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# Dynamic Inefficiencies in an Employment-Based Health Insurance System: Theory and Evidence†

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*We investigate the effects of the institutional settings of the US health care system on individuals' life-cycle medical expenditures. Health is a form of general human capital; labor turnover and labor-market frictions prevent an employer-employee pair from capturing the entire surplus from investment in an employee's health. Thus, the pair underinvests in health during working years, thereby increasing medical expenditures during retirement. We provide empirical evidence consistent with the comparative statics predictions of our model using the Medical Expenditure Panel Survey (MEPS) and the Health and Retirement Study (HRS). Our estimates suggest significant inefficiencies in health investment in the United States. (JEL* D14, D91, G22, I11, J32)

The United States is unique among industrialized nations in that it lacks a national health insurance system, and a private, employment-based system provides insurance to most of the working-age population. In this paper, we investigate the effects of the institutional settings of the US health care system on individuals' life-cycle medical expenditures. The core idea of this paper is that, in labor markets with frictions, an employment-based system can lead to inefficiently low investment in employees' health because part of the returns from the current health investment accrues to future employers.

Our premise is that health is a form of general human capital (Gary Becker 1964) and that health investment—medical expenditures, in particular—determines the evolution of health stock (Michael Grossman 1972). Hence, like all other forms of human capital, health increases labor productivity, thereby affecting the surplus generated in the employment relationship. Current health expenditures, therefore, are an investment that affects the surpluses of *both* current and future employment relationships. We embed this link between health investment and employment sur-

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plus in a frictional labor-market model, as in Daron Acemoglu and Jörn-Steffen Pischke (1999), and derive the implications of employee turnover on the employer-employee pair's incentives to invest in the employee's health.[1]

We show that employee turnover leads to an inefficiently low level of investment in employees' health and that the investment is lower and the inefficiencies larger when employee turnover is higher. The reason is that labor-market frictions imply that future employers capture part of the surplus generated from the current investment in the employee's health. Hence, the employer-employee pair does not internalize the full social surplus created by the current investment in the employee's health. As a result, the pair underinvests in health capital. Further, we show that this inefficiently low level of medical expenditures during the working years increases medical expenditures during retirement, possibly increasing overall expenditures.

We provide extensive empirical evidence consistent with the predictions of the model using two datasets, the Medical Expenditure Panel Survey (MEPS) and the Health and Retirement Study (HRS). Our empirical model is designed to deal explicitly with two issues that may make it difficult to identify the effect of job turnover on health investment. The first is *selection*: workers may select into different jobs due to unobserved characteristics, such as ability, discount factor, risk aversion, etc., that could potentially be correlated with both their job turnover and health investment. The second is *reverse causality*: workers' health outcomes and health expenditures could affect their job turnover. We deal with these issues using panel data to control for fixed and persistent unobservables that could affect selection into different jobs, along with demand-side instruments, e.g., plant closures, that arguably are not affected by reverse causality.[2]

We find that workers with shorter job tenures spend less on health care. We find a stark reversal of expenditures among the elderly, however, in that retirees who had longer preretirement job tenures spend less on health care. The magnitude of our results is considerable: workers whose log job tenures are one standard deviation longer have medical expenditures about \$500 higher per year; however, individuals over 65 whose tenure at their main pre-retirement job is one standard deviation longer spend about \$4,160 less per year on health care. Using these estimates, we can perform a back-of-the-envelope calculation to compare the lifetime medical expenditures of two workers whose only difference is their job tenures. Suppose that both individuals work 45 years and then retire for 15 years before dying, but the first individual's job tenure is one standard deviation longer than the second individual's. According to our estimates, during their working years, the first individual spent approximately \$22,250 more on health care than the second individual did; however, during retirement, the first individual's health expenditures would be approximately \$62,500 lower than the second individual's. The first individual's total lifetime health expenditures are about \$40,000 less than the second individual's. This calculation suggests that one additional dollar of health expenditures

[1] Any health expenditure that affects an individual's future health conceptually falls under our definition of health investment. Examples of such health investment include any preventive care, such as cancer screening, cholesterol checks, diabetes management, etc. Moreover, to the extent that not completely recovering from illnesses has long-lasting negative health effects, most, if not all, curative-care expenditures are health investments, as well.

[2] In online Appendix C, we present an alternative empirical approach that uses a measure of the importance of specific skills in each industry as a proxy for an industry's labor-turnover rate. The results are qualitatively similar.

during one's working years may lead to approximately 2.8 dollars of savings in retirement. Obviously, our calculation is very rough, as it does not incorporate discounting, does not adjust for the inflation of medical prices, and does not adjust for differences in quality of life and life expectancy. Nonetheless, it suggests that the dynamic externalities highlighted in our model can be substantial and may account, in part, for the high overall medical expenditures in the United States.[3]

This paper makes several contributions. First, it sheds light on the incentives generated by the employment-based health insurance system. We wish to emphasize at the outset that our paper is *not* about health insurance per se. Rather, we investigate how the health insurance system affects incentives to invest in health. By focusing on health investment, we do not tackle the difficult incidence issue of how much of the health insurance premium is actually paid for by the firms and by the workers (Jonathan Gruber 1994). Instead, we focus on how an employment-based system fails to internalize the entire surplus generated by health investment, leading to dynamic inefficiencies.

Second, the paper suggests that different institutional arrangements of the health care system can lead to different life-cycle dynamics of health expenditures. Our analysis indicates that in the United States, because of job turnover, the increase in expenditures over the life cycle is steeper when the parties—i.e., the employer-employee pair—appropriate a smaller share of the entire surplus generated by their health investment. Thus, our paper suggests that an employment-based system, as compared to a national health insurance system, may steepen the increase of health expenditures over an individual's life cycle.[4] Moreover, by not internalizing the full long-term benefits of health investment, an employment-based health system can also increase the overall expenditure level.

Third, taking the view that health is a form of general human capital, our paper also serves as an empirical analysis of how firms and workers invest in general human capital. In fact, we believe that health expenditures are particularly suited to studying how firms and workers jointly determine the *level* of general human capital investment. The reason is that health expenditures are typically well recorded, as most health investment is provided by third-party medical professionals with well-documented charges. In contrast, for almost all other investments in general human capital, it is quite difficult to obtain a quantitative measure of total costs and each party's contribution.[5]

The remainder of the paper is structured as follows. Section I reviews the related literature. Section II presents a simple model and derives its testable implications. Section III describes the datasets. The main empirical analysis is performed in Section IV. Section V provides additional evidence and presents robustness checks.

[3] The United States spends more than twice as much on health care as a fraction of GDP as other developed countries. For example, in 2005, the United States and the United Kingdom spent about 17 and 8 percent of their GDP, respectively, on health care (Laurence Kotlikoff and Christian Hagist 2005).

[4] Systematic comparisons of the dynamics of individuals' total health expenditures over the life cycle across different countries are limited. One related piece of evidence is in Kotlikoff and Hagist (2005), which focuses only on public health expenditures. They report that the ratio of per capita health expenditures of the 65–69 age group relative to that of the 50–64 age group is approximately five times higher in the United States than in nine other OECD countries.

[5] In these situations, it is often the case that only firms' general training expenditures may be recorded, while workers' contribution to general investment is unobserved.

Section VI discusses several alternative hypotheses. Section VII concludes. The Appendix provides more detailed information on the methodology developed by Manuel Arellano and Stephen Bond (1991) that we employ in the empirical analysis of Section IV. The online appendices collect additional results, including an extension of the model that accommodates firm-specific capital, along with an alternative empirical strategy that obtains similar qualitative and quantitative results. We discuss some of the results reported in these online appendices in the main text.

## I. Related Literature

The economic hypothesis that lies at the heart of our paper is that medical expenditures improve health, and healthy workers have higher productivity. The literature on this subject is vast, and, using different methods and different data, most papers share the findings that medical expenditures improve health and that healthier individuals are more productive. For a thorough survey on the relationships among medical expenditure, health, and productivity, see Emile Tompa (2002).

The paper is related to several strands of the literature. The most closely related papers are those that focus on dynamic inefficiencies in markets with frictions—labor and insurance markets, in particular. Specifically, one of the most celebrated results in the classical theory of human capital is that in a frictionless and competitive labor market, workers capture all the returns from their general human capital investments (Becker 1962, 1964). Thus, workers pay for the entire costs of general human capital investments, as they obtain the full return from them and invest the efficient amount. The empirical observation that firms often pay for general training of their employees—in contrast to the predictions of the classical human capital theory—has stimulated a few recent theoretical explanations. Acemoglu and Pischke (1998, 1999) show that, when labor-market frictions lead to "wage compression," firms may pay for investments in the general skills of their employees.[6, 7] The compression in the wage structure transforms "technologically" general skills into de facto "specific" skills, thus providing firms with incentives to invest in their workers' general skills. Even though Acemoglu and Pischke's theoretical models also yield testable predictions about the *level* of general human capital investment, most of the literature has focused exclusively on why firms *share* the costs of general training.

Similarly, a few papers investigate dynamic inefficiencies in insurance markets (Igal Hendel and Alessandro Lizzeri 2003; Keith Crocker and John Moran 2003; Amy Finkelstein, Kathleen McGarry, and Amir Sufi 2005; Bradley Herring 2010; Randall Cebul et al. 2011). Hendel and Lizzeri (2003), Crocker and Moran (2003), and Finkelstein, McGarry, and Sufi (2005) consider a different inefficiency from the one that we highlight. In particular, these papers suggest that inefficient risk-sharing arises when parties do not commit to long-term insurance contracts since short-term contracts cannot insure the reclassification risk arising from a change in insurees'

[6] Acemoglu and Pischke (1998, 1999) consider many potential forms of market frictions, including search friction, asymmetric information, complementarity between general and specific skills, etc.

[7] Recent papers by Felipe Balmaceda (2005) and Anke Kessler and Christoph Lülfesmann (2006) show that, under some surplus-sharing rules, specific and general human capital endogenously interact. Thus, even if the labor market is competitive, an employer may choose to contribute to workers' general training.

risk type.[8] To some extent, the dynamic inefficiency in our analysis is also related to the inability of workers to commit to long-term employment with the firm. Closer to our paper is Crocker and Moran (2003), which argues that workers in industries with higher specific-skill requirements are more committed to their firms, thereby allowing their employers to provide more complete *insurance* of health risks. In contrast, we focus on health investment and health outcomes, and on the trade-off between the short-term costs and the expected long-term gains of current health expenditures.

Nancy Beaulieu et al. (2007), Herring (2010), and Cebul et al. (2011) consider a type of inefficiency similar to the one on which we focus. In an interesting study of diabetes management, Beaulieu et al. (2007) find that improved diabetes care has economic benefits for health plans, as well as valuable benefits for diabetics. However, some of the long-term savings from good care management are not realized because plan turnover limits the health plan's ability to privately capture the benefits from its investments. Herring (2010) argues that insurees' turnover may reduce an insurer's incentives to provide the socially optimal level of preventive care. Using data from the Community Tracking Study's Household Survey, Herring finds that insurers' turnover has a negative effect on the utilization of preventive services. Cebul et al. (2011) study the effect of search frictions in the market for employer-based health insurance and make the point that frictions in insurance markets may reduce incentives to invest in future health. While clearly complementary, there are several key differences between our paper and those of Beaulieu et al. (2007), Herring (2010), and Cebul et al. (2011). First, we focus on employees' turnover rates, while these other papers focus on enrollees' turnover among insurance companies. Employers and workers enjoy most of the costs and benefits of medical expenditures, so we believe it is appropriate to focus on them. Moreover, almost half of all firms and more than 80 percent of firms with more than 5,000 employees are self-insured (Donald Barr 2007, p. 84). For these self-insured firms, insurers only administer the claims for the firms. Second, we examine the dynamics of medical expenditures over an individual's life cycle. Specifically, we investigate how retirees' medical expenditures and health status are related to their turnover rates prior to retirement. This allows us to understand the order of magnitude of the dynamic externality on which we focus.

The paper is also related to the literature on the interactions between health care markets and labor markets. Several papers examine how employer-provided health insurance may lead workers to keep jobs they would rather leave, for fear of losing insurance coverage for preexisting conditions (Brigitte Madrian 1994; Gruber and Madrian 1994, 1997, 2002; Janet Currie and Madrian 1999). Our paper complements these studies by focusing on a related, but conceptually different, link between the health care market and the labor market in the United States. In particular, in contrast to most papers in this strand of the literature, our paper delves deeper into the incentives generated by the institutional arrangements that govern health care, especially employer-provided health insurance and the interaction between private and public

[8] Peter Diamond (1992) mentions that the lack of long-term health insurance is an important market failure. John Cochrane (1995) shows that sequences of short-term contracts with properly chosen severance payments can fully insure consumers against reclassification risk. See also Mark Pauly, Howard Kunreuther, and Richard Hirth (1995).

insurance. Thus, our paper is also related to the literature that examines the interactions between public and private insurances (David Cutler and Gruber 1996; Jeffrey Brown and Finkelstein 2008). Most of these papers focus on how public insurance programs crowd out the demand for private insurance. Thus, while these papers consider the *contemporaneous* interaction between the public and private insurances, we focus on the *intertemporal* interactions and on health investment, rather than on insurance.

## II. A Simple Model

In this section, we present a simple model that adapts the theoretical framework of Acemoglu and Pischke (1999) to health expenditures. The goal of the model is to capture in the simplest way the effect of workers' turnover on the incentives to invest in health. In particular, we make the simplest assumptions to focus on the dynamic externality that we described in the introduction.

### A. *Assumptions*

There are two periods with no discounting. Health is a form of general human capital and, thus, it is an input in the production function of the worker. For simplicity, we assume that health is the only input in the production function $f(h)$, where $f(\cdot)$ is assumed to be increasing, differentiable, and concave.[9] Workers are risk-neutral and are endowed with an initial stock of health $h_1$. In the first period, workers can invest $m_1$ in health at a unit cost $p$. Health stock evolves according to

$$h_2 = k(h_1, m_1),$$

where $k(\cdot,\cdot)$ is the health production function, which we assume to be continuous and increasing in the initial stock of health $h_1$ and in the investment in health $m_1$—i.e., $\partial k/\partial h_1 > 0$ and $\partial k/\partial m_1 > 0$.

In the second period, the firm and the worker separate with an exogenous probability $q \in (0,1)$. If they separate, the worker gets an outside wage of $v(h_2)$, and the firm gets a surplus of zero. If they do not separate for exogenous reasons, the worker decides whether to stay with the firm and obtain the endogenous wage $w_2(h_2)$ or to quit and obtain the exogenous outside wage $v(h_2)$. It is important to note that the worker's productivity is $f(h_2)$ in both the current and outside firms; however, if the worker leaves her current firm, for either exogenous or endogenous reasons, she receives a wage equal to $v(h_2)$.[10] Acemoglu and Pischke (1999) assume that $v(h_2) < f(h_2)$ to reflect labor-market frictions and, more importantly, that $v'(\cdot) < f'(\cdot)$, which they term *wage compression*. Acemoglu and Pischke (1999) provide a variety of mechanisms that can lead to a wedge between $f(h_2)$ and $v(h_2)$—i.e., between a worker's

[9] Of course, healthier individuals may also *directly* enjoy a higher quality of life. We abstract from these incentives to invest in health. As long as health investment affects workers' productivity, the qualitative results of the model are not affected if workers also enjoy direct utility from health.

[10] The results do not change if we assume that the worker's productivity in the outside job is equal to $\gamma f(h_2)$, with $0 < \gamma < 1$.

productivity and her wage at other firms. Similarly, they describe several mechanisms that endogenously generate wage compression.[11]

We follow Acemoglu and Pischke's (1999) *full-competition regime*, in which firms in the first period compete for workers by offering them $\{w_1, m_1\}$, and in equilibrium they make zero profits.

## B. *Equilibrium*

If the worker and the current firm do not separate for exogenous reasons, their employment relationship generates a surplus equal to $f(h_2) - v(h_2)$ since $f(h_2)$ is the output generated by the worker, and $v(h_2)$ is the wage that the worker gets if she quits voluntarily. We assume that this surplus $f(h_2) - v(h_2)$ is divided between the worker and the firm according to the Nash bargaining solution, in which $\beta \in (0,1)$ represents the worker's bargaining power. Hence, the wage $w_2(h_2)$ that the worker obtains if she does not quit is

$$w_2(h_2) = (1 - \beta)v(h_2) + \beta f(h_2).$$

Thus, the firm's expected profit in period two is

$$\begin{aligned}\pi_2(h_2) &= (1 - q)[f(h_2) - w_2(h_2)] \\ &= (1 - q)(1 - \beta)[f(h_2) - v(h_2)].\end{aligned}$$

The firm's first-period profit is

$$\pi_1(h_1) = f(h_1) - w_1 - pm_1,$$

where $w_1$ is the worker's first-period wage, and $m_1$ is the worker's first-period medical expenditures, both to be determined in equilibrium.

Thus, the sum of profits for the firm in the two periods (recall the no-discounting assumption for simplicity) is

$$\begin{aligned}\Pi = \pi_1(h_1) + \pi_2(h_2) &= f(h_1) - w_1 - pm_1 \\ &\quad + (1 - q)(1 - \beta)[f(h_2) - v(h_2)].\end{aligned} \tag{1}$$

Ex ante competition among firms for the worker requires that the firm choose $m_1$ and $w_1$ to maximize the sum of profits $\Pi$, subject to the constraint that the worker receives as much utility as that offered by other firms $U$—i.e.,

$$w_1 + (1 - q)[(1 - \beta)v(h_2) + \beta f(h_2)] + qv(h_2) \geq U. \tag{2}$$

[11] Several papers provide empirical evidence on wage compression: Michael Beckmann (2001) and Harley Frazis and Mark Loewenstein (2006), among others.

Competition for the worker among firms implies that the utility level $U$ is high enough such that, in equilibrium, the firm makes zero profits—i.e., $\Pi = \pi_1(h_1) + \pi_2(h_2) = 0$.

Now, from equation (2), we have that, in equilibrium, the wage satisfies

$$w_1 = U - (1 - q)[(1 - \beta)v(h_2) + \beta f(h_2)] - qv(h_2). \tag{3}$$

Substituting the equilibrium worker's wage 3 into the firm's profit function 1 and maximizing with respect to the level of medical expenditures $m_1$, we obtain that the equilibrium level of medical expenditures $m_1^*$ solves the following first-order condition:

$$\left[qv'\left(k(h_1, m_1^*)\right) + (1 - q)f'\left(k(h_1, m_1^*)\right)\right]\frac{\partial k}{\partial m_1} = p. \tag{4}$$

Equation (4) implies that investment in health is socially inefficient unless there is never separation ($q = 0$). To see this, note that the efficient level of health investment $\hat{m}_1$ solves

$$f'\left(k(h_1, \hat{m}_1)\right)\frac{\partial k}{\partial m_1} = p, \tag{5}$$

which equates the marginal social benefit of medical expenditures $f'\left(k(h_1, \hat{m}_1)\right)\partial k/\partial m_1$ to their marginal cost $p$. The social benefit of health investment is given by the worker's productivity $f(h_2)$, which is independent of her employer, reflecting the nature of health as a form of general capital. The comparison between equations (4) and (5) reveals that the equilibrium health investment $m_1^*$ is lower than the socially efficient level $\hat{m}_1$ because of wage compression—i.e., $v'(\cdot) < f'(\cdot)$.

Moreover, Proposition 1 investigates the effect of the turnover probability $q$ on the equilibrium level of medical expenditures $m_1^*$, yielding the first implication that we empirically test in Section IVA:

PROPOSITION 1: *A decrease in the turnover rate $q$ increases equilibrium health expenditures $m_1^*$.*

PROOF:

From equation (4), let us define $\lambda(m_1^*, q)$ as

$$\lambda(m_1^*, q) = \left[qv'\left(k(h_1, m_1^*)\right) + (1 - q)f'\left(k(h_1, m_1^*)\right)\right]\frac{\partial k}{\partial m_1} - p = 0.$$

Using the implicit function theorem, we have

$$\frac{\partial m_1^*}{\partial q} = -\frac{\partial \lambda(m_1^*, q)/\partial q}{\partial \lambda(m_1^*, q)/\partial m_1^*}.$$

From the wage compression assumption $f' > v'$, we can obtain the following inequality:

$$\frac{\partial \lambda(m_1^*, q)}{\partial q} = -\left[f'\left(k(h_1, m_1^*)\right) - v'\left(k(h_1, m_1^*)\right)\right] \frac{\partial k}{\partial m_1} < 0.$$

Moreover, the necessary second-order condition implies that

$$\frac{\partial \lambda(m_1^*, q)}{\partial m_1^*} = \left[q v''\left(k(h_1, m_1^*)\right) + (1 - q) f''\left(k(h_1, m_1^*)\right)\right]\left(\frac{\partial k}{\partial m_1}\right)^2$$
$$+ \left[q v'\left(k(h_1, m_1^*)\right) + (1 - q) f'\left(k(h_1, m_1^*)\right)\right] \frac{\partial^2 k}{\partial m_1^2} < 0.$$

### C. *Dynamics of Health Expenditures*

We now provide a simple extension of our model to understand how health expenditures early in life affect health expenditures later on. In particular, we assume that there is also a third period, in which the individual is retired. In this third period, health still affects the utility of the individual—because of domestic production, for example. Formally, we assume that the utility of the individual is $d(h_3)$, with $d'(\cdot) > 0$. Moreover, $h_3$ evolves according to the following function:

$$h_3 = \min\left\{k(h_2, m_2), \overline{h}_3(h_2)\right\},$$

where $h_2$ is the individual's preretirement health, $m_2$ is the medical expenditures in period 2, and $k(\cdot,\cdot)$ is the standard health production function with $\partial k(\cdot)/\partial h_2 > 0$ and $\partial k(\cdot)/\partial m_2 > 0$. The function $\overline{h}_3(\cdot)$, with $\overline{h}_3'(\cdot) \geq 0$, captures in a reduced-form way the idea that the preretirement health stock $h_2$ determines the maximum level of health that can be achieved during retirement. For simplicity, we assume that all expenditures $m_2$ come *at no cost* to the retiree. This stark assumption reflects the fact that almost all retirees are covered by Medicare, and, thus, they do not bear the full costs of their medical expenditures.

Given these assumptions, all individuals choose medical expenditures $m_2^*$ so that their health reaches $\overline{h}_3(h_2)$—i.e.,

$$k(h_2, m_2^*) = \overline{h}_3(h_2). \tag{6}$$

Applying the implicit function theorem to equation (6), we obtain that

$$\frac{\partial m_2^*}{\partial h_2} = \frac{\partial \overline{h}_3/\partial h_2 - \partial k/\partial h_2}{\partial k/\partial m_2}.$$

Thus, we have $\partial m_2^*/\partial h_2 < 0$ if and only if $\partial \overline{h}_3/\partial h_2 - \partial k/\partial h_2 < 0$. A sufficient condition for this assumption to be satisfied is that one's health potential in retirement $\overline{h}_3(\cdot)$ is not too sensitive to current health stock $h_2$ (i.e., $\partial \overline{h}_3/\partial h_2$ is sufficiently small). For example, it is trivially satisfied if $\overline{h}_3(\cdot)$ is constant.

We can now combine the discussion above with our Proposition 1 and provide the full set of empirical implications that we test in Section IV.[12]

PROPOSITION 2: *If* $\partial \overline{h}_3 / \partial h_2 < \partial k / \partial h_2$, *then workers in jobs with lower turnover rates have:*

(*i*) *Higher medical expenditures* $m_1^*$ *while working; and*

(*ii*) *Lower medical expenditures* $m_2^*$ *and better health during retirement.*

## III. Data

We use several distinct data sources in our empirical analysis. In particular, we use the annual MEPS and the biannual HRS to study the medical expenditures and medical care utilization of working individuals and retirees, respectively. We complement MEPS and HRS with additional variables that we use to construct instruments obtained from the Statistics of US Businesses (SUSB). Furthermore, we use the annual British Household Panel Survey (BHPS) to perform falsification tests on UK workers. Since all these datasets are publicly available, we describe them only briefly here and refer the reader to their respective websites for a more thorough description.[13]

*MEPS.*—MEPS is a set of large-scale annual rotating panel surveys of families and individuals, their medical providers, and employers across the United States. It is designed to provide nationally representative estimates of health care use, expenditures, sources of payment, and insurance coverage for the US civilian noninstitutionalized population.

MEPS has several components, and the Household Component (HC) serves our purposes. HC provides data from individual households and their members, supplemented by data from their medical providers. HC surveys households in two consecutive years, collecting detailed information for each person in the household on demographic characteristics, health conditions, health status, use of medical services, charges and source of payments, access to care, satisfaction with care, health insurance coverage, income, and employment. The public version of the survey reports the one-digit codes of industry and occupation of the individual.[14] In our empirical analysis in Section IVA, we use MEPS data from the 1996–2006 surveys. We deflate all monetary values using the GDP implicit price deflator, with 2000 as the base year.

[12] The extension of the model to include a retirement period will change the first-order condition for equilibrium level of $m_1^*$ from 4 to

$$\left[qv'(k(h_1,m_1^*)) + (1-q)f'(k(h_1,m_1^*)) + d'(\overline{h}_3(h_2))\overline{h}_3'(h_2)\right]\frac{\partial k}{\partial m_1} = p.$$

Proposition 1 and its proof remain unchanged.

[13] MEPS is available at http://www.meps.ahrq.gov; HRS is available at http://hrsonline.isr.umich.edu; SUSB is available at http://www.census.gov/csd/susb; and BHPS is available at http://www.iser.essex.ac.uk/survey/bhps.

[14] The three-digit industry codes are contained in a version restricted from public access.

*HRS*.—HRS began as a panel survey of a nationally representative sample of people aged 51 to 61 in 1992, as well as their spouses, with oversamples of blacks, Hispanics, and Florida residents. This original cohort (wave 1) has been reinterviewed every other year since then. In 1998, the sample was supplemented with both older and younger cohorts. Eight waves are currently available.

HRS contains detailed information about the current and past health status of respondents, along with rich data on their job history and information about economic and demographic variables, including education, income, and wealth. Beginning with wave 3, the survey asks questions about total medical expenditures. In some waves, a continuous value is reported, while in others, a series of unfolding bracket questions are asked. Based on these brackets (and some additional variables), the RAND Corporation imputes a continuous value of total medical expenditures in each wave, and this is the main dependent variable that we use in our empirical analysis on retirees. As with MEPS, we also deflate all monetary values using the GDP implicit price deflator, with 2000 as the base year.

HRS also asks questions about the individual's employment history. A respondent is asked about past jobs at his/her first interview. From the responses, the RAND Corporation reconstructs the years of tenure at the longest reported job and the one-digit industry codes of the longest job.[15] Because total medical expenditures were surveyed from 1996 (wave three), we use HRS data from 1996 to 2002 in the analysis in Section IVB.[16] Furthermore, our sample includes only individuals over 66 years of age. This age restriction is dictated by the fact that in each wave, HRS reports the medical expenditures for the previous two years, and we want our individuals to have access to similar medical coverage through Medicare.

*SUSB*.—SUSB is a dataset extracted from the Business Register, a file of all known single- and multi-establishment companies maintained and updated by the US Census Bureau. The Business Register is the same database that is used to produce County Business Patterns (CBP). SUSB shares some features with CBP. It provides national and sub-national data on the distribution of economic data by size and industry, reporting the number of establishments, employment, and annual payroll for each geographic-industry-size cell.

More important, SUSB reports the number of establishments and corresponding employment change for births, deaths, expansions, and contractions by employment size of enterprise, industry, and state. We use data on establishment deaths to construct our instruments for job turnover in the empirical analyses in Sections IVA and IVB.

*BHPS*.—BHPS, an annual panel survey that began in 1991, follows about 5,500 households and 10,300 individuals drawn from 250 areas of Great Britain. It is a dataset with rich individual-level demographic, social and economic variables, as well as detailed information on health-related issues, such as number of doctor visits and self-perceived health status.

[15] Employing the procedure that the RAND Corporation uses for the one-digit industry code, we construct the three-digit industry code from the restricted-access HRS data, which are used in Section C.2 of the online Appendix.

[16] Only six waves were available when we started this project.

Table 1—Descriptive Statistics of Key Variables in MEPS and HRS Samples

| | MEPS (1995–2005) | | HRS (1996–2002) | | BHPS (1995–2002) | |
|---|---|---|---|---|---|---|
| Variable | Mean | SD | Mean | SD | Mean | SD |
| Individual medical expenditure | 1,832 | 6,111 | 8,327 | 24,707 | … | … |
| Job tenure | 6.67 | 7.71 | … | … | 6.1 | 7.2 |
| Longest job tenure | … | … | 23.8 | 12.7 | … | … |
| Age | 38.92 | 11.89 | 75.1 | 6.8 | 41.1 | 12.3 |
| Years of education | 12.87 | 2.90 | 11.9 | 3.2 | 11.8 | 2.4 |
| Income | $31,519 | $25,525 | … | … | £ 28,934 | £ 22,129 |
| Total assets/10,000 | … | … | 3.34 | 8.12 | … | … |
| Male | 0.50 | 0.50 | 0.51 | 0.50 | 0.46 | 0.49 |
| White | 0.80 | 0.39 | 0.85 | 0.35 | 0.96 | 0.18 |
| Black | 0.14 | 0.34 | 0.13 | 0.33 | … | … |
| Married | 0.59 | 0.49 | 0.59 | 0.49 | 0.60 | 0.48 |
| Family (household) size | 3.21 | 1.60 | 1.94 | 0.93 | 3.00 | 1.32 |
| Union | 0.13 | 0.33 | … | … | 0.96 | 0.29 |

*Note:* Medical expenditures, income, and total assets have been deflated to correspond to 2000 dollars in MEPS and HRS.

### A. *Summary Statistics*

Table 1 reports summary statistics for the main variables of the three datasets that we use in our analysis. Average annual medical expenditures are about $1,800 per individual in MEPS, and about $8,300 in HRS. Obviously, individuals are younger in MEPS than in HRS (39 years old versus 75 years old), and they have, on average, shorter job tenures (the average current job tenure in MEPS is 6.7 years, while the average longest preretirement job tenure in HRS is 23.8 years). Other individual characteristics—i.e., education, race, gender, and marital status—are roughly similar across the two datasets.

We employ multiple, distinct datasets because we are not aware of a single, publicly available dataset that both follows many individuals over a long period of time and reports their employment history and their medical expenditures at several points in their lives, including retirement. MEPS and HRS are the best available datasets we know of to investigate how current and past job turnover affects the health expenditures of employed and retired individuals, respectively. In particular, these datasets report very detailed characteristics of the individuals, including the outcome variable that is the focus of our model: health expenditures. This richness of the data implies that we can control for several observed individual characteristics that are often unobserved in other studies. In particular, the time-series dimension of the data implies that we can construct an empirical model (described in detail in Section IV) that controls for unobserved factors that may simultaneously affect individual labor-market choices and health expenditures. Moreover, the similarity of datasets allows us to use the same variables—i.e., number and rates of plant closures—to construct instruments that shift labor-market histories.

## IV. Empirical Analysis

In this section, we test the main implications of the model. The empirical analysis closely follows Propositions 1 and 2: using MEPS data, Section IVA investigates how job attachment affects the medical expenditures of employed individuals, and, using HRS data, Section IVB analyzes how past job attachment affects the medical expenditures of retired individuals.

### A. *Health Expenditures of Workers*

To investigate the effect of job attachment on individuals' health investment and health status, we specify the following reduced-form regression equation:

$$(7) \qquad y_{jirt} = \beta_0 + \beta_T \log(\textit{Job Tenure}_{jirt}) + \beta_X X_{jirt} + \eta_{rt} + \zeta_j + \epsilon_{jirt},$$

where the dependent variable $y_{jit}$ is one of the outcomes of interest (e.g., individual medical expenditures, individual doctor visits) for individual $j$ working in industry $i$ in region $r$ in year $t$. The main explanatory variable of interest is (the log of) $\textit{Job Tenure}_{jirt}$, which is the number of years the individual has been employed in his/her current job; $X_{jirt}$ is a large set of control variables—e.g., a cubic polynomial in age, gender, race, years of education, annual income, family size, etc.; $\eta_{rt}$ is a region $r$-year $t$ fixed effect; $\zeta_j$ is an individual fixed effect; and $\epsilon_{jirt}$ is an unobservable component.

*Empirical Challenges and Solutions*.—Two main issues challenge the identification of the effect of *Job Tenure* in equation (7). The first is selection: Workers are not randomly allocated to jobs. Individual characteristics induce different people to select into different jobs and different industries. If these characteristics are unobserved—as is the case with ability, discount factor, risk aversion, etc.—and are correlated with *Job Tenure*, then the estimated coefficient $\beta_T$ will be biased. The second issue is reverse causality: individual health may affect labor-supply decisions and, thus, individuals who have higher medical expenditures could be more or less likely to change jobs. For example, individuals in poor health may quit their jobs to receive medical care, leading to a downward bias in the coefficient $\beta_T$. Alternatively, as the literature on "job lock" has highlighted (e.g., Madrian 1994), individuals without health insurance could be more likely to change jobs, and not having health insurance may lead them to spend less on health care, resulting in an upward bias in the coefficient $\beta_T$.

The panel component of MEPS (two years, at most) allows us to deal with selection by including individual fixed effects $\zeta_j$ to control for any fixed, individual unobserved factor. Moreover, we deal with the reverse causality due to labor-supply decisions by using instruments that shift the main endogenous variable—*Job Tenure*—for plausibly exogenous reasons. In particular, the SUSB dataset reports the number and the rate of deaths of establishments in industry $i$ and region $r$, as well as the number and the rate of workers who lost their jobs due to establishment deaths in industry $i$ and region $r$. Hence, we link these variables to the industry and the region in which each individual is working, and use them as instruments for *Job Tenure*. We further construct interactions of these instruments with the individual's age, and a binary indicator equal to one if the individual is a male, and zero otherwise.

The idea of these instruments is that they are correlated with workers' *Job Tenure*, as they shift labor demand, and their effect differs according to observable individual characteristics, such as education and gender. Moreover, the instruments arguably do not suffer from reverse-causality concerns, since, for example, it is unlikely that individuals with higher or lower health expenditures cause establishment deaths. The validity of these instruments further relies on two assumptions: (i) The employer-employee pair forms expectations about plant closures that are correlated with their realizations, so that expected turnover and actual turnover generated by plant closures covary; and (ii) establishment deaths do not *directly* affect individual medical expenditures—i.e., the exclusion restriction. Several papers report evidence in favor of this exclusion restriction, in particular for short-run expenditures. For example, Martin Browning, Anne Moller Dano, and Eskil Heinesen (2006) find that being displaced does not cause hospitalization for stress-related disease, and Andreas Kuhn, Rafael Lalive, and Josef Zweimüller (2009) report that job loss following a plant closure does not cause a significant increase in public health costs associated with take-up of health provisions.[17] In addition, since individual *Job Tenure* is self-reported and, thus, potentially mismeasured, the use of instruments corrects the attenuation bias generated by the measurement error of our key explanatory variable.

To gain intuition about identification, we report the results of the "first-stage" regression in full detail in panel A of Table A1 in online Appendix A. The key findings are that the instruments are jointly significant (the $F$-test on the exogenous instruments has a value above 23), and that larger values of the instruments—i.e., more plant closures and more workers losing jobs due to plant closures—reduce job tenure, as expected.

*Results on Medical Expenditures*.—Table 2 presents the results for the log of individual medical expenditures. We present the results of several specifications. Column 1 presents the results of a simple OLS regression. Column 2 presents the results of an IV regression, instrumenting for *Job Tenure* using the instruments described in the previous section. Column 3 presents the results of an IV regression with individual fixed effects.[18] Column 4 presents the results of a specification that uses the Arellano and Bond (1991) methodology. The main advantage of this methodology is that it allows us to control for additional persistent unobservable components (beyond fixed characteristics captured by the fixed effects) that may induce individuals to select into different jobs. However, since the panel component of MEPS is limited to two years, while this methodology requires a longer panel, we generate a synthetic panel by constructing cohorts of people grouped by sex, decade of birth, one-digit industry, and census region. In the Appendix, we describe the methodology and the construction of the cohorts in greater detail.

The results reported in Table 2 indicate that the coefficients of log(*Job Tenure*) are positive in all specifications. Thus, these coefficients are consistent with the idea that an employer-employee pair invests more in the employee's health when the

[17] One potential drawback of these papers is that they focus on different countries.

[18] The number of observations varies across specifications 1–3 because the instruments used in specification (2) do not apply to individuals working in public administration and in the active military. Further, the fixed effect IV specification of column 3 requires individuals to appear in two surveys. The OLS specification (1) performed on the same sample as the IV specification (2) yields almost identical results.

TABLE 2—THE RELATIONSHIP BETWEEN WORKERS' JOB TENURE AND INDIVIDUAL MEDICAL EXPENDITURES

| | OLS<br>(1) | IV<br>(2) | IV<br>with fixed effects<br>(3) | Arellano-Bond<br>with AR(1) errors<br>(4) |
|---|---|---|---|---|
| Log (JOB TENURE) | 0.032***<br>(0.007) | 0.505***<br>(0.208) | 0.801*<br>(0.454) | 0.535**<br>(0.256) |
| Age | −0.712***<br>(0.035) | −0.792***<br>(0.051) | −1.481***<br>(0.616) | −0.598**<br>(0.263) |
| Age squared | 0.018***<br>(0.0008) | 0.019***<br>(0.001) | 0.026***<br>(0.012) | 0.013*<br>(0.006) |
| Education | 0.232***<br>(0.005) | 0.239***<br>(0.007) | −0.035<br>(0.064) | 0.189***<br>(0.022) |
| Income/10,000 | 0.214***<br>(0.015) | 0.073<br>(0.059) | 0.007<br>(0.028) | 0.036<br>(0.067) |
| Male | −2.223***<br>(0.026) | −2.261***<br>(0.030) | | −1.090***<br>(0.058) |
| Married | 0.767***<br>(0.030) | 0.691***<br>(0.047) | 0.044<br>(0.134) | 0.265*<br>(0.142) |
| Family size | −0.346***<br>(0.009) | −0.354***<br>(0.011) | −0.251***<br>(0.044) | −0.212***<br>(0.041) |
| Union | 0.450***<br>(0.037) | 0.097***<br>(0.129) | −0.182<br>(0.337) | 0.284<br>(0.196) |
| $\rho$ | | | | 0.262***<br>(0.028) |
| Observations | 108,427 | 91,287 | 72,330 | 4,216 |
| Panels | | | 36,165 | 594 |

*Notes:* All regressions also contain age cubed, income squared, firm size, race, and year fixed effects not reported. Robust standard errors in parentheses.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

employee's expected turnover is lower, as Proposition 1 predicts. The magnitude of the estimated effect of *JOB TENURE* varies across specifications, and the coefficients are larger as we use instruments to correct for the endogeneity of *JOB TENURE*. Moreover, the coefficients of specifications (2) to (4) indicate that the economic significance of the effect is nontrivial. Specifically, increasing *JOB TENURE* by 10 percent increases workers' annual individual medical expenditures by 5 to 8 percent, a rather large effect.

*Results on Doctor Visits*.—We further investigate whether individuals are more likely to visit a doctor when job turnover is lower. These additional regressions have three goals. First, total medical expenditures include many different types of expenditures, and it is useful to check whether the results also apply to a more narrow and basic category of health care. Second, the price of medical expenditures may differ across individuals, and doctor visits offer a *quantity* of services acquired. Third, in Section IVB, we compare patterns of health investment in the United States and the United Kingdom, and the UK data—like data from many countries with a national health system—report only the quantity of medical services acquired, not the expenditures.

Table 3—The Relationship between Workers' Job Tenure and Individual Doctor Visits

| | OLS (1) | IV (2) | IV with fixed effects (3) | Arellano-Bond with AR(1) errors (4) |
|---|---|---|---|---|
| Log (JOB TENURE) | −0.003*** | −0.041*** | −0.172** | −0.098** |
| | (0.0007) | (0.015) | (0.085) | (0.043) |
| Age | 0.049*** | 0.055*** | 0.282*** | 0.039 |
| | (0.004) | (0.005) | (0.097) | (0.039) |
| Age squared | −0.001*** | −0.001*** | −0.004** | −0.0005 |
| | (0.0001) | (0.0001) | (0.002) | (0.001) |
| Education | −0.015*** | −0.015*** | −0.0001 | −0.023*** |
| | (0.0005) | (0.0006) | (0.007) | (0.003) |
| Income/10,000 | −0.018*** | −0.007 | −0.0001 | −0.0001 |
| | (0.001) | (0.004) | (0.003) | (0.011) |
| Male | 0.221*** | 0.226*** | | 0.208*** |
| | (0.003) | (0.003) | | (0.010) |
| Married | −0.071*** | −0.067*** | −0.013 | −0.023 |
| | (0.003) | (0.004) | (0.016) | (0.022) |
| Family size | 0.028*** | 0.029*** | 0.024*** | 0.015** |
| | (0.0009) | (0.001) | (0.006) | (0.006) |
| Union | −0.038*** | −0.011*** | 0.059 | −0.002 |
| | (0.004) | (0.010) | (0.054) | (0.031) |
| $\rho$ | | | | 0.093*** |
| | | | | (0.031) |
| Observations | 108,427 | 91,287 | 72,330 | 4,216 |
| Panels | | | 36,165 | 594 |

*Notes:* All regressions also contain age cubed, income squared, firm size, race, and year fixed effects not reported. Robust standard errors in parentheses.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

Thus, we estimate linear probability models in which the dependent variable is equal to one if the individual *did not* visit a doctor in the previous year, and Table 3 reports the estimated coefficients. As in Table 2, we present the results of several specifications. Column 1 presents the results of a simple OLS regression. Column 2 presents the results of an IV regression, instrumenting for job tenure using the instruments described in Section IVA. Column 3 presents the results of an IV regression with individual fixed effects. Column 4 presents the results of a specification that uses the Arellano and Bond (1991) methodology. Since the unit of observation in the specification of column 4 is a cohort, the dependent variable is equal to the fraction of people in the cohort who did not visit a doctor in the previous year.

The results reported in Table 3 indicate that the coefficients of log (*Job Tenure*) are negative in all specifications, confirming the idea that an employer-employee pair invests more in the employee's health when the employee's expected turnover is lower. Moreover, the estimated coefficients in columns 3 and 4 imply that increasing *Job Tenure* by 10 percent decreases the probability of not visiting a doctor at least once per year by 1–1.7 percentage points, which represents a 3 to 4 percent decrease from the average sample probability of not visiting a doctor, equal to 0.39.

*Assessment of Our Estimates*.—Our simple model in Section II is intended to provide the qualitative comparative statics predictions summarized in Proposition 1. Overall, the results reported in Table 2 provide strong *qualitative* evidence consistent with these predictions. An interesting and important question is whether our estimated elasticity of medical expenditures with respect to job tenure is also *quantitatively* consistent with the key economic mechanism of our paper and with existing evidence in the literature. To provide a quantitative assessment of our estimates—reported in Table 2 to be in the range 0.5–0.8—we consider in online Appendix D an infinite-horizon extension of the model (also adapted from Acemoglu and Pischke 1998) that retains our key idea—i.e., that frictions in the labor market prevent an employer-employee pair from capturing the entire surplus from investment in an employee's health. Using some plausible functional forms and some parameters from the existing literature, we show that this extension of the model delivers a calibrated elasticity of medical expenditure with respect to job tenure equal to 0.7, which lies in the range of our estimates.

### B. *Health Expenditures of Retirees*

The analysis of retirees' medical expenditures follows as closely as possible from the previous analysis of workers' medical expenditures. Nonetheless, some slight modifications are necessary because of the different data, and we now describe them in detail. Our reduced-form equation now reads

$$y_{jt} = \beta_0 + \beta_T \log(\textit{Past Tenure}_{jt}) + \beta_X X_{jt} + \eta_t + \zeta_j + \epsilon_{jt}, \tag{8}$$

where the dependent variable $y_{jt}$ is one of the outcomes of interest (e.g., individual medical expenditures, health status) of individual $j$ in year $t$. The main explanatory variable of interest is (log of) $\textit{Past Tenure}_{jt}$: the number of years of the longest job tenure; $X_{jt}$ is again a set of control variables: a cubic polynomial in the age of the individual; years of education; total assets; total assets squared; size of the family; gender; and race; $\eta_t$ is a year fixed effect; $\zeta_j$ is an individual effect; and $\epsilon_{jt}$ is an unobservable component. In summary, the variables included in equation (8) are almost identical to the variables employed in the analysis of workers' medical expenditures. The main differences are that we use assets instead of income since most people in the HRS sample are retired, and we cannot include union status in the specification since the union status of the longest job tenure is not reported in HRS.[19]

Since HRS is a panel dataset, we could potentially control for unobserved heterogeneity with individual fixed effects. However, the HRS sample is composed mainly of retired individuals, and, thus, the main variable of interest—i.e., *Past Tenure*—has almost no within-panel variation. Hence, we cannot treat $\zeta_j$ as individual fixed effects, and, instead, treat them as individual random effects. Nonetheless, since almost all individuals in HRS are retired, their labor-market histories were clearly determined many years before the sample period (1996–2002). This temporal lag

[19] The fixed effects included in the regression also differ slightly, because the instruments exploit a different variation. Thus, the subscripts in equations (7) and (8) differ.

implies that period-$t$ shocks that may affect medical expenditures should not be correlated with past labor-market history. Moreover, this temporal lag helps us in the construction of the instruments and, thus, in the identification of the effect of *Past Tenure* on medical expenditures in equation (8).

In particular, HRS reports the census division of birth of each individual. Our instruments exploit the variation in the rate of establishment deaths and in the rate of workers who lost their jobs due to establishment deaths in 1990 in the division of birth of each individual. We further construct interactions of these instruments with the individual's years of education, and a binary indicator equal to one if the individual is a male, and zero otherwise. Thus, the instruments are exactly the same as those we employed in the analysis of workers, although they now empasize a slightly different variation in the data. More precisely, the absence of variation within individuals in the main variable of interest—i.e., *Past Tenure*—prevents us from exploiting the temporal variation of plant closures (as in the analysis of MEPS data), and we exploit only its geographic variation. Thus, the key idea of the instruments is that the census division of birth is arguably exogenous to the individual, and so are its plant closures. Plant closures differ across geographic regions, and their effect differs according to observable individual characteristics, such as education and gender. Thus, the instruments are plausibly uncorrelated with unobserved individual characteristics that may simultaneously determine past labor-market outcomes and retirees' current medical expenditures. Panel B of Table A1 in online Appendix A reports the results of the first-stage regression in detail. The key findings are that the instruments are jointly significant (the $F$-test on the exogenous instruments has a value above 18) and that a larger value of the instruments—i.e., a greater rate of plant closures and a greater rate of workers losing jobs due to plant closures—reduces job tenure, as expected.

*Results on Medical Expenditures*.—Panel A of Table 4 presents the results for the (log of) individual medical expenditures in the HRS sample. These results are remarkable. As Proposition 2 predicts, the coefficient of log(*Past Tenure*) in column 1 is negative and significant. Moreover, the economic significance of the coefficient is large. The magnitude of the coefficient of log(*Past Tenure*) in column 1 means that increasing *Past Tenure* by 10 percent decreases annual individual medical expenditures by approximately 6.5 percent.

*Results on Health Status*.—We further investigate whether retirees with higher past job attachment report better health. Panel B of Table 4 reports the coefficient of a regression in which the dependent variable is equal to one if the retiree reported being in the lowest two categories of self-reported health (i.e., fair and poor), and zero otherwise. The coefficient estimate of log(*Past Tenure*) is negative and statistically significant at the 1 percent level. The magnitude of the coefficient indicates that the probability of a retiree reporting poor health decreases by almost three percentage points when his job tenure prior to retirement increases by 10 percent.

In summary, our analysis indicates that retirees with a longer past job tenure have lower medical expenditures and are in better health, consistent with the predictions of Proposition 2 of our model.

Table 4—The Relationship between Retirees' Past Job Tenure and Medical Expenditures (Panel A) and Health Status (Panel B)

| | *Panel A. Log individual medical expenditure* (1) | *Panel B. Health status* (2) |
|---|---|---|
| Log (JOB TENURE) | −0.643*** | −0.265*** |
| | (0.247) | (0.058) |
| Age | 1.213 | 0.141 |
| | (0.817) | (0.178) |
| Age squared | −0.015 | −0.002 |
| | (0.106) | (0.002) |
| Education | 0.044*** | −0.016*** |
| | (0.011) | (0.002) |
| Total assets/1,000,000 | 0.032 | −0.025** |
| | (0.036) | (0.008) |
| Male | 0.351*** | 0.151*** |
| | (0.120) | (0.029) |
| Married | −0.040 | −0.021** |
| | (0.043) | (0.010) |
| Household size | 0.011 | 0.001 |
| | (0.020) | (0.004) |
| Observations | 17,530 | 17,530 |
| Panels | 7,055 | 7,055 |

*Notes:* The dependent variable for column 2 is a dummy variable taking value one if the retiree reported being in the lowest two categories of self-reported health, i.e., fair and poor, and zero otherwise. The estimated equations also contain: age cubed; total assets squared; year, race, and census division fixed effects. Their coefficients are not reported.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

## C. *Assessing the Magnitude of Life Cycle Inefficiency*

Our previous analysis shows that the differences in medical expenditures among workers and among retirees with different job tenures are large. Moreover, the analysis reveals a stark reversal in medical expenditures: Individuals with higher expenditures during their working years have lower expenditures during retirement. In this section, we combine the previous estimates of workers' and retirees' expenditures and seek to quantify the dynamic externality that lies at the heart of this paper. More precisely, we wish to compare the lifetime expenditures of two workers, A and B, whose only difference is their job tenures. We want to be explicit that calculating the exact size of the externality implied by our regressions is complicated, in particular because our main variable of interest—job tenure—is measured at two different points in time. Nevertheless, we try to perform a simple back-of-the-envelope calculation.

Suppose that both individuals work for 45 years and then retire for 15 years before dying. Individual A works in a job in which mobility is high, while individual B works in a job in which mobility is low. For example, let us assume that individual B's log job tenure is one standard deviation higher than that of individual A. Using the coefficient of log (*Job Tenure*) in the Arellano-Bond specification (4) in Table 2, and applying it to one standard deviation of log (*Job Tenure*) in MEPS cohort data—equal to 0.52—we obtain $0.52 \times 0.53 = 27$ percent. At the average MEPS medical expenditures (\$1,814), this implies that individual A has expenditures lower than B's by approximately \$500 per year.

Let us now consider both individuals' medical expenditures during retirement. In particular, let us assume that the cross-sectional difference in job tenure in MEPS data carries over and, thus, one standard deviation in log tenure in MEPS translates into one standard deviation in log tenure in HRS. One standard deviation of log tenure in the HRS data is equal to 0.77. Multiplying that by the coefficient of log tenure in the HRS regressions, we obtain $0.65 \times 0.77 = 50$ percent. At the average HRS medical expenditures (\$8,327), this implies that individual A has expenditures higher than B's by approximately \$4,160 per year.

Thus, if individuals A and B work for 45 years and then retire for 15 years, not discounting their expenditures, we have that, during their working years, individual A's health expenditures are approximately \$22,500 lower than individual B's. And, during retirement, individual A's health expenditures are approximately \$62,500 higher than individual B's. The total difference is approximately \$40,000, a rather large difference. This calculation suggests that one additional dollar of health expenditures during the working years may lead to approximately 2.8 dollars of savings in retirement.[20] Obviously, this is a very rough calculation that neglects many important factors, such as mobility between low-attachment and high-attachment jobs. Moreover, it comes from two different datasets, and not from a single, long panel. In addition, on the one hand, it neglects discounting, but on the other hand, it also neglects that the price of medical care has been rising more than the interest rate. Furthermore, it neglects any effect of the changes in life cycle medical expenditures on the quality of life and on mortality. In summary, we believe that this calculation neglects many other factors that are important in assessing the full effect of the externality. Nonetheless, we believe it describes in a simple way the externality we have in mind and its magnitude in the data.

## V. Additional Evidence

The results of the previous empirical analysis provide strong evidence that job turnover affects health expenditures, as Propositions 1 and 2 predict. Nevertheless, we acknowledge that our empirical strategies may have limits. Like many papers that cannot employ a randomized experiment, despite our best attempts to control for individual characteristics and to use arguably exogenous instruments, it is possible that the instruments are correlated with unobserved characteristics that affect medical expenditures. Precisely because we are concerned by this possibility, we have performed several additional tests. We now report on additional regressions that investigate employers' provision of health plans to their employees; on falsification tests that use data from the UK BHPS; and on a robustness check that uses a different set of instruments for retirees' job tenure. Online Appendix B further reports on robustness checks that control for the potential mismeasurement of workers' job tenure; online

[20] The magnitude of our back-of-the-envelope calculation is lower if we use individual data rather than cohort data, since the standard deviation of log (*Job Tenure*) is lower in cohort data than in individual data because the within-cohort variation is suppressed. If we use the standard deviation of individual log (*Job Tenure*) in our calculation, we obtain that savings would be equal to about \$1.7.

Appendix C.2 provides additional evidence that supports our argument using an alternative empirical strategy. While we acknowledge that none of these alternatives alone is definitive, we believe that each has different strengths, and we find that all these alternative approaches deliver results consistent with our main ones.

### A. *Provision of Health Plans*

The goal of this section is to investigate how workers' job tenure affects whether firms offer them a health plan. These additional regressions have one main goal. In an employment-based health insurance system, employers' investments in their employees' health involve the provision of a health plan. Thus, these regressions investigate precisely this channel. Specifically, we use a linear probability model in which the dependent variable is equal to one if the individual is offered a health plan in his current main job. As in Tables 2 and 3, we present the results of several specifications. Column 1 presents the results of a simple OLS regression. Column 2 presents the results of an IV regression, instrumenting for job tenure using the instruments described in Section IVA. Column 3 presents the results of an IV regression with individual fixed effects. Column 4 presents the results of a specification that uses the Arellano and Bond (1991) methodology, in which the unit of observation is a cohort. Thus, the dependent variable is equal to the fraction of people in the cohort who are offered a health plan in their current main job.

The results reported in Table 5 indicate that the coefficients of log(*Job Tenure*) are positive in all specifications, indicating that employers are more likely to provide a health plan when employees' expected turnover is lower. Moreover, the estimated coefficients in columns 2 to 4 imply that increasing *Job Tenure* by 10 percent increases the probability that the employer provides the employee with a health plan by 1.7–1.8 percentage points, which represents a 2.5 percent increase from the average sample probability of being offered a health plan, equal to 0.68.[21]

### B. *Falsification Test: UK Workers*

In this section, we investigate whether individuals in the United Kingdom—a country with a national health system—exhibit the same patterns that we documented for individuals in the United States. We believe that this is a useful comparison: while the employment-based health care system is unique to the United States, wages and turnover patterns are very similar across many developed countries (Lawrence Katz, Gary Loveman, and David Blanchflower 1995). In a national health insurance system, an employer-employee pair does not directly pay for the employee's health investment. Thus, the dynamic inefficiency that we emphasize is not as relevant as in the US system. Hence, if the employment-based health insurance system is indeed responsible for the relationship between job turnover and health expenditures in the United States, we would expect that similar relationship does *not* hold in United Kingdom. Therefore, we conduct "falsification" tests by

[21] Further evidence that the provision of health plans is the main channel through which employers affect employees' health expenditures comes from the fact that the tenure effects documented in Tables 2 and 3 are reduced when we add the explanatory variable Health Plan Offered in those regressions.

Table 5—Job Tenure and Health Plan Offerings

| | OLS | IV | IV with fixed effects | Arellano-Bond with AR(1) errors |
|---|---|---|---|---|
| Log (JOB TENURE) | 0.051*** | 0.189*** | 0.172** | 0.161*** |
| | (0.0007) | (0.015) | (0.064) | (0.037) |
| Age | 0.090*** | 0.073*** | −0.034 | 0.049 |
| | (0.003) | (0.005) | (0.077) | (0.039) |
| Age squared | −0.002*** | −0.001*** | 0.001 | −0.001 |
| | (0.0001) | (0.0001) | (0.001) | (0.001) |
| Education | 0.012*** | 0.013*** | −0.001 | 0.016*** |
| | (0.0005) | (0.0007) | (0.005) | (0.003) |
| Income/10,000 | 0.086*** | 0.047*** | 0.004 | 0.105*** |
| | (0.004) | (0.005) | (0.003) | (0.011) |
| Male | 0.014*** | 0.012*** | | −0.007 |
| | (0.002) | (0.003) | | (0.005) |
| Married | 0.030*** | 0.009*** | 0.001 | 0.030 |
| | (0.002) | (0.004) | (0.014) | (0.026) |
| Family size | −0.020*** | −0.025*** | −0.016*** | −0.019*** |
| | (0.0008) | (0.001) | (0.005) | (0.006) |
| Union | 0.109*** | 0.024*** | 0.187* | 0.038 |
| | (0.002) | (0.009) | (0.055) | (0.030) |
| $\rho$ | ... | | | 0.444*** |
| | | | | (0.044) |
| Observations | 101,881 | 86,009 | 65,480 | 4,216 |
| Panels | | | 32,740 | 594 |

*Notes:* All regressions also contain age cubed, income squared, firm size, race, and year fixed effects not reported. Robust standard errors in parentheses.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

replicating as closely as possible some of the analysis of Section IVA using data from the BHPS, a dataset that reports quite detailed information on individual labor-market histories, along with some information on health-related issues.

Unfortunately, we cannot directly investigate the relationship between medical expenditures and job tenure, as in Table 2. The reason is that BHPS does not report total medical expenditures at the individual level.[22] Indeed, we are not aware of any non-US dataset that, like MEPS, collects this information at the individual level. Nonetheless, BHPS reports the number of doctor visits for a sample of UK individuals. This allows us to conduct a falsification test by replicating the analysis of doctor visits for US individuals, as we reported in Table 3. If the employment-based health care system in the United States is responsible for the relationship we documented in Table 3, then we should not expect the number of doctor visits of United Kingdom workers to have the same relationship with job tenure.

We implement this falsification test in a panel regression in which the dependent variable is a binary variable equal to one if the individual did not visit a doctor in the last year, and zero otherwise. The specification is as close as possible to the specification of Table 3, with the additional advantage that we can use the Arellano-Bond

[22] BHPS collects only information about out-of-pocket medical expenditures, but this represents only a small fraction of total medical expenditures in a national health insurance system.

procedure on individual data since BHPS is a long panel dataset, with several individual samples in at least three surveys. Moreover, BHPS reports rich individual-level variables that allow us to construct instruments for the potentially endogenous variable *Job Tenure*. Unfortunately, we cannot use the same instruments—i.e., plant closures—that we used on MEPS and HRS data since we are not aware of any UK public dataset that collects the same information collected in the US SUSB. Nonetheless, BHPS reports the district of birth of the individual, so we use as instruments for (the log of) *Job Tenure* of individual $i$ (the log of) the average tenure of all individuals of the same sex born in the same five-year window and in the same district as individual $i$. The idea of the instruments is that the district of birth is obviously exogenous to the individual, as is its industrial composition, for example. However, continuing the example, the industrial composition of the district of birth affects the skills that the individual accumulates (say, through intergenerational transmission of human capital or the type of schooling). Therefore, individuals are more likely to work in industries that are popular in the labor market of their area of birth. Through such a mechanism, the longest job tenure of each individual is correlated with the job tenure of all individuals born in the same district. However, the instruments are plausibly uncorrelated with individual ability or risk attitudes, the main unobserved individual effects that may simultaneously determine health investment and labor-market outcomes, as we discussed in Sections IVA and IVB. Moreover, since the career choices of men and women are quite different, we separate the instruments by gender.

Table 6 presents the results of the same specifications reported in Table 3. All specifications show that individuals with a longer job tenure are not more likely to visit a doctor in the United Kingdom, in sharp contrast with the US evidence reported in Table 3. Moroever, this result is robust to several different methodologies—OLS column 1; IV column 2; IV with individual fixed effects column 3; and Arellano-Bond procedure with autoregressive residuals column 4. The coefficient of the log of *Job Tenure* is never negative and statistically significant in any of these specifications.[23]

### C. *Alternative Instruments for Retirees' Job Tenure*

As we highlighted in Section IVB, in our analysis of retirees' medical expenditures, we cannot employ individual fixed effects/first differences to purge unobserved individual characteristics because the independent variable log (*Past Tenure*) has almost no variation within individuals, as individuals are retired. Conversely, the variation exploited by the instruments—i.e., census division of birth—appears truly exogenous to the individual and, thus, uncorrelated with individual characteristics.

Nevertheless, to verify the robustness of the results, we have rerun the regressions on retirees' medical expenditure using a different set of instruments. Specifically, the HRS reports the Census division of birth of each individual. Our instruments exploit the variation in employment protection across Census divisions and the variation in the

[23] The number of observations varies across specifications (1)–(4) because the IV specification with individual fixed effects of column 3 requires individual to appear in at least two surveys, and the Arellano-Bond specification with autoregressive residuals of column 4 requires individual to appear in at least three surveys. The OLS specification (1) performed on the same sample as the IV specification (3) yields almost identical results.

Table 6—Falsification Test: The Relationship between Workers' Job Tenure and Individual Doctor Visits in the United Kingdom

| | OLS (1) | IV (2) | IV with Fixed Effect (3) | Arellano-Bond with AR(1) Residual (4) |
|---|---|---|---|---|
| Log (JOB TENURE) | 0.008*** | 0.011 | −0.011 | 0.111 |
| | (0.001) | (0.018) | (0.019) | (0.101) |
| Age | 0.007* | 0.007*** | 0.016* | 0.022 |
| | (0.004) | (0.003) | (0.009) | (0.048) |
| Age squared | $3.5 \times 10^{-5}$ | $3.6 \times 10^{-5}$ | $-3.4 \times 10^{-6}$ | −0.0002 |
| | $(9 \times 10^{-5})$ | $(7 \times 10^{-5})$ | $(1.4 \times 10^{-7})$ | (0.001) |
| Education | 0.002*** | 0.002*** | ... | 0.002 |
| | (0.0006) | (0.001) | | (0.002) |
| Income/10,000 | 0.007*** | 0.007*** | −0.002 | −0.001 |
| | (0.0009) | (0.0006) | (0.001) | (0.003) |
| Male | 0.155*** | 0.155*** | ... | 0.134*** |
| | (0.002) | (0.002) | | (0.005) |
| Married | −0.005 | −0.005 | 0.0003 | −0.006 |
| | (0.003) | (0.004) | (0.007) | (0.009) |
| Household size | 0.002 | 0.002** | 0.002 | −0.0001 |
| | (0.001) | (0.001) | (0.002) | (0.003) |
| Union | −0.015*** | −0.016*** | −0.007 | −0.012 |
| | (0.005) | (0.004) | (0.006) | (0.011) |
| $\rho$ | ... | | ... | 0.130*** |
| | | | | (0.007) |
| Observations | 94,015 | 94,015 | 93,709 | 75,955 |
| Panels | | | 15,931 | 15,760 |

*Notes:* The dependent variable in each specification is a dummy variable taking a value of one if the individual did not visit a doctor in the last year, and zero otherwise. All regressions also include age cubed, income squared, race, year fixed effects, and geographic region fixed effects. Their coefficients are not reported.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

effects of protection across workers with heterogeneous, predetermined characteristics—i.e., education, gender, and age. More precisely, David Autor, John Donohue, and Stewart Schwab (2006) investigates the labor-market impacts of wrongful-discharge protections adopted by US state courts between 1972–1999. They find that one doctrine—the implied contract exception—reduced employment and that the short-term impact was most pronounced for demographic subgroups that change jobs most frequently: female, younger, and less-educated workers. Thus, arguably, the implied contract exception affected *Past Tenure*, and the effect was different according to individual characteristics, such as education, gender, and age. Autor, Donohue, and Schwab (2006) construct an annual panel, reporting whether each state implemented the implied contract exception. Since the HRS reports the Census division, we take the average of all states within a Census division for the years 1980 and 1990, and we further construct interactions of these two instruments with: the age of the individual in 1980 and 1990, respectively; a binary indicator equal to one if the individual reports more than 13 years of education, and zero otherwise; and a binary indicator equal to one if the individual is a male, and zero otherwise.

Panel A of Table 7 presents the results for (the log of) medical expenditures of individuals in the HRS sample, confirming that the coefficient of log(*Past Tenure*) in

Table 7—The Relationship between Retirees' Past Job Tenure and Medical Expenditures (Panel A) and Health Status (Panel B)—Alternative Instruments

| | *Panel A. Log individual medical expenditure* (1) | *Panel B. Health status* (2) |
|---|---|---|
| Log (JOB TENURE) | −0.746** | −0.430*** |
| | (0.356) | (0.063) |
| Age | 0.222 | −0.234** |
| | (0.442) | (0.109) |
| Age squared | −0.001 | 0.003** |
| | (0.005) | (0.001) |
| Education | 0.033*** | −0.011*** |
| | (0.010) | (0.002) |
| Total assets/1,000,000 | −0.052 | −0.013** |
| | (0.033) | (0.006) |
| Male | 0.441*** | 0.228*** |
| | (0.169) | (0.031) |
| Married | −0.066** | 0.010 |
| | (0.033) | (0.009) |
| Household size | 0.011 | 0.001 |
| | (0.016) | (0.041) |
| Observations | 27,229 | 27,229 |
| Panels | 10,395 | 10,395 |

*Notes:* The dependent variable for column 2 is a dummy variable taking a value of one if the retiree reported being in the lowest two categories of self-reported health i.e., fair and poor. The estimated equations also contain age cubed, total assets squared, year, race, and census division fixed effects. Their coefficients are not reported. Standard errors in parentheses.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

column 1 is negative and significant, as Proposition 2 predicts. Moreover, the economic significance of the coefficient is large. The magnitude of the coefficient of log(*Past Tenure*) in column 1 means that increasing *Past Tenure* by 10 percent decreases the annual medical expenditures by approximately 7.5 percent. Panel B reports the coefficient of a regression in which the dependent variable is equal to one if the retiree reported being in the lowest two categories of self-reported health (i.e., fair and poor), and zero otherwise. The coefficient estimate of log(*Past Tenure*) is negative and statistically significant at the 1 percent level. The magnitude of the coefficient indicates that the probability of a retiree reporting poor health decreases by approximately four percentage points when his job tenure prior to retirement increases by 10 percent.

The results reported in Table 7 are remarkable: they are very similar to the results in Table 4, although they came from a very different set of instruments. The magnitudes are slightly larger, but the hypothesis of identical coefficients of log(*Past Tenure*) cannot be statistically rejected.

## VI. Alternative Hypotheses

We now consider several alternative hypotheses. For most of these alternatives, we discuss how our empirical model allows us to distinguish the implications of our model from other plausible explanations. The analysis confirms and strengthens the previous findings.

*Good Jobs versus Bad Jobs*.—Several papers document true wage differentials across industries and jobs, as well as a negative correlation between wage differentials and quit rates (e.g., John Pencavel 1970; Alan Krueger and Lawrence Summers 1988; Robert Gibbons and Katz 1992). If workers are less likely to leave "good jobs" than "bad jobs"—good jobs offer higher wages and richer benefits, including health insurance—and if health insurance and health expenditures are related—in the data, they are—then differences between good jobs and bad jobs could imply a positive correlation between job attachment and health expenditures.

However, as we described in Section IVA, our empirical model on workers' medical expenditures is designed to control precisely for fixed and for persistent unobserved effects that may induce different workers to select into different jobs/industries. Moreover, the instruments that we employ in the empirical analysis on workers' medical expenditures exploit demand-side (i.e., firms) variation in turnover across regions and industries, precluding any reverse-causality hypothesis based on supply-side (i.e., workers) variation in quits. Similarly, the instruments we use in the empirical analysis on retirees' health expenditures exploit variations in plant closures across individuals' regions of birth, as well as the heterogeneous effect of these plant closures across individuals with heterogeneous, exogenous characteristics.

*What about Job-Lock?*—An influential literature shows that the employment-based health insurance system provides inefficiently low separation between mismatched workers and firms (Madrian 1994; Gruber and Madrian 1994, 1997, 2002). This "job-lock" literature postulates that workers are less likely to leave jobs that offer health insurance than to leave comparable jobs without health insurance. If health insurance and health expenditures are related—and in the data, they are—these hypotheses could also imply a positive correlation between job attachment and health expenditures.

We emphasize, however, that our empirical analysis is designed to circumvent this reverse-causality hypothesis (and others). Moreover, if job-lock were the *only* mechanism at work in the data, we would expect individuals with worse health to select jobs with more generous health benefits since less healthy workers presumably benefit more from them. Thus, in steady state, we should expect to find less healthy workers in jobs with lower turnover. Instead, Panel B of Table 4 shows that the opposite is true: healthier individuals were working in lower-turnover jobs.

In summary, we believe that wage differentials and job-lock are well suited to addressing the question of why mobility differs across individuals and jobs. However, we think that they cannot explain the empirical patterns in health care expenditures that are the focus of our paper. Most likely, they are valid explanations for complementary facts but do not provide alternative interpretations of all the empirical findings in this paper.

*Is Health More Important in Jobs that Also Have Higher Attachment?*—The empirical relationship between job attachment and health expenditures could simply be due to the fact that health is more important in industries that have higher job attachment.

The evidence from UK workers does not substantiate this claim, however. The results reported in Table 6 indicate that UK workers with longer job tenures are not more likely to visit a doctor, in sharp contrast to the results for US workers reported in Panel B of Table 2. This difference in health care utilization between the United Kingdom and the United States is also in stark contrast with many labor-market patterns—i.e., wages and inequality—that are remarkably similar in the two countries (Katz, Loveman, and Blanchflower 1995; Amanda Gosling and Thomas Lemieux 2004).

*What about Myopic Workers?*—A potential explanation of our empirical findings has to do with different wage profiles over time. If, in high-turnover jobs, wage profiles are flatter (relatively higher earlier, slower growth later), more-myopic people choose these jobs, attracted by the higher initial wage. These people are likely to have a different intertemporal discount—i.e., they value today much more than tomorrow. This could explain why their health expenditures are lower.

This explanation is not consistent with current theories of human capital, however. *General* human capital *steepens* wage-tenure profiles because workers must pay, in the form of lower wages, for any training that is general and, thus, transferable across employers. Early in their career, workers receive lower wages and investment in human capital. When human capital begins to increase productivity later in their career, workers have higher earnings. Because general human capital is transferable, firms must pay workers their full marginal product in the postinvestment period. Conversely, any type of *specific* human capital *flattens* wage-tenure profiles because the firm makes a specific investment but recoups its investment later, once the workers are locked in. Indeed, this is exactly what the extension of our model presented in online Appendix C.1 predicts.

Moreover, in high-turnover jobs, the relative importance of specific human capital is presumably lower than that of general human capital. As a result, we should expect high-turnover jobs to have steeper, not flatter, wage profiles (as this alternative explanation needs). Indeed, Crocker and Moran (2003) provide empirical evidence consistent with these predictions of human-capital theories. In the wage regressions reported in Table 2 of their paper, they find that returns to tenure are higher in high-turnover jobs.

*Is It a Pure Wealth Effect?*—Another potential explanation is a pure wealth effect. If wages are higher in low-turnover jobs, then a simple wealth effect might explain why health expenditures are higher in low-turnover jobs. Indeed, Robert Hall and Charles Jones (2007) argue that the growth of health spending in the past half-century is a rational response to the growth of income per person. According to their model, health spending is a superior good with an income elasticity well above one.[24]

Clearly, our explanation and that of Hall and Jones are not mutually exclusive. Hall and Jones focus on the growth of expenditures in the last 20 years, while we focus on the intertemporal profile of expenditures. However, we believe that the wealth effect cannot fully explain a number of our cross-sectional results. First, all our regressions on workers' medical expenditures include workers' current

[24] Acemoglu, Finkelstein, and Matthew Notowidigdo (2009), *however*, use oil-price shocks and cross-sectional variation in the oil reserves across different areas of the United States and find that the income elasticity of health expenditures is almost always less than one.

income and the best proxy for permanent income—i.e., education. Moreover, in the regressions on retirees' medical expenditures, we find that the coefficient of total assets is negative, and not significant. Thus, we have no evidence that wealthier retired individuals spend more on health.

## VII. Conclusion

In this paper, we investigate how the employment-based health insurance system in the United States affects individuals' life cycle health care decisions. We take the viewpoint that health is a form of human capital that affects workers' on-the-job productivity, and we derive implications of employees' turnover for the incentives to undertake health investment. Our model suggests that employees' turnover leads to dynamic inefficiencies in health investment. In particular, it suggests that the employment-based health insurance system may lead to an inefficient, low level of individual health investment during individuals' working lives. Moreover, we show that underinvestment in health is more severe when workers' turnover rate is higher, and this leads to increased medical expenditures during retirement.

We present a model that makes this process explicit and then investigate its empirical relevance using data from the Medical Expenditure Panel Survey and the Health and Retirement Survey. We document a large number of empirical patterns, all consistent with our model. Moreover, the magnitude of our estimates suggests a significant degree of intertemporal inefficiencies in health investment in the United States. Our back-of-the-envelope calculations suggest that, on average, one dollar of medical expenditures during the working years may decrease medical expenditures during retirement by about 2.8 dollars.

## Appendix: Arellano and Bond (1991) Method and Synthetic Cohorts

If the unobservables in equation (7) have a persistent component—i.e., $\epsilon_{jirt} = \rho\epsilon_{jirt-1} + \nu_{jirt}$—then fixed effects or first differences are not sufficient to eliminate the persistent component of the error term, as $\Delta y_{jirt} = \beta\Delta Z_{jirt} + \Delta\eta_{rt} + \Delta\epsilon_{jirt}$, where $Z_{jirt}$ is the set of all control variables—i.e., $Z_{jirt} = \left(\log\left(\textit{Job Tenure}_{jirt}, X_{jirt}\right)\right.$. However, the Arellano and Bond (1991) procedure is specifically designed to handle persistent unobservables in panel data. Arellano and Bond suggest subtracting $\rho y_{jirt-1}$ from $y_{jirt}$ to eliminate $\epsilon_{jirt} - \rho\epsilon_{jirt-1}$, leaving only the innovation $\nu_{jirt}$ of the unobservable

$$y_{jirt} = \rho y_{jirt-1} + \beta Z_{jirt} - \rho\beta Z_{jirt-1} + (1 - \rho)\zeta_i + \eta_{rt} - \rho\eta_{rt-1} + \nu_{jirt}. \tag{9}$$

Taking first differences, the following equation obtains:

$$\Delta y_{jirt} = \rho\Delta y_{jirt-1} + \beta\Delta Z_{jirt} - \rho\beta\Delta Z_{jirt-1} + \Delta\eta_{rt} - \rho\Delta\eta_{rt-1} + \Delta\nu_{jirt}. \tag{10}$$

In the differenced form, however, the new errors $\Delta\nu_{jkit}$ are correlated with the differenced lagged dependent variable $\Delta y_{jit-1}$ by construction, and, potentially, with the variables $\Delta Z_{jkit}$ and $\Delta Z_{jkit-1}$, as well. Therefore, a vector $W$ of instruments is required to construct moments $E(\Delta\nu_{jkit} \times W)$, and to estimate equation (10) via generalized method of moments (GMM). Arellano and Bond use the lagged values $y_{jkit-h}$ and $Z_{jkit-h}$ with lags $h \geq 2$ as instruments for $\Delta y_{jkit-1}$ and $\Delta Z_{jkit-l}$, $l = 0,1$, respectively, as the new error term $\Delta \nu_{jkit}$ is uncorrelated by construction with lags of order higher than two. These instruments, $y_{jkit-h}$ and $Z_{jkit-h}$ with lags $h \geq 2$, are "mechanically" correlated with the potentially endogenous variables $\Delta y_{jkit-1}$ and $\Delta Z_{jkit-l}$. Hence, following Arellano and Bond, we can use $y_{jkit-h}$ with lags $h \geq 2$ as instruments for the lagged endogenous variable $\Delta y_{jkit-1}$. Moreover, we follow Arellano and Bover (1995) and Blundell and Bond (1998), who suggest adding the original equation (9) in levels to the GMM criterion, instrumenting the endogenous variables in levels with first differences of the instruments. The SUSB data provide us with instruments for the main endogenous variable—*Job Tenure*—that have a stronger economic content than Arellano and Bond's instruments—i.e., instruments that shift the endogenous variable for plausibly exogenous reasons. In particular, we use lags (of order higher than two) of the variables described in Section IVA as instruments for *Job Tenure*, since lags purge any undesired correlation with $\Delta \nu_{jkit}$, the first difference in the innovation in the unobservables.[25] Unfortunately, the panel component of MEPS is too limited (two years) to use the Arellano-Bond procedure on individual data. Since the procedure has the attractive feature that it allows us to control for persistent unobserved heterogeneity—including, for example, any industry-region–specific trend that could threaten the validity of our exclusion restriction—we use MEPS data to construct synthetic panels. As in all papers that use synthetic panels, the definition of a cohort is arbitrary. In our case, we are constrained by the sample size of each MEPS survey and by the limited geographic and industry information available in the public version of the MEPS. As a result, we choose to define cohorts by grouping people by sex, decade of birth, one-digit industry, and census region. With a slight abuse of notation, we can write the cohort version of the empirical model defined by equations (7) and (10) as

$$y_{jt} = \beta_0 + \beta_T \log(\textit{Job Tenure}_{jt}) + \beta_X X_{jt} + \eta_{rt} + \zeta_j + \epsilon_{jt} \tag{11}$$

$$\Delta y_{jt} = \rho \Delta y_{jt-1} + \beta \Delta Z_{jt} - \rho \beta \Delta Z_{jt-1} + \Delta \eta_{rt} - \rho \Delta \eta_{rt-1} + \Delta \nu_{jt}, \tag{12}$$

where the subscript $j$ now denotes a cohort, for which industry $i$ and region $r$ are fixed over time. The dependent variable $y_{jt}$ is again one of the outcomes of interest for cohort $j$ (working in industry $i$ in region $r$) in year $t$. Similarly, $\textit{Job Tenure}_{jt}$ is the average number of years individuals in cohort $j$ have been employed in their current firm; $X_{jt}$ is now the cohort-average of a large set of control variables: the average age of individuals in the cohort, age squared, age cubed, average education, annual income, annual income squared, size of the family, fraction of whites, and fraction of blacks; $\eta_{rt}$ is, as before, a year fixed effect for each region $r$; $\zeta_j$ is now a fixed effect for cohort $j$ (again, working in industry $i$ in region $r$); and $\epsilon_{jt}$ is an unobservable, autoregressive component with innovation $\nu_{jt}$ —i.e., $\epsilon_{jt} = \rho \epsilon_{jt-1} + \nu_{jt}$.

[25] As Blundell and Bond (1998) demonstrates, the Arellano and Bond procedure does not work well if the dependent variables are very persistent. However, this does not appear to be a concern in our case, as our dependent variables exhibit year-to-year variation. Similarly, our main explanatory variable, *Job Tenure*, and its instruments (death rate of establishments and the fraction of workers that lost their jobs due to establishment deaths) exhibit substantial variation.

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