_{t-1}$, $Ln(Wage)_{t-1}$, and $TotalJobs_{t-2}$. Regressions including the second lag of wage also include a quartic in $Ln(Wage)_{t-2}$. Column (7) restricts the sample to individuals with weeks experience ≤ the 25th percentile of the distribution (269 weeks, or approximately 5 years). In the final column, observations who were in a new job last period are matched to observations not in a new job last period, based on weeks $experience_{t-1}$, $wage_{t-1}$, total $jobs_{t-2}$, and survey year. Exact matching was specified for survey year. The procedure uses the bias adjustment in Abadie and Imbens (2011). See text for details of sample construction, regression specifications, and matching procedure.

4.1 Longer-term Wage Growth

Table 3 also shows no evidence of adverse selection based on four-year wage growth. Column 1 shows that wages of workers who moved four years ago are approximately 1.2% higher than wages of workers who did not change jobs four years ago, a difference of about 0.3% per year. The effect does not change dramatically when controlling for demographics. Columns 3 and 4 show that when controlling for the fifth lag of wage, the effect increases slightly in magnitude. The effect appears smaller for males, larger for blue-collar workers, and similar for less-experienced workers, but all differences are small and statistically insignificant. Again, there is no evidence of adverse selection when allowing for heterogeneity by wage in $t - t'$. Results from the matching estimation are similar to the principal results (column 9).[22] Overall, the estimated effects, while precisely estimated, are negligible over a four-year period.

The interactions between $NewJob_{i,t-4}$ and $TotalJobs_{i,t-5}$ are jointly sta-

[22] When we match on the 5th lag of wage as well, the effect is larger (magnitude of .023) and more statistically significant than the regression estimate.

Table 3: Relation between Job Mobility and Four-Year Wage Growth

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	Matching
$NewJob_{t-4}$	0.012**	0.010*	0.016**	0.015**	0.011	0.020**	0.015	0.047	0.011*
	[0.005]	[0.005]	[0.006]	[0.006]	[0.009]	[0.009]	[0.013]	[0.034]	[.006]
$NewJob_{t-4}$*Ln($Wage_{t-4}$)								-0.013	
								[0.014]	
Observations	35,958	35,958	28,376	28,376	16,374	12,960	7,095	28,376	35,958
R-squared	0.652	0.655	0.691	0.693	0.683	0.587	0.505	0.693	
Subgroup	All	All	All	All	Male	Blue $Collar_{t-5}$	$Experience_{t-4}$ ≤25th percentile	All	All
Include Demographics	No	Yes	No	Yes	Yes	Yes	Yes	Yes	
Include Ln($Wage_{t-5}$)	No	No	Yes	Yes	Yes	Yes	Yes	Yes	

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors, clustered at the individual level, in brackets. Observations weighted by the sampling weights of the survey. Each regression includes year fixed effects, and a quartic in weeksexperience$_{t-4}$, Ln(Wage)$_{t-4}$, and TotalJobs$_{t-5}$. Regressions including the fifth lag of wage also include a quartic in Ln(Wage)$_{t-5}$. Column (7) restricts the sample to individuals with weeks experience ≤ the 25th percentile of the distribution (258 weeks, or approximately 5 years). In the final column, observations who were in a new job last period are matched to observations not in a new job last period, based on weeks experience$_{t-4}$, wage$_{t-4}$, total jobs$_{t-5}$, and survey year. Exact matching was specified for survey year. The procedure uses the bias adjustment in Abadie and Imbens (2011). See text for details of sample construction, regression specifications, and matching procedure.

tistically significant with $p = .06$ (Appendix Table A4). The combined effect of being at a new job is positive up through about 10 jobs (the mean number of jobs up through $t-5$ is approximately 7.2). There is no evidence of adverse selection when estimating a separate regression for each education group, or among males in each education group. However, while the coefficients are not statistically significant, the coefficients on $NewJob_{i,t-4}$ are smallest (and negative for males) for those with at least a bachelor's degree (Appendix Table A2).

Table 4 shows no evidence of adverse selection based on ten-year wage growth. Wages of workers who moved 10 years ago are approximately 3.5 to 4.5%, or roughly 0.4% per year, higher than wages of workers who did not change jobs ten years ago, depending on whether we control for the 11th lag of wage. Similar to the four-year results, the effects appear smaller among males, although here they are also smaller among blue-collar workers and the differences fall short of significance at conventional levels (for both groups effects are still positive and statistically significant). There is no statistically significant differential effect among workers with higher wages in $t-10$. Results

Table 4: Relation between Job Mobility and Ten-Year Wage Growth

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	Matching
$NewJob_{t-10}$	0.036***	0.033***	0.045***	0.043***	0.035**	0.038**	0.054**	0.111**	0.027***
	[0.009]	[0.009]	[0.011]	[0.011]	[0.014]	[0.015]	[0.021]	[0.053]	[.010]
$NewJob_{t-10}$*Ln($Wage_{t-10}$)								-0.027	
								[0.022]	
Observations	18,167	18,167	14,065	14,065	8,266	6,296	3,517	14,065	18,167
R-squared	0.526	0.532	0.569	0.572	0.562	0.450	0.379	0.573	
Subgroup	All	All	All	All	Male	Blue $Collar_{t-11}$	$Experience_{t-10}$ ≤25th percentile	All	All
Include Demographics	No	Yes	No	Yes	Yes	Yes	Yes	Yes	
Include Ln($Wage_{t-11}$)	No	No	Yes	Yes	Yes	Yes	Yes	Yes	

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors, clustered at the individual level, in brackets. Observations weighted by the sampling weights of the survey. Each regression includes year fixed effects, and a quartic in $weeksexperience_{t-10}$, $Ln(Wage)_{t-10}$, and $TotalJobs_{t-11}$. Regressions including the 11th lag of wage also include a quartic in $Ln(Wage)_{t-11}$. Column (7) restricts the sample to individuals with weeks experience ≤ the 25th percentile of the distribution (251 weeks, or approximately 5 years). In the final column, observations who were in a new job last period are matched to observations not in a new job last period, based on weeks $experience_{t-10}$, $wage_{t-10}$, total $jobs_{t-11}$, and survey year. Exact matching was specified for survey year. The procedure uses the bias adjustment in Abadie and Imbens (2011). See text for details of sample construction, regression specifications, and matching procedure.

from the matching estimation are similar to the principal results (column 9).[23]

The interactions between $NewJob_{i,t-10}$ and $TotalJobs_{i,t-11}$ are not statistically significant, though the magnitudes suggest positive effects across the distribution of total number of jobs (Appendix Table A4). There is also not any evidence of adverse selection among any of the education groups, or for the males in any of the education groups. The coefficients on $NewJob_{i,t-10}$ are large, positive, and statistically significant for those with a high school diploma and those with at least a bachelor's degree. The coefficients are much closer to zero and not statistically significant for those with less than a high school diploma and those with some college (Appendix Table A3).

5 Conclusion

We have shown that, in a broad class of models of adverse selection in the labor market, following a move workers should experience slower wage growth than otherwise observationally identical workers who do not change employers. We

[23] When we match on the 11th lag of wage as well, the effect is very similar to the regression estimate.

have also shown that there is little or no empirical support for this prediction, suggesting either that this form of adverse selection is less important in the labor market than other factors generating mobility or that a key auxiliary assumption is false.

The most plausible candidate among the auxiliary assumptions is that the raiding market is competitive. This, along with the assumption that firms earn quasi-rents on their private information, generates the prediction that newly hired workers are, on average, overpaid. It is difficult to make strong claims about the effects of adverse selection in a market with, for example, on-the-job search. This might depend on the bargaining model, search technology and/or other details of the model. At the same time, the tautology that adverse selection means that mobility is a bad signal suggests that the prediction may be quite robust.

We are therefore inclined to the conclusion that adverse selection is not a major driving force behind job turnover. Instead turnover is likely to be driven by improvements in match quality as workers move jobs or by the natural progression of careers.

Our prediction is a test for adverse selection in the labor market, and we find little empirical support for the prediction and thus for adverse selection. However, we note that this prediction is generated by two assumptions that are applicable to a wide range of competitive models, raiding firms make expected profit of zero and incumbents earn quasi-rents, and one which applies less generally but is still applicable in many other models, future productivity growth is independent of mobility.

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Theory Appendix

Bertrand competition between informed employers

Suppose there are two types, a fraction g of type h and a fraction $1-g$ of type l with productivity $v_l < v_h$. If a worker leaves the incumbent firm and joins another firm, his productivity is known to both firms who subsequently Bertrand compete and thus pay v_i. In case of a tie between the value of the best outside offer and the counteroffer, $h - type$ workers (except for random movers) remain with the incumbent firm. In equilibrium, all raiding firms will make the same offer. We assume that workers randomize among raiding firms. Therefore, for simplicity of exposition we will often refer to the set of raiding firms as *the* outside or raiding firm.

We use q_t to denote the wage received in period t by a worker who quits in that period and s_t to denote the wage received by an $h - type$ who stays with her incumbent firm and has not previously quit.

Proposition 3 *The incumbent firm offers a wage of v_l to all $l - type$ workers.*

Proof. If it offered a wage greater than v_l and some $l - type$ workers remained with the firm, it could increase profit by lowering the wage. If no $l - type$ workers remain with the firm in this case, we normalize the (meaningless) wage counteroffer to v_l. Suppose that the counteroffer were less than v_l. If $l - type$ workers are indifferent between quitting endogenously and remaining with the incumbent, the incumbent could increase profit by raising its wage infinitesimally. If $l - type$ workers strictly prefer to quit endogenously, it is costless to the incumbent to raise its wage to the lesser of v_l or a wage that makes the workers indifferent between staying and quitting. In the latter case, the previous argument applies. In the former, we normalize the counteroffer to v_l. If no $l - type$ worker quit endogenously, then raiders would offer the mean productivity in all periods. But in this case, $l - type$ workers would all prefer to quit immediately. ∎

Lemma 1 *No $h - type$ quits endogenously.*

Proof. If $s_t < v_h$, the incumbent can increase profit by raising its offer to any $s_t < v_h$ which retains $h - type$ workers. If $s_t = v_h$ is required to retain $h - types$, we assume that the incumbent makes this counteroffer. ■

Lemma 2 *If there is a last period in which an $l - type$ quits endogenously, all $l - types$ must quit in that period.*

Proof. Let t^* be the last such period. Then $q_{t^*+1} = q_t \; \forall t > t^*$. But then $l - types$ will strictly prefer to quit in period $t^* + 1$ than in any subsequent period. ■

Proposition 4 *All $l - type$ workers quit in finite time.*

Proof. If there is only one period in which l workers quit endogenously, then by the previous lemma, all workers quit that period. If l workers quit endogenously in multiple periods but there is no last period in which they quit, then we must have

$$v_l + \frac{q_{t'}}{(1+r)^{t'-t}} = q_t + \frac{v_l}{1+r} \tag{1}$$

since both quitters and stayers earn v_l in every period except t and t'. But for t' sufficiently large, this implies $q_{t'} > v_h$, a contradiction. ■

Proposition 5 *The path of raiding offers is given by*

$$\begin{aligned} q_{t+1} &= q_t (1+r) - r v_l \; if \;\; q_t (1+r) - r v_l < v_h \\ q_t &= v_h, \; otherwise. \end{aligned} \tag{2}$$

Proof. Set $t' = t + 1$ in (1) and rearrange terms. ■

Proposition 6 *The counteroffers to $h - types$, are given by*

$$\begin{aligned} s_t &= \frac{v_h + v_l r}{1+r}, \; if \;\; t < T \\ s_T &= q_T \\ s_t &= v_h, \; if \;\; t > T. \end{aligned} \tag{3}$$

Proof. We require that $h - types$ be just indifferent between quitting endogenously in periods t and $t+1$ so that (using the fact that all subsequent wages do not depend on the choice between these two strategies)

$$q_t + \frac{v_h}{1+r} = s_t + \frac{q_{t+1}}{1+r}. \tag{4}$$

Combining (4) with the expression for q_t gives (3) ■

Lemma 3 *There is exactly one period in which* $v_h > q_t \geq (v_h + v_l r) / (1+r)$ *.*

Proof. Replace q_t with $(v_h + v_l r) / (1+r)$ in (1) to get $q_{t+1} = v_h$. ■

Finally, let p_t be the proportion of all $l - type$ workers who quit, endogenously or exogenously in period t. We require that

$$\Sigma_{t=1}^{\infty} p_t = 1. \tag{5}$$

Lemma 4 *Equation (5) has a solution.*

Proof. Using the fact that a fraction d quits randomly each period, we have that the fraction of $h - types$ quitting each period is $d(1-d)^{t-1}$. Therefore for raiding firms to make zero profit, we require that

$$q_t = \frac{p_t v_l + d(1-d)^{t-1} v_h}{p_t + d(1-d)^{t-1}}. \tag{6}$$

Moreover, by consecutive substitution, we have

$$q_t = q_1 (1+r)^{t-1} - v_l \left((1+r)^{t-1} - 1\right). \tag{7}$$

Combining (6) and (7) gives

$$p_t = d(1-d)^{t-1} \frac{v_h - q_1 (1+r)^{t-1} + v_l \left((1+r)^{t-1} - 1\right)}{\left((1+r)^{t-1}\right)(q_1 - v_l)} \tag{8}$$

which is decreasing and continuous in q_1.

Next we need to show that Σp_t is continuous. This follows immediately if the number of periods in which $l-types$ quit is constant. Therefore consider a q_1 such that $l-types$ are just indifferent between quitting in period T and $T+1$ when they would receive $q_{T+1} = v_h$. Since no $l-type$ actually quits in period $T+1$, if we increase q_0, there are still T periods in which $l-types$ quit. So we have continuity in that direction. If we reduce, q_0, the number of $l-types$ quitting in the first T periods increases continuously and the number quitting in the $T+1$ period increases continuously from 0. Therefore Σp_t is continuous in q_1.

Finally let $q_1 \downarrow v_l$. Then $p_t \rightarrow \infty$. While if $q_1 = v_h$

$$p_t = d\,(1-d)^{t-1}\,\frac{1-(1+r)^{t-1}}{(1+r)^{t-1}} \tag{9}$$

which equals d if $t=1$ and is negative for $t>1$ which ensures that $\Sigma p_t < 1$. ■

Combining the various lemmas and propositions gives the principal result:

Theorem 1 *There exists a unique equilibrium in which raiding offers are given by (2), the incumbent counteroffers to $l-types$ with v_l and to $h-types$ according to (3) and in which all $l-types$ quit in finite time.*

To see that this example satisfies our general claim about the wages of stayers and quitters, note that quitters, on average, receive their expected VMP in both the period in which they quit and in the next period. Stayers are, on average, paid less than their VMP and thus, on average, receive a wage increase in the period that they quit. In addition, $h-types$ receive wage increase if they stay in the period in which the wage jumps to v_h and, except in a knife-edge case in the preceding period.

Data Appendix

Blue- and White-Collar Workers

Up until 2000, the 1970 Census occupation codes are used, and we define the following as white collar: managers, officials, and proprietors; professional, technical, and kindred; clerical and kindred; sales workers. The following are defined as blue collar: craftsmen, foremen, and kindred; operatives and kindred; laborers, except farm; service workers, except private household. Starting in 2002, the 2000 Census occupation codes are used. The following codes are defined as white collar: management; business and financial operations; computer and mathematical; architecture and engineering; legal; education, training, and library; arts, design, entertainment, sports, media (except equipment workers); healthcare practitioners and technical; sales and related; office and administrative support; life, physical, and social sciences; community and social services. The following are defined as blue collar: healthcare support; protective service; food preparation and serving related; building and grounds cleaning and maintenance; personal care and service; construction and extraction; installation, repair, and maintenance; production; transportation and material moving; arts, design, entertainment, sports, and media (only equipment workers).

Matching Results

Of the 50,855 observations in the specifications analyzing one-year wage growth, 51% are never used as a match for another observation, and 36% are used more than zero times, but less than or equal to three times (weighted by the number of other observations used to match the same individual). All but three of the observations are matched to another observation in the exact survey year.

Of the 35,958 observations in the specifications analyzing four-year wage growth, 50% are never used as a match for another observation, and 36% are used one to three times (weighted by the number of other observations used to match the same individual). All but one of the observations is matched to another observation in the exact survey year.

Of the 18,167 observations in the specifications analyzing ten-year wage growth, 50% are never used as a match for another observation, and 35% are used one to three times (weighted by the number of other observations used to match the same individual). All of the observations were matched to another observation in the exact survey year.

Appendix Table A1: Relationship between Job Mobility and One-Year Wage Growth, by Education

	No High School		High School		Some College		At Least Bachelor's	
$NewJob_{t-1}$	0.003	-0.002	0.018***	0.019**	0.013	0.014	-0.001	-0.0001
	[0.012]	[0.014]	[0.006]	[0.008]	[0.010]	[0.014]	[0.009]	[0.013]
Observations	4,075	2,966	18,392	10,569	8,639	4,000	9,403	5,267
R-squared	0.591	0.565	0.700	0.677	0.695	0.671	0.750	0.745
Subgroup	All	Males	All	Males	All	Males	All	Males
Include Demographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Include $Ln(Wage_{t-2})$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Robust standard errors, clustered at the individual level, in brackets. Observations weighted by the sampling weights of the survey. Each regression includes year fixed effects, and a quartic in $weeksexperience_{t-1}$, $Ln(Wage)_{t-1}$, $TotalJobs_{t-2}$, and a quartic in $Ln(Wage)_{t-2}$. See text for details of sample construction and regression specifications.

Appendix Table A2: Relationship between Job Mobility and Four-Year Wage Growth, by Education

	No High School		High School		Some College		At Least Bachelor's	
$NewJob_{t-4}$	0.026	0.017	0.017*	0.018	0.023	0.013	0.004	-0.003
	[0.019]	[0.024]	[0.009]	[0.012]	[0.015]	[0.024]	[0.013]	[0.018]
Observations	2,619	1,948	13,067	7,740	6,173	2,910	6,513	3,775
R-squared	0.454	0.400	0.579	0.551	0.606	0.551	0.663	0.658
Subgroup	All	Males	All	Males	All	Males	All	Males
Include Demographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Include $Ln(Wage_{t-5})$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors, clustered at the individual level, in brackets. Observations weighted by the sampling weights of the survey. Each regression includes year fixed effects, and a quartic in $weeksexperience_{t-4}$, $Ln(Wage)_{t-4}$, $TotalJobs_{t-5}$, and a quartic in $Ln(Wage)_{t-5}$. See text for details of sample construction and regression specifications.

Appendix Table A3: Relationship between Job Mobility and Ten-Year Wage Growth, by Education

	No High School		High School		Some College		At Least Bachelor's	
$NewJob_{t-10}$	0.018	0.002	0.044***	0.048**	0.017	0.008	0.048**	0.035
	[0.030]	[0.033]	[0.015]	[0.019]	[0.020]	[0.026]	[0.021]	[0.028]
Observations	1,017	765	6,526	3,960	3,159	1,533	3,362	2,007
R-squared	0.389	0.339	0.400	0.383	0.487	0.421	0.541	0.491
Subgroup	All	Males	All	Males	All	Males	All	Males
Include Demographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Include $Ln(Wage_{t-11})$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors, clustered at the individual level, in brackets. Observations weighted by the sampling weights of the survey. Each regression includes year fixed effects, and a quartic in $weeksexperience_{t-10}$, $Ln(Wage)_{t-10}$, $TotalJobs_{t-11}$, and a quartic in $Ln(Wage)_{t-11}$. See text for details of sample construction and regression specifications.

Appendix Table A4: Relationship between Job Mobility and Wage Growth, by Number of Prior Jobs

		One Year	Four Year	Ten Year
(1)	$NewJob_{t-t'}$	0.074***	0.081**	0.062
		[0.024]	[0.036]	[0.060]
(2)	$NewJob_{t-t'}$*$TotalJobs_{t-t'-1}$	-0.020**	-0.016	0.002
		[0.010]	[0.015]	[0.027]
(3)	$NewJob_{t-t'}$*$(TotalJobs_{t-t'-1})^2$	0.002	0.001	-0.001
		[0.001]	[0.002]	[0.004]
(4)	$NewJob_{t-t'}$*$(TotalJobs_{t-t'-1})^3$	-2.90E-07	-0.00004	0.00004
		[2.84e-7]	[0.00009]	[0.0002]
(5)	$NewJob_{t-t'}$*$(TotalJobs_{t-t'-1})^4$	9.79E-10	5.02E-07	-2.54E-07
		[9.2e-10]	[1.35e-6]	[3.29e-6]
	Test (2) through (5)	0.043	0.062	0.532
	Test (1) through (5)	0.005	0.007	0.001
	Observations	40,514	28,376	14,065
	R-squared	0.772	0.693	0.573
	Include Demographics	Yes	Yes	Yes
	Include $Ln(Wage_{t-t'-1})$	Yes	Yes	Yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors, clustered at the individual level, in brackets. Observations weighted by the sampling weights of the survey. Each regression includes year fixed effects, and a quartic in $weeksexperience_{t-t'}$, $Ln(Wage)_{t-t'}$, $Ln(Wage)_{t-t'-1}$, and $TotalJobs_{t-t'-1}$. See text for details of sample construction and regression specifications.

Appendix Table A5: Relationship between Job Mobility and One-Year Wage Growth, Using Total Moves Definition of Mobility

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$NewJob_{t-1}$	0.002	0.000	0.011**	0.010**	0.009	0.011*	0.015*	0.023
	[0.004]	[0.004]	[0.004]	[0.004]	[0.006]	[0.006]	[0.008]	[0.026]
$NewJob_{t-1}$*$Ln(Wage_{t-1})$								-0.005
								[0.010]
Observations	50,859	50,859	40,516	40,516	22,805	18,222	10,129	40,516
R-squared	0.742	0.743	0.771	0.772	0.766	0.697	0.648	0.772
Subgroup	All	All	All	All	Male	Blue $Collar_{t-2}$	$Experience_{t-1}$ ≤25th percentile	All
Include Demographics	No	Yes	No	Yes	Yes	Yes	Yes	Yes
Include $Ln(Wage_{t-2})$	No	No	Yes	Yes	Yes	Yes	Yes	Yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors, clustered at the individual level, in brackets. Observations weighted by the sampling weights of the survey. These specifications include the alternative measure of mobility in period *t* , constructed by adding the number of times *newjob* is equal to one up through *t.* See text for details. Each regression includes year fixed effects, and a quartic in $weeksexperience_{t-1}$, $Ln(Wage)_{t-1}$, and $TotalMoves_{t-2}$. Regressions including the second lag of wage also include a quartic in $Ln(Wage)_{t-2}$. Column (7) restricts the sample to individuals with weeks experience ≤ the 25th percentile of the distribution (269 weeks, or approximately 5 years). See text for details of sample construction and regression specifications.

Appendix Table A6: Relationship between Job Mobility and Four-Year Wage Growth, Using Total Moves Definition of Mobility

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$NewJob_{t-4}$	0.015***	0.013**	0.017***	0.016**	0.010	0.018*	0.019	0.050
	[0.005]	[0.005]	[0.006]	[0.006]	[0.009]	[0.009]	[0.013]	[0.034]
$NewJob_{t-4}$*Ln($Wage_{t-4}$)								-0.014
								[0.014]
Observations	35,960	35,960	28,376	28,376	16,374	12,960	7,095	28,376
R-squared	0.652	0.654	0.691	0.693	0.683	0.587	0.504	0.693
Subgroup	All	All	All	All	Male	Blue $Collar_{t-5}$	$Experience_{t-4}$ ≤25th percentile	All
Include Demographics	No	Yes	No	Yes	Yes	Yes	Yes	Yes
Include Ln($Wage_{t-5}$)	No	No	Yes	Yes	Yes	Yes	Yes	Yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors, clustered at the individual level, in brackets. Observations weighted by the sampling weights of the survey. These specifications include the alternative measure of mobility in period *t*, constructed by adding the number of times *newjob* is equal to one up through *t*. See text for details. Each regression includes year fixed effects, and a quartic in $weeksexperience_{t-4}$, $Ln(Wage)_{t-4}$, and $TotalMoves_{t-5}$. Regressions including the fifth lag of wage also include a quartic in $Ln(Wage)_{t-5}$. Column (7) restricts the sample to individuals with weeks experience ≤ the 25th percentile of the distribution (258 weeks, or approximately 5 years). See text for details of sample construction and regression specifications.

Appendix Table A7: Relationship between Job Mobility and Ten-Year Wage Growth, Using Total Moves Definition of Mobility

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$NewJob_{t-10}$	0.038***	0.035***	0.046***	0.043***	0.034**	0.035**	0.056***	0.120**
	[0.009]	[0.009]	[0.010]	[0.010]	[0.014]	[0.015]	[0.021]	[0.054]
$NewJob_{t-10}$*Ln($Wage_{t-10}$)								-0.030
								[0.022]
Observations	18,169	18,169	14,065	14,065	8,266	6,296	3,517	14,065
R-squared	0.525	0.531	0.569	0.572	0.561	0.451	0.379	0.572
Subgroup	All	All	All	All	Male	Blue $Collar_{t-11}$	$Experience_{t-10}$ ≤25th percentile	All
Include Demographics	No	Yes	No	Yes	Yes	Yes	Yes	Yes
Include Ln($Wage_{t-11}$)	No	No	Yes	Yes	Yes	Yes	Yes	Yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors, clustered at the individual level, in brackets. Observations weighted by the sampling weights of the survey. These specifications include the alternative measure of mobility in period *t*, constructed by adding the number of times *newjob* is equal to one up through *t*. See text for details. Each regression includes year fixed effects, and a quartic in $weeksexperience_{t-10}$, $Ln(Wage)_{t-10}$, and $TotalMoves_{t-11}$. Regressions including the 11th lag of wage also include a quartic in $Ln(Wage)_{t-11}$. Column (7) restricts the sample to individuals with weeks experience ≤ the 25th percentile of the distribution (251 weeks, or approximately 5 years). See text for details of sample construction and regression specifications.