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Retirement Lock and Prescription Drug Insurance: Evidence from Medicare Part D[†]

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I examine whether lack of an individual market for prescription drug insurance causes individuals to delay retirement. Exploiting the 2006 introduction of Medicare Part D, which subsidized drug insurance for Americans over age 65, I use a triple-differences design that compares labor outcomes of individuals with retiree health insurance up to age 65 to those with insurance for life, before and after age 65, before and after 2006. I find that those with benefits only to age 65 decreased full-time work by 8.4 percentage points, of which 70 percent was due to transitions to part-time work. (*JEL* G22, H51, I13, I18, J14, J26)

Do Americans work for the purpose of maintaining employer sponsored health benefits? This question has been the focus of substantial literature, with a wide range of answers. In the context of prescription drug insurance, before Medicare Part D's introduction, Medigap and Medicare Advantage policies covering drugs for those over 65 offered limited coverage and were rarely taken up (Pauly and Zeng 2004), while the majority of Americans acquired their health and drug insurance through an employer (virtually all employer plans cover prescription drugs).[1] Thus, individuals dependent on their employers for insurance may have been "retirement locked": prevented from optimally retiring due to this extraneous consideration. The extent of retirement lock is important for many reasons, not least its role in the

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[1] In 2002, only 12 percent of Medicare beneficiaries had drug coverage through Medicare Advantage plans (Cubanski et al. 2005). In 2005, only 3.2 percent of Medigap policyholders in federally standardized plans chose plans offering any drug coverage (America's Health Insurance Plans 2006). In 2017, 99 percent of employer plans also covered prescription drugs (Kaiser Family Foundation 2017), unchanged from earlier years.

design of policies, such as the Affordable Care Act, which weaken the link between employment and insurance, impacting both the benefits of such policies and their costs.

This paper addresses the question of retirement lock by exploiting the quasi-experiment generated by the 2006 introduction of Medicare Part D. Part D expanded traditional Medicare in 2006 to give everyone over age 65 access to subsidized prescription drug insurance, inducing a sharp change in the incentives of individuals regarding whether to retire. Whereas before 2006, prescription drug insurance was available almost exclusively through employer sponsored insurance (ESI), after 2006, it became publicly available to everyone over age 65.

I examine the effect of Part D on labor supply using a triple-differences design. I estimate the causal effect of Part D by comparing labor outcomes of individuals reaching age 65 before 2006 to those reaching age 65 after 2006, with a control group of individuals who had retiree health insurance (RHI) for life. I find that eligibility for Part D substantially decreased the labor supply of those who would have previously been dependent on continued work for drug insurance.

To focus on individuals who were potentially retirement locked, I use the Health and Retirement Study (HRS) to consider only those who had RHI, and divide this population into two groups: a "treatment group" of those who would be covered by their RHI *only until age* 65, and a "control group" who would have RHI *after age* 65 as well. For the former, retiring implied a loss of drug coverage at age 65 before 2006, but no such constraint existed after 2006. The control group was not retirement locked before or after 2006, so Part D should not change their retirement decisions. Including this control group in the analysis group can absorb secular changes in labor markets specifically affecting the cohort of individuals attaining age 65 after 2006, such as changes in Social Security's full retirement age (FRA) or age-specific demand shocks during the Great Recession. Furthermore, if relaxation of retirement lock is the mechanism by which the labor of the treatment group is affected by Part D, it should exhibit a reduction in labor at age 65 in 2006, while there should be no change for the control group (in the absence of other shocks).

I find results consistent with these predictions. Those in the treatment group reduced their rate of full-time work by 8.4 percentage points more at age 65 after 2006 than they did at age 65 before 2006; for the control group I observe a statistically insignificant *increase* in full-time work. Over a baseline of 35 percentage points of full-time work, this amounts to a 24 percent reduction in full-time work upon eligibility for Part D among the treated. This drop in full-time work was largely composed of an intensive margin response of a shift to part-time work—an increase of 5.9 percentage points—with only a 2.5 percentage point reduction in any work. Furthermore, the entire effect is concentrated among individuals with chronic health conditions, who were more plausibly retirement locked to begin with due to their likely higher valuation of insurance.

To interpret this effect, I compare the reduction in labor supply due to Part D to the effect of an increase in Social Security benefits (as estimated in previous literature). I find that a $1 subsidy to drug insurance leads to a labor response equivalent to $5 of Social Security benefits. The substantial estimated response to the relaxation of retirement lock suggests potential inefficiency in the individual drug insurance market in the absence of Part D, and implies a willingness to pay of $5 for every dollar of the subsidy among retirees.[2]

The triple-differences design allows me to account for myriad changes that might otherwise affect the labor supply of 65-year-olds, such as health status and age-dependent factors (e.g., private pensions and Social Security benefits). The identifying assumptions in this reduced-form approach (e.g., what is a relevant control group of those not affected by the policy change) are different from and complementary to the assumptions of past efforts to structurally estimate the effect of health insurance availability on retirement behavior.[3]

My reduced-form approach is most closely related to a number of previous papers that look at quasi-experiments estimating conceptually similar effects.[4] An important source of variation in this literature has been continuation of coverage laws (COBRA). This literature tends to find significant effects of relatively small magnitude (Madrian, Burtless, and Gruber 1994; Gruber and Madrian 1995). However, the variation induced by COBRA can only identify the effect of a year or two of continued coverage; and the law still requires individuals to pay for coverage with after-tax dollars, making it less generous than ESI. Thus, both within the structural and reduced-form attempts to estimate the extent of retirement lock there have been inconclusive results (Gruber and Madrian 2004).

Some recent papers estimate the effect of public health insurance on employment for prime-age workers: Baicker et al. (2014); Garthwaite, Gross, and Notowidigdo (2014); and Dague, DeLeire, and Leininger (2017) use exogenous changes in Medicaid, while Boyle and Lahey (2010) considers changes to the Veterans Affairs system. These papers come to different conclusions, with the latter three finding employment lock of varying magnitudes and the former finding only small and insignificant effects. In the case of both Garthwaite, Gross, and Notowidigdo (2014) and Dague, DeLeire, and Leininger (2017), results indicate that among prime-age workers older individuals react more strongly to the availability of Medicaid, raising the possibility that those affected by Part D, who are over 65, might have been even more job locked than prime-age populations.

To put the results of the current paper in context, the estimates in Boyle and Lahey (2010) imply that expansion of public insurance leads to a decline of 1.3 percentage points in full-time work (a lower bound of the true effect due to limited take-up). My estimates are larger, consistent with my sample being older

[2] Recent work estimates Part D reduced mortality among beneficiaries, which might contribute to this high willingness to pay (Huh and Reif 2017). It is worth noting that longer life expectancy might induce individuals to work *longer* to save for longer retirements, countervailing the job-lock reduction of Part D. My estimates are of the reduced-form effect of Part D, inclusive of its entire "package" of costs and benefits, and the welfare analysis assumes individuals optimize with this entire package in mind.

[3] There is a rich literature structurally estimating the effect of ESI on retirement. The conclusions of these papers are diverse, with some finding little effect (e.g., Gustman and Steinmeier 1994; Lumsdaine, Stock, and Wise 1994), while others find significant effects (for example, Rust and Phelan 1997, Blau and Gilleskie 2006, French and Jones 2011).

[4] A number of reduced-form analyses not relying on quasi-experiments are also relevant, including Karoly and Rogowski (1994), Rogowski and Karoly (2000), Blau and Gilleskie (2001), Nyce et al. (2013), and Shoven and Slavov (2014). These studies tend to find large effects of ESI on retirement. The current paper's identification strategy reinforces those results by circumventing some of the concerns raised by lack of exogenous variation in insurance coverage, such as potential correlation of ESI with employer pension plans, or selection of individuals with particular preferences into matches with employers who provide ESI (Gruber and Madrian 2004).

and having chosen employers who provide RHI. The particular responsiveness of this group is further underscored by the fact that the effect I find is entirely driven by those with chronic health conditions, who experience a 12.3 percentage point decline in full-time work. On the extensive margin of work, my estimate of a 2.5 percentage point decline in any work is similar to the estimates in Garthwaite, Gross, and Notowidigdo (2014) and Dague, DeLeire, and Leininger (2017) (ranging from 2.5 to 5.3 percentage points) and slightly larger than the 1.6 percentage point estimate in Baicker et al. (2014); however large standard errors do not permit rejecting a null of no effect on the extensive margin.

My approach to welfare is similar to that of Gruber and Madrian (1995), who benchmark the scale of retirement lock by comparing its impact to retirement wealth. They find that one year of continuation of coverage has the same effect on retirement as \$13,600 of pension wealth, substantially higher than the \$3,600 they estimate to be the cost of such coverage. I formalize this comparison in a way which allows identification of both the distortion in labor supply induced by the inefficiency of the individual insurance market, and the willingness to pay of individuals for correcting this inefficiency. Such inference of welfare from labor market responses is related to Shimer and Werning (2007), Chetty (2008), Hendren (2016), and Fadlon and Nielsen (2019).

This paper also contributes to the literature on Part D itself. An overview of early results on the structure and the effects of the program is available in Duggan, Healy, and Morton (2008). A great deal of research quantifies the effect of Part D on health expenditures and outcomes: for example, Engelhardt and Gruber (2011) finds that the program increased prescription drug coverage and utilization among the elderly, while reducing their out-of-pocket spending. They estimate the welfare benefits of Part D by focusing on the gains due to increased insurance. These same authors also estimate large crowd-out of private insurance by the new program, cases in which there was no net gain in insurance.[5] This paper's results are consistent with those estimates of crowd-out, and complement them by considering gains in welfare precisely among those whose private (employer) prescription drug insurance is crowded out by Part D. Rather than the null effect on welfare implied by the idea of crowd-out, I show substantial welfare gains from Part D; these gains accrue mostly along the margin of avoided labor disutility, rather than of a less risky distribution of health expenditures.

The structure of the paper is as follows: Section I describes a simple conceptual framework of retirement lock in the context of drug insurance. Section II provides the institutional details of Part D. Section III describes the data and the identification strategy. Section IV contains the main empirical estimates of retirement lock. Section V discusses how the reduced-form estimates pertain to retirement lock. Section VI concludes.

[5] Levy and Weir (2009) finds less conclusive evidence of crowd-out, although the authors do estimate that employer coverage decreased faster between 2004 and 2006 than between 2002 and 2004. The authors do not rule out that some individuals might replace ESI with Part D coverage, which is even more likely in the years following 2006 due to labor market frictions. A large related literature finds an increase in utilization of prescription drugs due to Part D, including, for example, Lichtenberg and Sun (2007); Kaestner, Schiman, and Alexander (2019); Abaluck, Gruber, and Swanson (2018); Ayyagari and Shane (2015); and Wettstein (2018).

I. Conceptual Framework of Retirement Lock

This section develops a model of labor supply in the presence of a drug insurance market that allows for subsidization of retiree drug insurance (as through Part D). I also formally define "retirement lock" as a distortion in labor supply due to inefficiency in the individual insurance market, and show that a negative labor effect of the policy is not, in itself, evidence of retirement lock. I then provide a test of retirement lock comparing the effect on labor of a subsidy to individual market insurance to that of retirement income.

Individual Optimization.—Individual i's preferences over consumption ($c_i \in R_+$) and labor ($l_i \in \{0,1\}$) are given by a separable utility function $U_i(c_i,l_i) = u(c_i) - v_i \times l_i$ with $u'(c) > 0$ and $u''(c) < 0$. Because the relevant margin of labor in this context is full- versus part-time work, as most ESI is available only to full-time workers, $l_i = 1$ indicates full-time work and $l_i = 0$ otherwise, and v_i is the individual's disutility from work. Note that v_i is privately observed by the individual, and distributed according to a cumulative density function $G(v)$, with a probability density function $g(v)$.

The individual's income, $I(l_i)$, satisfies $I(0) < I(1)$, and $I(0) = I_0 + b$, where b signifies a government pension such as Social Security benefits.[6] Individuals also face stochastic drug costs, Y_i, against which they can purchase insurance at the quantity of x_i, yielding out-of-pocket costs $y(Y_i,x_i)$, which are assumed to be twice differentiable and strictly concave in x_i.

Insurance is purchased at a per unit price, $p(l_i)$, satisfying $p \equiv p(1) < p(0) \equiv P$, allowing individual insurance to be more expensive or of poorer quality than ESI.[7] In addition, I consider a stylized policy like Part D, subsidizing individual insurance by $\sigma(l_i)$: $\sigma(1) = 0, \sigma(0) = s$. Thus, the consumer price on the individual market for a unit of insurance is $P - s$, while the ESI price is p.[8]

Each i chooses l_i and x_i after observing v_i but prior to observing Y_i. Thus, c_i is a function of i's choices and the realization of Y_i given by $c_i = I(l_i) - y_i(Y_i,x_i) - (p(l_i) - \sigma(l_i)) \times x_i(p(l_i) - \sigma(l_i))$.

Focusing first on the choice of c_i, denote by c^*_{1i} and c^*_{0i} the values of consumption maximizing i's utility when optimally choosing to work or to retire, respectively.

[6] For simplicity, I assume homogeneous individual preferences and income levels in work and retirement.

[7] The price of individual insurance might be higher than ESI for a number of reasons. First, health insurance markets tend to suffer from adverse selection (e.g., Hackmann, Kolstad, and Kowalski 2012; Hendren 2013). This is particularly true of prescription drug insurance, due to the persistence of drug expenditures (Pauly and Zeng 2004). Such selection may be weaker at the employer group level than at the individual level. Second, there are fixed costs to contracting with an insurer due to administrative costs and the complexity of the choice problem. The latter may be particularly onerous for the elderly in the context of drug insurance (Abaluck and Gruber 2011). ESI may offer a simpler, curated, menu of options. Third, the exemption of ESI from the income tax leaves it cheaper in after-tax dollars than individual alternatives. Fourth, the difficulty of forming long-term insurance contracts that do not result in premium increases following a negative shock makes risk pooling an integral part of insurance (Cutler 1994).

[8] Part D also subsidized ESI at a lower effective rate from the individual's perspective, as the incidence of the subsidy is shared between the employer and the individual. For simplicity, I assume this subsidy to ESI was 0. What matters for this analysis is the differential subsidy.

Note that for any (s, b) there exists a threshold value of v, denoted by $\bar{v}(s, b)$, such that $l_i = 0$ for $v > \bar{v}(s, b)$.[9] This threshold is given by

$$\bar{v}(s,b) = E(u(c_{1i}^*) - u(c_{0i}^*)). \tag{1}$$

Benchmark Optimal Insurance Choice.—The optimal insurance choice conditional on working or retiring satisfies the following first-order condition:

$$\frac{dE(u(c_i))}{dx_i} = -E\left(u'(c_i) \times \left(\frac{\partial y_i}{\partial x_i} + p(l_i) - \sigma(l_i)\right)\right) = 0. \tag{2}$$

Individuals may be prevented from satisfying this first-order condition by additional constraints; for example, individuals with preexisting conditions may be prevented from purchasing insurance at all (Hendren 2013).

A Subsidy to Retiree Drug Insurance.—The change in the share of individuals working full time when the subsidy is increased is

$$\frac{dG(\bar{v}(s,b))}{ds} = -g(\bar{v}(s,b)) \times E\left(u'\left(c_{0i}^*\right) \times \frac{dc_{0i}^*}{ds}\right). \tag{3}$$

Labor declines with the subsidy regardless of inefficiency in the insurance market. To see this, note that $E\left(u'(c_{0i}^*) \times \frac{dc_{0i}^*}{ds}\right) = E\left[u'(c_{0i}^*) \times \left(-\frac{\partial y_i}{\partial x_i}\frac{dx}{ds} + x_i - (p(l_i) - \sigma(l_i))\frac{dx_i}{ds}\right)\right] = E(u'(c_{0i}^*)x) > 0$ for any positive quantity of insurance, where the last equality is due to equation (2).

Evidence of retirement lock requires a labor decline beyond what would result from a mere increase in retirement income due to the subsidy. To find such evidence, instead of increasing s we can increase b, so that the threshold labor disutility change is $d\bar{v}(s,b)/db = -E(u'(c_{0i}^*))$, leading to a change in the share of full-time workers of

$$\frac{dG(\bar{v}(s,b))}{db} = -g(\bar{v}(s,b))E\left(u_0'(s)\right). \tag{4}$$

Define the ratio of a change in labor due to an increase in the insurance subsidy to the change in labor due to an increase in retirement income (noting that an increase of 1 in s corresponds to an increase of x_i dollars, holding insurance constant):

$$R \equiv \frac{\frac{dG(\bar{v}(s,b))}{ds}\Big/x_i}{\frac{dG(\bar{v}(s,b))}{db}} = \frac{E\left(u'(c_{0i}^*) \times \frac{dc_{0i}^*}{ds}\right)\Big/x_i}{E\left(u'(c_{0i}^*)\right)}. \tag{5}$$

[9] In principle, individuals could be made indifferent between working and retiring if employers could freely wage discriminate (Gruber and Madrian 2004). Two frictions prevent this: the administrative cost of designing worker specific contracts; and preference revelation constraints, where employers do not know individual insurance and leisure valuations. There is some evidence that while employers can offset the value of benefits by reducing compensation for groups of workers (e.g., Gruber 1994), they cannot do so at an individual level (Chetty et al. 2011).

Evidence of retirement lock would be a "large" R. How large is helpfully illustrated by R's expected magnitude if individuals faced an efficient individual insurance market.

CLAIM: *In the presence of constrained efficient insurance markets, the effect of a dollar's worth of subsidy on labor supply is equal to the effect of a dollar of retirement income.*

PROOF:

Plugging in the first-order condition from equation (2) into equation (5) gives

$$(6)\quad R = \frac{\frac{dG(\bar{v}(s,b))}{ds}\Big/ x_i}{\frac{dG(\bar{v}(s,b))}{db}} = 1 - \frac{dx_i}{ds} \times \frac{E\Big(u'(c^*_{0i}) \times \Big(p(l_i) - \sigma(l_i) + \frac{\partial y_i}{\partial x_i}\Big)\Big)}{x_i E(u'(c^*_{0i}))} = 1.$$

This is intuitive: if markets are efficient, with internal solutions, then compensation provided in the form of insurance is equivalent to compensation in dollars, because insurance and money are fungible.[10] I define the distortion due to retirement lock, $R - 1$, to be the extent to which labor responds to the insurance subsidy above and beyond its response to equivalent retirement income. A positive value indicates individuals work more for a dollar's worth of insurance than for a dollar of income, inconsistent with an efficient insurance market. To tie this model to the estimates in Section IV, note that for a small change in s, the quantity $s\frac{dG(\bar{v}(s,b))}{ds} \approx \frac{\Delta G(\bar{v}(s,b))}{\Delta s}$, which is what I estimate in the empirical section.[11]

II. The Medicare Part D Program

This section provides institutional details regarding the Part D program: a 2006 change to traditional Medicare providing a subsidy to prescription drug insurance for individuals over age 65. These details inform the identification strategy detailed in the next section.

Medicare provides universal health insurance to Americans over age 65. When the program started in 1966, it did not cover prescription drugs. However, in the past 30 years, the share of health expenditures going toward drugs increased

[10] This is an application of Roy's identity. Note also that we can express R as: $R = \frac{\mathrm{cov}\left(u'(c^*_{0i}), \frac{dc^*_{0i}}{ds}\right) + E(u'(c^*_{0i})) \times E\left(\frac{dc^*_{0i}}{ds}\right)}{x_i E(u'(c^*_{0i}))}$. All else equal, the larger the covariance of marginal utility of consumption and the gain in consumption from increasing s, the greater the labor effect of the subsidy. This demonstrates the insurance value of the subsidy: individuals value it more the more it tends to increase consumption when marginal utility is otherwise high. Furthermore, the willingness to pay for a dollar of subsidy is the ratio of the value of such a dollar, $E\left(u'(c^*_{0i}) \times \frac{dc^*_{0i}}{ds}\right)\Big/ x_i$, to the value of a dollar of income, $E(u'(c^*_{0i}))$. This ratio is precisely equal to R, which therefore also measures the willingness to pay for the subsidy.

[11] The intuition that the equivalent variation of the subsidy can be estimated from the ratio of labor responses to the subsidy versus to retirement income can be derived from a simpler model, albeit one that provides less insight to the mechanisms at play. This is demonstrated in online Appendix E, without specifying that the change in s is small or imposing any structure on the insurance market.

substantially: in 1982 prescription drugs accounted for 4.5 percent of health expenditures, while by 2005 that share had more than doubled, to 10.1 percent (Duggan, Healy, and Morton 2008). To address the lack of insurance for such large expenditures among the elderly, the administration and Congress passed the Medicare Prescription Drug, Improvement and Modernization Act (MMA) which, beginning January 1, 2006, provided subsidized prescription drug insurance to everyone eligible for Medicare. Thus, from 2006 onward, every American over age 65 gained access to prescription drug insurance.

Part D allows anyone eligible for Medicare to choose between three subsidized insurance options: a stand-alone prescription drug plan; a Medicare Advantage plan offering the full range of Medicare benefits including prescription drugs; and the option of remaining on ESI, provided its drug coverage is at least as generous as the standard Part D plan.

Those choosing the option of staying on ESI still receive a subsidy.[12] This subsidy is intended to discourage employers from dropping coverage for elderly employees, and is noteworthy for interpretation of the results estimated below. It implies that the change in the insurance environment for individuals with ESI stems from introducing and subsidizing an alternative to ESI, not from a loss of ESI due to changes in the worker's compensation package as a result of the policy change.[13]

In sum, whereas before 2006 access to prescription drug insurance had been almost exclusively restricted to those with ESI, from 2006 onward everyone over age 65 had the option of purchasing subsidized prescription drug insurance. This change forms the basis of my identification strategy, to which I turn in the next section.

III. Data and Empirical Strategy

This section describes the data I use in the analysis and how I estimate the effect of Part D eligibility on labor supply. The rich data in the HRS provide detailed information on employment status, permitting differentiation of full-time and part-time work. They also allow identification of the insurance status of individuals, enabling construction of treatment and control groups for a triple-differences design. This design recovers the effect of Part D on labor supply and reveals the extent to which individuals work solely in order to retain their ESI.

The data I use are from the RAND version of the HRS (Chien et al. 2013).[14] The HRS is a longitudinal survey of roughly 20,000 Americans over the age of 50 and their spouses conducted every two years since 1992. As Part D began January 1, 2006, I restrict the sample to years 2000–2010. Because eligibility for Part D, as for Medicare in general, begins at age 65, I further restrict the sample to individuals

[12] This subsidy covers 28 percent of employer costs between the deductible of $310 and an upper limit of $6,350 in 2014, for a maximum subsidy of $1,691.

[13] There has been a long-term trend of employers offering less RHI since at least the 1980s; the share of employers offering health benefits to active workers who also offer RHI has fallen from 66 percent in 1988 to 25 percent in 2017 (Kaiser Family Foundation 2017). However, there was no sharp change in this trend around 2006, nor has there been any change in the share of employer plans covering prescription drugs.

[14] For information on prescription drug coverage and out-of-pocket spending, I refer to the raw HRS data (Health and Retirement Study 2013).

aged 55–68. The HRS asks questions about prospective RHI over age 65 only of respondents below age 65 at the time of the survey, and these questions were first asked in the 1996 wave of the survey. Those older than 68 in 2000 would have been too old to be asked these questions in any wave in which they were surveyed, and thus cannot be included in the sample. For more details, see the online Data Appendix.

Retirement lock is not expected to operate on all individuals. In particular, for those with RHI with no age limit, the retirement decision is divorced from considerations of prescription drug insurance. These individuals will have such insurance irrespective of whether they work or not. Similarly, individuals who have no ESI whatsoever should not be expected to have any labor supply response, as they will not have prescription drug insurance regardless of whether or not they work.

Because the HRS does not ask questions about RHI coverage over age 65 of respondents who are already over that age, I infer RHI status for those over 65 using lagged values from before age 65 for those first surveyed prior to that age. I complement this approach by inferring post-65 retiree coverage from leading values of coverage in practice among those over 65 and retired. Results are qualitatively similar using only the first approach.[15]

To estimate the effect of Part D on those affected by the new policy with respect to their labor supply, I define a "treatment" group of individuals who would have RHI from their employer *only until age* 65. Before 2006, such individuals could generally retire at any age before 65 and keep their health and prescription drug insurance. However, upon reaching age 65, they would have lost the latter. Non-prescription drug health insurance was guaranteed to them at that age by Medicare, but Medicare did not cover drugs. Therefore, if maintaining drug coverage were sufficiently important for them, members of the treatment group would have had to keep working, most likely at full time, or else lose coverage at age 65.

In contrast, from 2006 onward Medicare began to cover prescription drugs as well. As a result, members of the treatment group were released from the potential retirement lock imposed by their employer sponsored drug coverage in the past, and could choose when to retire without having to take into account loss of drug insurance. They could retire at any age and maintain continuous coverage of both health and drug insurance until age 65 (from their RHI) and from age 65 onward (when Medicare would provide both health and drug insurance).

This change in the chaining of the labor decision to availability of prescription drug insurance at age 65, in year 2006, motivates a difference-in-difference design for the treatment group. The average change in outcomes before 2006 for individuals over age 65 (ages 65–68) relative to individuals under age 65 (aged 55–64) reveals the life-cycle-driven changes in the outcomes at age 65. Two assumptions allow this design to identify the effect of Part D: parallel trends absent the treatment, and that those under age 65 are, in fact, untreated. Under the assumption of parallel trends between these groups in the absence of the program, comparing the mean

[15] Similar point estimates and statistical significance hold for the main specifications. However, significance is lost in specifications without individual fixed effects. See the online Data Appendix for a fuller description of the data construction and online Appendix Table 6 for results relying solely on the first method.

change at age 65 after 2006 (years 2006–2010) to the mean change that prevailed before 2006 (years 2000–2004) identifies the effect of the post-Part D period on individuals aging into eligibility for the program. This assumption is more credible given parallel trends before 2006 (see Section IV).

The second necessary assumption is that individuals under age 65 are truly untreated by the policy change—that Part D had no effect on their incentives to retire before they attained eligibility for it. If before 2006 people under age 65 continued working for the option value of having a job after age 65, which would provide drug coverage, then the estimator would understate the effect of Part D, as both those over and under 65 would reduce their labor supply in 2006.[16] Conversely, it is possible that, rather than reducing labor supply, the young group increased it in response to Part D, perhaps to accumulate more savings in response to longer life expectancy due to the program. The use of these younger individuals as a control group therefore assumes such effects are negligible relative to the effect on those immediately eligible for Part D coverage.[17]

Defining the treatment group as those who had RHI if and only if they were younger than age 65 suggests a natural control group: individuals who have RHI for life. Including this latter group in the analysis leads to a triple-differences design (as in, e.g., Gruber 1994), whereby the control group serves two purposes. The first is to absorb any residual labor market shocks post-2006 that might differentially affect individuals aged 65–68 and those aged 55–64, which would bias a simple difference-in-differences. This is of particular concern due to the inability to distinguish between cohort and age effects at a particular time. One concrete worry along those lines is that the Social Security FRA was increasing from age 65 to age 66 in the sample years, which might have raised retirement ages irrespective of Part D. This change, and other potential age-time specific changes (such as possible age-specific labor demand shocks during the Great Recession), would affect the control group as well and would be absorbed by their inclusion in the analysis. Hence, the triple-differences design strengthens the ability to attribute the estimated effect to Part D itself.[18]

The second role the control group could play is to demonstrate that Part D eligibility did not have a significant effect on the labor supply decisions of

[16] Such potential bias does not seem to be quantitatively important: full-time work rates in ages 55–64 *rise* after 2006, rather than fall, continuing long-term trends (see Section IV). Lack of forward thinking in the labor response to health insurance is consistent with recent literature on demand for health care, where individuals seem to neglect option value (e.g, Dalton, Gowrisankaran, and Town 2015 and Brot-Goldberg et al. 2017).

[17] See Huh and Reif (2017) for evidence of reduced mortality due to Part D. However, see also Kaestner, Schiman, and Alexander (2019), who find no significant effect of Part D on mortality. Any change in option value or life expectancy is likely to most strongly impact the labor choices of members of the young group who are closest to attaining Medicare eligibility. It is therefore reassuring that the main results are robust to excluding 63–64-year-olds from the sample (see online Appendix Table 14 in online Appendix B3).

[18] As an alternative to the assumption that there is little reaction to the option value of Part D by those below age 65, one can assume that the control group is a sufficient counterfactual to the treatment group without controlling for different time trends between the two groups pre-age 65. Under this assumption, one could estimate a difference-in-differences using *only* individuals over age 65, comparing the treatment and control groups before and after 2006. This alternative assumption is less consistent with the data: there is a larger increase in full-time work over time before age 65 for the treatment versus the control group. Nevertheless, for completion, this difference-in-differences is estimated in online Appendix B3. Qualitative results are similar, but of smaller magnitude due to these differential time trends, and statistically insignificant.

individuals who were not subject to retirement lock to begin with. By displaying no change in behavior at age 65 before and after Part D, the control group helps establish the mechanism of the effect on the treated: reduction in their labor supply can be more confidently attributed to Part D's relaxation of their retirement lock.

This third dimension of the triple differences rests on the assumption that selection of individuals into the treatment and control groups (i.e., their choice of a job whose RHI persists after age 65 or not) is exogenous to the Part D reform conditional on the other controls (including the age and time controls determining Part D eligibility).[19] This seems plausible given the difficulty of changing a compensation package at relatively short notice due to the new program (especially among workers already approaching retirement). In practice, for over 85 percent of individuals there is no change in RHI status at any point in the sample period. To assess robustness to this assumption, online Appendix B2 repeats the analysis using an alternative control group of individuals with no ESI, and finds similar estimates. Moreover, while I do not define treatment group based on lagged pre-2006 values in the main analysis to preserve sample size and to avoid measurement error, assigning treatment status based on 2002 values, before the introduction of the MMA in 2003, also yields similar point estimates, albeit slightly attenuated and with larger standard errors, potentially due to the additional measurement error involved in relying on lagged values for treatment group assignment. Full results are in online Appendix Table 6.

Estimation Equation.—The following equation will form the baseline specification for the analysis in the next section:

$$\begin{aligned} y_{i,t,a} = {} & \beta_0 + \beta_1 \times Post2006_t \times Over65_a \times Treat_i \\ & + \beta_2 \times Post2006_t \times Over65_a \\ & + \alpha_a + \gamma_t + \delta_a \times Treat_i + \zeta_t \times Treat_i + \mu_i + \mathbf{X}'_{i,t}\theta + \varepsilon_{i,t,a}, \end{aligned} \tag{7}$$

where i indexes individuals, t indexes years, and a indexes age.[20] The term $y_{i,t,a}$ is an outcome variable such as an indicator of full-time work; $Post2006_t$ and $Over65_a$ are indicators equal to 1 if and only if the observation is observed in year 2006 or later, and at age 65 or over, respectively; and $Treat_i$ is an indicator equal to 1 if and only if the individual would be eligible for RHI only until age 65. All specifications

[19] This is a weaker identification assumption than would be required if the effect were estimated purely off a difference between workers with and without RHI after age 65: in general, it seems likely that workers' selection into jobs with retiree benefits that expire at age 65 or not *is* related to their anticipated labor past age 65, rendering a simple comparison between the groups an imperfect method of estimating job lock (though highly informative, as in, for example, Nyce et al. 2013 and Shoven and Slavov 2014). Any bias in such a comparison may be exacerbated by the correlation between other unobserved employer benefits affecting retirement timing, such as the structure of pension plans, with the provision of RHI.

[20] *Treat* is mostly constant over time within individual and is therefore not conceptually indexed by t, although to the extent that it does vary, it is permitted to change. I include a time-varying indicator for treatment status to allow for those cases where treatment status changes within individual, in a way that is therefore not captured by the individual fixed effect. See online Appendix Table 6 for results holding *Treat* constant after age 65, or holding it constant after 2002.

also include a full set of age and year fixed effects (with age 68 in year 2010 as the comparison group), as well as their interactions with $Treat_i$. Some specifications further include μ_i, an individual fixed effect, and a vector of additional controls, $\mathbf{X}_{i,t}$. Thus, β_2 gives the estimated change in the outcome at age 65 after 2006 relative to before 2006 (the difference-in-differences) for the control group. The main parameter of interest, β_1, gives the triple-differences estimate of the effect of Part D on the outcome.[21]

Inclusion of individual fixed effects has the advantage of absorbing unobserved heterogeneity across individuals. However, it is inappropriate to control for fixed effects when the outcome variable is an absorbing state. In this paper, the main outcome is full-time work, and moving in and out of full-time work is quite common. Relative to those working full time (35 percentage points at the baseline), 22 percent of individuals who were not working full time at some point do so eventually (7.6 percentage points). Regarding full retirement, Maestas (2010) finds that 26 percent of retirees later "unretire." Nevertheless, to alleviate this concern, I estimate all main regressions both with and without fixed effects, and find very similar results. To account for unobserved heterogeneity without the fixed effects under the assumption that leaving full-time work *is* an absorbing state, I follow Kroft and Notowidigdo (2016) and estimate a hazard model (see online Appendix B3). This model estimates the probability a given individual will transition into retirement as a function of their RHI status, age, year, and the interactions of these variables, and allows for the same fixed (e.g., gender) and time-varying (e.g., self-reported health) controls as equation (7). The results of these hazard models have the same signs as the results of the main specifications, and their confidence intervals include the estimates implied by those specifications.

The controls in $\mathbf{X}_{i,t}$ are: an indicator for being single, indicators for each of the census divisions, and a fifth-order polynomial in nonhousing household wealth. Health controls are also included except where stated otherwise: indicators for self-reported health on a scale of 1–5 from poor to excellent; body mass index; and indicators for having any of the following physician diagnosed conditions: cancer, lung disease, heart disease, stroke, arthritis, or psychiatric conditions. All monetary variables are inflated to 2010 prices by the consumer price index. All standard errors are clustered at the individual level, and the main results are similar when clustering at the household level (see online Appendix B3).

The main outcomes of interest are a full-time work indicator and an indicator of part-time work. Individuals are considered full-time workers if they report working more than 35 hours a week for more than 36 weeks a year. If they work less than that, they are considered part-time workers. Hours from both main and secondary jobs are counted. In addition, some specifications have as their outcome an indicator of job switching: it is 1 if tenure with the current employer declines from more than two years to less than two years between two consecutive survey waves, and

[21] The HRS does not survey a random sample of the US population, but rather oversamples minorities and some states. Because individuals are sampled at different years and weighted to match different populations (based on the Current Population Survey) the results presented below are not weighted. However, results are similar when individuals are weighted by the HRS sampling weights at the wave when they were first sampled.

0 otherwise. This indicates a change of a relatively long-term employer at the finest resolution available in the biannual HRS survey. Labor earnings are also analyzed; they are constructed from the RAND variable on earnings summing wages and salaries, bonuses, overtime pay, commissions, tips, second job and military reserve earnings, and professional practice or trade income.

Descriptive statistics for the pre-Part D period are presented in online Appendix A. They show that the treatment and control groups are broadly similar on observable demographics and insurance coverage before Part D. The distribution of the treatment and control groups' occupations and industries (among those still working) are also similar, and there is no substantial change in these distributions from before Part D's introduction to after it.

IV. Estimation of Retirement Lock

This section provides the main analysis, starting with graphical results on full-time work. I then analyze the effect of Part D on full-time work, and decompose it into shifts to part-time work and full retirement, as well as examine the effect on job mobility and earnings. Following that, I perform some robustness checks to address potential labor demand shocks that might confound the estimates, particularly the Great Recession. Finally, I demonstrate heterogeneity in the treatment effect by health status consistent with relaxation of retirement lock.

A. *Graphical Evidence*

The left-side panel of Figure 1 depicts the full-time work rate of individuals in the treatment group at different ages. The squares show full-time work of individuals at the age along the *x*-axis before 2006. The circles show the corresponding values after 2006. Full-time work declines both before and after 2006 at age 65, and, to a lesser extent, at age 62. These drops correspond to eligibility for Social Security full and early retirement ages, respectively (at least in the early years of the sample).

Of particular interest, however, is the noticeably larger decline in full-time work at age 65 after 2006, relative to before 2006. Also of note is the parallel movement of the curves before 2006. The identifying assumption of difference-in-differences is that absent the treatment, treatment and control groups will move in parallel. These parallel pretrends provide support for this assumption.[22]

The central panel of Figure 1 displays the same data as the left-most panel, but adjusts the post-2006 data points so that they have the same mean among ages 55–64. This visually represents the first difference taken by the triple-differences design, nullifying the time trend for 65–68-year-olds in the treatment group, and demonstrating clearly the parallel pretrends before and after 2006 within the

[22] It is apparent in Figure 1 that post-2006 the level of full-time work is higher at ages 55–64 than it was in the years 2000–2004. While identification requires only parallel trends, not identical levels, one might be concerned as to what drives that difference in levels. In this case, there has been a long-term trend of increasing labor supply among the elderly since the mid-1980s, long before Part D. Due to this secular trend, the levels of full-time work were higher in 2006–2010 than they were in 2000–2004. This is not directly related to Part D, nor is it an artifact of the HRS data.

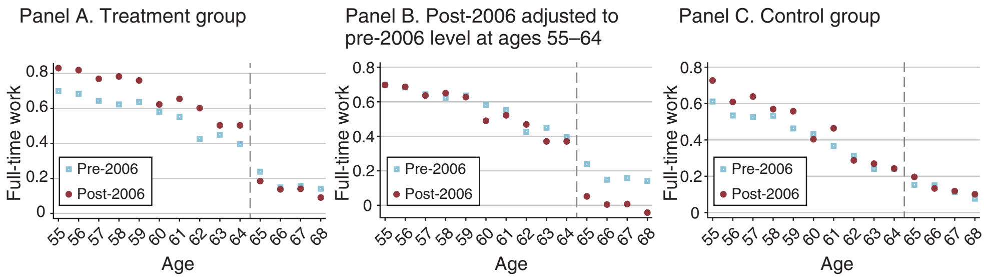

FIGURE 1. FULL-TIME WORK RATES BY AGE AND PRE- AND POST-PART D STATUS

Notes: This figure shows the difference-in-differences of full-time work for the treatment and control groups, in panels A and C, respectively. Panel B is identical to panel A, except that the post-2006 data points are adjusted so that their mean is equal for ages 55–64—a graphical representation of the first of the three differences composing the triple-difference design. The sample is individuals aged 55–68, in the years 2000 until 2010, who have RHI through their employer. The squares indicate rates of full-time work by age in the years 2000–2004, while the circles indicate full-time work rates by age for years 2006–2010. The dashed gray line differentiates between ages eligible for Part D, on the right, and those ineligible, on the left (in the post-2006 period).

treatment group. As with the regression estimates of the triple differences, the figure accomplishes this by using the difference in outcomes for 55–64-year-olds to construct the counterfactual difference in outcomes among 65–68-year-olds had no secular trends in the full-time work of near-retirees taken place between the 2000–2004 and 2006–2010 periods (assuming parallel counterfactual trends between the groups in the absence of Part D).

In contrast to the treatment group, the right-hand panel of Figure 1 repeats the same analysis for the control group. In this case, it is clear that the decline of full-time work after age 65 remained roughly the same before and after 2006. This validates the results for the treatment group: that the control group did not experience a decline in labor after age 65 in 2006 suggests there were no unobserved shocks driving the results for the treatment group. It is also reassuring for the interpretation of Part D's labor effect as one driven by relaxation of retirement lock: where there is no retirement lock, there is also no effect on full-time work.[23]

Figure 2 displays the dynamic triple-differences estimates of Part D's effect on full-time work. That is, for every year the treatment effect is estimated separately using only indicators for treatment group, being over age 65, year, and the interactions of all these indicators (with year 2000 as the reference year). Ninety-five percent confidence intervals are given by the dashed lines. The pre-2006 estimates can be considered placebo tests, and the null effects there are evidence of parallel trends pretreatment. The estimate at 2006 is an 8.7 percentage point decline in full-time work (marginally significant, $p = 0.064$). This effect

[23] Figure 7 in the online Appendix recasts the left- and right-hand panels of Figure 1, but with the different dimensions of the three conditions (pre-/post-2006, before/after age 65, treatment/control group) substituted so that the left-hand panel shows the full-time work rates for the treatment and control groups before 2006, while the right-hand panel shows them after 2006. This perspective allows verification of the parallel trends not just within the treatment and control groups before and after 2006, but also between the two groups.

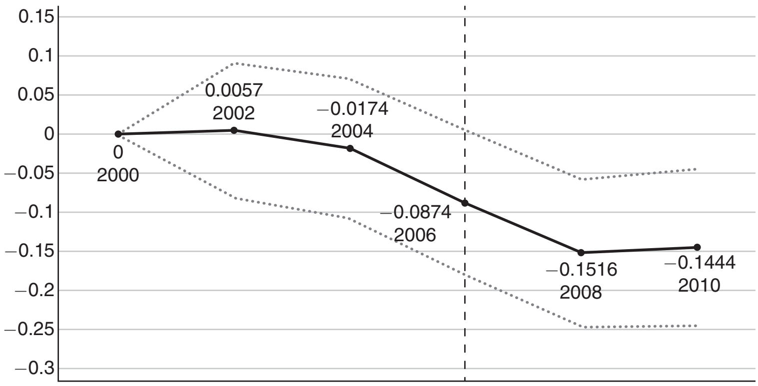

Figure 2. Dynamic Triple-Difference Estimates of Full-Time Work

Notes: This figure shows the dynamic triple-difference estimate (with no controls, only indicators for treatment group, being over age 65, year, and the interactions of all these indicators), allowing for a separate treatment effect in every year relative to 2000 (with 2000 normalized to 0). Point estimates for each year are displayed. Ninety-five percent confidence intervals on these estimates are given by the dashed lines.

increases further in 2008 and 2010, at 15 and 14.4 percentage points, respectively (both significant at the 1 percent level). This magnified effect in the later years relative to 2006 is consistent with some labor market frictions in the year of the reform. The figure also provides evidence that the decline in full-time work for the treatment group began in 2006, before the beginning of the Great Recession.

B. *Full-Time Work Estimates*

I now turn to estimating regressions. Results of the triple-differences estimation can be seen in columns 1 and 2 of Table 1. Column 1 shows the raw results without individual fixed effects or demographic and health controls, and column 2 shows the baseline specification of equation (7). The estimate of the effect on full-time work is quite robust, and in the baseline specification indicates a reduction of 8.36 percentage points in the rate of full-time work for the treated group. This reduction is large relative to the baseline rate of full-time work, 0.349 (the rate of full-time work for individuals aged 65–68 in the years 2006–2010, net of the estimated effect of Part D); it corresponds to a drop of 24 percent in treated individuals working full time.[24] “Intermediate” specifications including controls but no fixed effects yield very similar results. Results are also robust to a wide range of alternative specifications: a difference-in-difference design with the sample restricted to the treatment

[24] This reduction is also very large relative to the effect of wealth in the regression. Mean nonhousing household wealth in the sample is about $380,000. At this mean, and using the fifth-order polynomial of wealth controlled for in the regression, an increase of $10,000 of wealth is predicted to reduce the rate of full-time work by 0.09 percentage points, two orders of magnitude smaller than the effect of Part D. The effect of wealth estimated here is likely biased due to measurement error, reverse causality, and omitted variables. For a more careful comparison of the effect of Part D to Social Security wealth, see Section V.

Table 1—Triple-Differences Estimates of Part D Eligibility's Effect on Labor

Dependent variable	Full-time work		Part-time work		Any work	
Specification	No controls (1)	Baseline (2)	No controls (3)	Baseline (4)	No controls (5)	Baseline (6)
Post65 × Post2006 × RHI only up to age 65	−0.107	−0.0836	0.0381	0.0589	−0.0692	−0.0247
	(0.0287)	(0.0313)	(0.0266)	(0.0308)	(0.0329)	(0.0338)
Post65 × Post2006	−0.0384	0.0199	0.0175	0.00157	−0.0209	0.0215
	(0.0196)	(0.0217)	(0.0191)	(0.0218)	(0.0236)	(0.0234)
Age and year indicators × RHI only up to age 65	Yes	Yes	Yes	Yes	Yes	Yes
Demographics, health, and individual fixed effects	No	Yes	No	Yes	No	Yes
Observations	15,828	15,382	15,828	15,382	15,828	15,382
Number of clusters	6,819	6,516	6,819	6,516	6,819	6,516

Notes: This table presents triple-differences estimates of the effect of Part D eligibility on full-time work (columns 1 and 2), part-time work (columns 3 and 4), and any work (columns 5 and 6). The controls in columns 1, 3, and 5 are age, time, and having retiree health insurance (RHI) only up to age 65 fixed effects, and age and time fixed effects interacted with treatment group. Columns 2, 4, and 6 also include an indicator for being single, a set of indicators for each of the census divisions, a fifth-order polynomial in nonhousing household wealth, a set of indicators for self-reported health on a scale of 1–5, body mass index, and a set of indicators for having any of the following physician diagnosed conditions: cancer, lung disease, heart disease, stroke, arthritis, or psychiatric conditions. The first row provides the triple-differences estimates of Part D eligibility on the dependent variable for individuals with employer sponsored RHI only until age 65. The third row provides the estimates of the effect of Part D eligibility on the dependent variable for the control group of individuals with RHI unlimited by age. Robust standard errors clustered at the level of the individual are in parentheses.

group and no control group; a difference-in-difference design with both treatment and control groups only over age 65; using an alternative control group of individuals with no ESI; restricting the sample to ages 62–68; excluding individuals ages 63–64; excluding individuals with Medicaid or VA coverage; estimating a hazard model of leaving full-time work assuming it is an absorbing state; taking the first-difference of full-time work as the dependent variable; and including age and year indicators interacted with the demographic controls to allow for demographic-group specific age and time trends. For details of these and other results, see online Appendix B3.

Reassuringly, the effect of eligibility for Part D on the control group is generally not statistically significant.[25] For example, there is an insignificant point estimate of a 2 percentage point *increase* in full-time work for the control group in the baseline specification. This formalizes the visual impression from Figure 1 that Part D eligibility has no effect on labor outcomes for individuals who were not retirement locked to begin with. The fact that no significant effect is seen for the control group helps allay concerns that the results in the treatment group are influenced by other unobserved changes (e.g., the change in the Social Security FRA) rather than the relaxation of retirement lock due to Part D.

[25] This is true across the numerous other specifications estimated. One exception is the specification with no controls in column 1; however, this effect disappears when controls and/or fixed effects are added.

C. *Part-Time Work Estimates*

Having established the effect on full-time work, I now turn to consider what alternatives individuals are replacing their full-time work with. Individuals may wish to slowly phase from full-time work to complete retirement (Ruhm 1990, Rust 1990, Peracchi and Welch 1994). However, just as the prospect of losing ESI may prevent individuals from completely retiring, it may also prevent them from reducing their labor supply gradually, as the vast majority of employers do not offer health insurance to part-time workers.[26] It is therefore of interest to explore how much of the reduction in full-time work estimated above is due to individuals shifting to part-time work, and how much of it is due to individuals shifting into complete retirement.

Columns 3 and 4 of Table 1 show the results of estimating equation (7) with part-time work as the dependent variable, without individual fixed effects or controls (in column 3) and with the baseline specification (in column 4). They show an increase in part-time work among the treated, of 5.9 percentage points in the baseline specification. Over a base rate of part-time work of 16.2 percentage points, this represents an increase of 36 percent. As with full-time work, the control group shows no significant or systematic change in part-time work.

Columns 5 and 6 of Table 1 show the effect of Part D eligibility on any work; this is the residual of the effect on full-time work after accounting for the increase in part-time work. In the baseline specification, participation declined by 2.5 percentage points with Part D. According to these estimates 70 percent of those leaving full-time work do so to go into part-time work. Only 30 percent of people leaving full-time work as a result of the relaxation of retirement lock do so in order to fully retire.[27]

D. *Job Lock and Transition from Full- to Part-Time Work*

There are two ways to go from full-time to part-time work: reducing hours in essentially the same job; and switching jobs to one with fewer hours. Previous literature has found this latter to be common (e.g., Ruhm 1990). Table 2 shows to what extent these two mechanisms operate in reaction to Part D.

Columns 1 and 2 show the increase in job switching for the treated upon Part D eligibility, with no controls and with the standard controls from equation (7), respectively. This estimates job lock in the sense of job mobility: eschewing movement between jobs due to concerns about ESI coverage, as defined, for example, in Gruber and Madrian (2004). The estimate with controls

[26] In 2017, only 24 percent of employers who provided health insurance to some workers extended that offer to part-time workers (Kaiser Family Foundation 2017). In my sample, I regressed ESI on full-time work using the baseline specification, with the sample restricted to those not eligible for Part D (because they were below age 65 or before 2006), and found that coverage declined by 12 percentage points among those leaving full-time work.

[27] Qualitatively similar results on full-time work and any work are also found in a difference-in-difference specification based only on eligibility for Part D (over age 65 after 2006) with no control group, estimated on the American Community Survey. For details, see online Appendix B4. Furthermore, as a falsification test, the baseline regressions are estimated with placebo Part D eligibility assigned at ages 60 and 62. These regressions do not yield any significant treatment effects; the results are in online Appendix B5.

Table 2—Job Switches

Dependent variable	Job switching		Part-time work × job switching	
Specification	No controls (1)	Baseline (2)	No controls (3)	Baseline (4)
Post65 × Post2006 × RHI only up to age 65	0.0448	0.0439	0.0414	0.0408
	(0.0207)	(0.0209)	(0.016)	(0.0162)
Post65 × Post2006	−0.0038	−0.00126	−0.0074	−0.00481
	(0.0143)	(0.0144)	(0.0111)	(0.0112)
Age and year indicators × RHI only up to age 65	Yes	Yes	Yes	Yes
Demographics, health, and individual fixed effects	No	Yes	No	Yes
Observations	15,828	15,382	15,828	15,382
Number of clusters	6,819	6,516	6,819	6,516

Notes: This table presents triple-differences estimates of the effect of Part D eligibility on job switching (in columns 1 and 2), and on job switches involving movement into part-time work (in columns 3 and 4). The controls in columns 1 and 3 are age, time, and having retiree health insurance (RHI) only up to age 65 fixed effects, and age and time fixed effects interacted with treatment group. Columns 2 and 4 also include an indicator for being single, a set of indicators for each of the census divisions, a fifth-order polynomial in nonhousing household wealth, a set of indicators for self-reported health on a scale of 1–5, body mass index, and a set of indicators for having any of the following physician diagnosed conditions: cancer, lung disease, heart disease, stroke, arthritis, or psychiatric conditions. The first row provides the triple-differences estimates of Part D eligibility on the dependent variable for individuals with employer sponsored RHI only until age 65. The third row provides the estimates of the effect of Part D eligibility on the dependent variable for the control group of individuals with RHI unlimited by age. Robust standard errors clustered at the level of the individual are in parentheses.

indicates that individuals increase the rate at which they move between jobs by 4.4 percentage points when no longer faced with prescription drug-induced job lock. The baseline rate of job switching in any two-year wave of the sample is 4.5 percentage points; thus, this estimate represents a large semi-elasticity of job switching with respect to Part D eligibility of 0.98.

This job lock estimate includes job switches between two full-time jobs and between two part-time jobs, as well as those between full- and part-time jobs. To decompose the full- to part-time movements into those entailing job switches and those only involving a reduction of hours, columns 3 and 4 of Table 2 have the interaction of part-time work and job switching as their outcome, with no controls and with the standard controls, respectively. The resulting estimate with controls shows that Part D eligibility increases part-time work associated with job switching among the treated by 4.1 percentage points. Thus, about 69 percent of the increase in part-time work among the treated is due to a change in jobs, while only 31 percent is due to a reduction of hours on the same job.[28]

[28] My elasticity of job-switching estimate of 0.98 is large relative to previous literature. For example, Madrian, Burtless, and Gruber (1994) finds an increase of 25 percent in job turnover due to introduction of COBRA. This divergence is due to two main differences between the current setting and previous work. First, the nature and scale of the policy reform are substantially different. Part D provides drug insurance in perpetuity, whereas COBRA provides health insurance only for up to 18 months. Second, a large bulk of the changes in jobs here is accounted for by a reduction in work intensity, moving from full-time to part-time work. For my treated group of over 65-year-olds this is evidently an attractive option, but it may be less so for the prime working-age males, which have been the focus of most previous work.

Table 3—Effect on Annual Labor Earnings

Dependent variable	Annual labor earnings		Wages	
Specification	No controls (1)	Baseline (2)	No controls (3)	Baseline (4)
Post65 × Post2006 × RHI only up to age 65	−6,327	−1,477	−1.0453	−0.00949
	(1,933)	(1,915)	(4.3177)	(7)
Post65 × Post2006	−1,582	1,557	2.33	4.834
	(1,388)	(1,441)	(3.7623)	(7.137)
Age and year indicators × RHI only up to age 65	Yes	Yes	Yes	Yes
Demographics, health, and individual fixed effects	No	Yes	No	Yes
Observations	15,515	15,076	6,959	6,694
Number of clusters	6,764	6,465	3,899	3,688

Notes: This table presents estimates of the effect of Part D eligibility on annual labor earnings and wages. Dollars are inflated to 2010 prices by the consumer price index, and top coded at $100,000, the ninety-fifth percentile of earnings for full-time workers. Observations with over 70 hours of work reported in a usual week are excluded. The dependent variable of columns 1 and 2 is annual earnings. The dependent variable of columns 3 and 4 is wages, defined as: $w_{i,t} = AnnualLaborEarnings_{i,t}/\left(UsualWeeklyHours_{i,t} \times 52\right)$. The controls in columns 1 and 3 are age, time, and having retiree health insurance (RHI) only up to age 65 fixed effects, and age and time fixed effects interacted with having RHI only up to age 65. Columns 2 and 4 also include an indicator for being single, a set of indicators for each of the census divisions, a fifth-order polynomial in nonhousing household wealth, a set of indicators for self-reported health on a scale of 1–5, body mass index, and a set of indicators for having any of the following physician diagnosed conditions: cancer, lung disease, heart disease, stroke, arthritis, or psychiatric conditions. The first row provides the triple-differences estimates of Part D eligibility for individuals with employer sponsored RHI only until age 65. The third row provides the estimates of the effect of Part D eligibility for the control group of individuals with RHI unlimited by age. Robust standard errors clustered at the level of the individual are in parentheses.

E. *Earnings and Wages Estimates*

Earnings.—A statistic that captures both the decline in full-time work and the increase in part-time work is annual labor earnings. The advantages that labor earnings has as a summary statistic of the two main and partially offsetting effects of Part D on labor are paired with two problems of self-reported earnings: they are often inaccurately reported, and they are very right-skewed. To ameliorate the latter, I top code earnings at the ninety-fifth percentile among full-time workers ($100,000). Furthermore, I exclude observations with over 70 usual hours of work per week as likely misreported.

Results are reported in Table 3. Columns 1 and 2 provide estimates with no controls and with the standard controls, respectively. Both indicate substantial declines in annual labor earnings, although the estimates with controls are not statistically significant. That specification indicates a reduction of $1,477, albeit with a large standard error of about 1,900.

Wages.—In equilibrium, labor outcomes are determined not only by labor supply but also by labor demand. It would be helpful to rule out that the decline in full-time work is driven by a negative labor demand shock, rather than a change in labor supply. The primary evidence on this point comes from the control groups: a general shock to labor demand would impact the labor outcomes of individuals both above and below the age 65 cutoff for Medicare eligibility; and the control group of individuals with RHI for life allows me to test whether any age-specific shock to over 65-year-olds after 2006 remains. Thus, the triple-differences estimator should absorb such demand shocks.

Nevertheless, there may be a demand shock after 2006 for the particular kinds of workers who are over age 65 *and* have RHI limited to pre-age 65. One way to allay this concern is by verifying that there was no decline in wages, as in Garthwaite, Gross, and Notowidigdo (2014): such a decline in wages might indicate a negative demand shock, rather than a negative supply shock. Columns 3 and 4 of Table 3 show the effect of Part D eligibility on wages, without and with controls, respectively. Conditional on positive wages, there is no significant effect on the wages of the treated group (or of the control group), with a point estimate of a reduction of less than $0.01 per hour for the treatment group in the baseline specification. Large standard errors preclude conclusively saying that there was no change in wages. However, the small point estimates do not suggest that the fall in full-time work for the treated group at age 65 in 2006 is driven by a fall in demand for their labor.

F. *Accounting for the Great Recession*

One particularly large negative labor demand shock in the period after 2006 was the Great Recession, which began in 2007 and ended in 2009. This section tests robustness of the results to addressing this potential shock in various ways. The stability of the results to these perturbations both argues against the Great Recession driving the main estimate, and demonstrates robustness with respect to more general concerns, as well.

The triple-differences design is intended to absorb unobservable shocks correlated with Part D eligibility, including labor demand shocks. Presumably, 55–64-year-olds are close substitutes for 65–68-year-olds; and to the extent that having RHI might have mediated such shocks or been correlated with relevant worker characteristics, use of the control group of those with RHI to any age should have simultaneously absorbed such an idiosyncratic shock, as well as tested for its existence (insofar as having RHI until any age is similar to having RHI only until age 65). Since there are generally no significant effects for the control group, there is no substantial evidence of such residual shocks.

Another way to assess whether the Great Recession is biasing the estimates of the Part D effect is based on the observation that the Recession was not equally severe in all parts of the country, or for all demographic groups (Elsby et al. 2010). Allowing for heavily hit regions to have separate time trends from areas that were less affected by the Great Recession helps to distinguish the treatment effect of Part D on labor supply from the differential Recession-related trends in labor demand. Columns 1 and 2 of online Appendix Table 18 in online Appendix B.6, for full- and part-time work respectively, allow for separate time and age trends for each census division, as well as a census division-specific effect of being in the treatment group.[29] The resulting estimates

[29] The nine census divisions are the finest geographic information available in the public-use HRS data.

are very close to the baseline specification, with a 7.25 percentage point decline in full-time work.[30]

A similar approach is taken with respect to observable demographics (although differences between the treatment and control groups on demographics are generally insignificant, see online Appendix A). Detailed results are in online Appendix B3 with the estimate of the Part D treatment effect remaining very similar to the baseline estimate.[31] Thus, it does not appear to be the case that demographic differences between the treatment and control group are interacting with differential impacts of the Great Recession (or any other time or age-varying shocks) across demographic groups to bias the estimate of the treatment effect.

Nevertheless, if the control group is different on *unobservables* from the treatment group, there might be specific impacts of the Great Recession on the treatment group that remain unaccounted for, and which are not picked up by allowing for differential geographic and demographic trends. To check whether the Great Recession is confounding the results in this way, I utilize the fact that the treatment period includes observations from before and after the recession. Online Appendix Table 18 in online Appendix B.3 also shows results excluding some of the later sample years entirely. Columns 3 and 4 show results for full-time and part-time work, respectively, when the only treatment period is 2006 (before the recession). While the standard errors are large due to the small sample size, leading to statistical insignificance, the effects are still economically large. In particular, they indicate a 5.6 percentage point reduction in full-time work for the treated. This parallels the estimates presented in Figure 2 showing that the decline in full-time work for the treatment group began in 2006.

That the magnitude of the effects in 2006 is smaller than for the entire post-2006 period (albeit by less than one standard deviation) is consistent with a certain amount of labor market friction. Part D went into effect at the beginning of 2006, but it may have taken time for individuals to change their labor supply. In particular, as the HRS is a survey, individuals surveyed during 2006 may have been contacted before they had time to adjust their working arrangements in response to the reduced retirement lock. This somewhat delayed response is also graphically apparent in Figure 2.

Columns 5 and 6 of online Appendix Table 18 show results only excluding observations surveyed during the Great Recession (i.e., observations from 2008). Thus, the pre-Part D period consists of years 2000–2004; and the post-Part D period here consists of observations from before the recession, in 2006, and from after the recession ended in 2010. Here, once again, there is a large and

[30] I also estimated a regression allowing the treatment effect itself to vary by census division. While none of the estimates are significant, that is due primarily to large standard errors in the resulting small census division-specific samples. The point estimates of the treatment effect remain large in magnitude, and range from 1.9 percentage points (in the West North Central division) to 28 percentage points (in the Mountain division) decline in full-time work due to Part D. Overall, there was little relation between the magnitude of the treatment effect and the severity of the Great Recession at the census-division level. As a measure of the severity of the recession, I used the increase in the unemployment rate within the division between 2008 and 2009. The correlation between this measure and the estimated treatment effect at the census-division level was only -0.077.

[31] Similarly, including the interaction of wealth quartile and year fixed effects to control for potentially differential time trends across households of different wealth yielded almost identical results as the baseline specification.

statistically significant drop in full-time work for the treated of 9.3 percentage points, and a (statistically insignificant) concurrent rise in part-time work rate of 4.3 percentage points.

G. *Heterogeneity in the Treatment Effect*

In this section, I examine whether there is more retirement lock for workers who need more prescription drugs.[32] For individuals who have experienced negative health shocks, drug insurance is more valuable, both because they are more likely to use this insurance (Pauly and Zeng 2004) and because they would have found it more expensive to purchase insurance on the private market (if any insurer were willing to cover them). Their demand for insurance is therefore higher and the supply of such insurance on the individual market is slimmer, raising the relative value of ESI.

I first define two groups based on plausibly exogenous, physician-diagnosed health conditions.[33] The first group is the "sick" group, comprised of individuals who had at least one of the following conditions: cancer, heart disease, lung disease, stroke, arthritis, or psychiatric conditions. Roughly two-thirds of the sample fall in this group. The second group is the "healthy" group, comprised of individuals who do not have any of those conditions. The first group is more likely than the latter to require a greater quantity of expensive prescription drugs, and to face a larger risk of drug expenses: mean monthly out-of-pocket spending on drugs in the sick group is \$80 with a standard deviation of 466, while for the healthy, it is \$34 with a standard deviation of 125.

Equation (7) can be estimated for each of these groups separately (excluding health status controls). Figure 3 shows full-time work for the treatment group, by health status. While there is no substantial difference in the evolution of full-time work over age before and after Part D for the healthy, the decline in full-time work at age 65 for the sick is much more pronounced after 2006.

Table 4 shows the corresponding regression results. The first two columns give the estimates on full-time work for the sick and healthy groups, respectively. Columns 3 and 4 do the same for part-time work, and columns 5–6 for any work. Reflecting the impression from Figure 3, the entire effect is concentrated in the sick group. This group experiences a 12.3 percentage point drop in full-time work, and a 9.5 percentage point increase in part-time work (the sum of the two is the effect on any work, a decline of 2.8 percentage points). For the healthy group, there are no significant changes in any direction (likewise for the control group in all

[32] Kapur (1998); Bradley et al. (2007); and Bradley, Neumark, and Barkowski (2013) use similar heterogeneity by health status to identify job lock due to health insurance.

[33] The HRS contains data on whether individuals use prescription drugs, however this is endogenous to insurance coverage. Indeed, previous work has found that Part D eligibility increased prescription drug utilization (Lichtenberg and Sun 2007, Engelhardt and Gruber 2011, Ayyagari and Shane 2015, Wettstein 2018). Nevertheless, this measure has been used in other contexts (e.g., in the discussion of Krueger, Katz, and Notowidigdo 2017); for completeness, the following exercise is also done with self-reported regular use of prescription drugs as the dimension along which the sample is split. Results are qualitatively similar, and reported in online Appendix B.3.

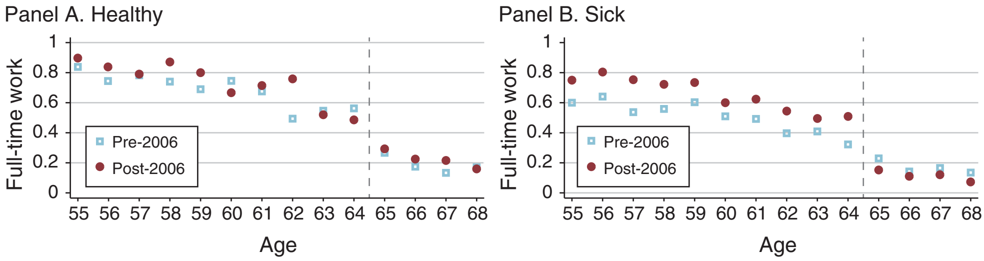

Figure 3. Full-Time Work Rates for the Treatment Group, by Health Status

Notes: These figures show the difference-in-differences of full-time work for the treatment group, broken down by individual health status. The sample is individuals aged 55–68, in the years 2000 until 2010, who have RHI through their employer only until age 65. The squares indicate rates of full-time work by age in the years 2000–2004, while the circles indicate full-time work rates by age for years 2006–2010. The dashed gray line differentiates between ages eligible for Part D, on the right, and those ineligible, on the left (in the post-2006 period). The sample is divided into "sick" and "healthy" groups, with the sick group including any individual who, at the time of the survey, had at least one of the following physician diagnosed conditions: cancer, lung disease, heart disease, stroke, arthritis, or psychiatric conditions. Individuals were classified as healthy otherwise. Panel A includes only healthy individuals, while panel B includes those who were sick.

Table 4—Heterogeneity by Health Status

Dependent variable	Full-time work		Part-time work		Any work	
Subsample	Sick (1)	Healthy (2)	Sick (3)	Healthy (4)	Sick (5)	Healthy (6)
Post65 × Post2006 × RHI only up to age 65	−0.1227 (0.037)	0.0063 (0.0654)	0.0946 (0.0376)	−0.0171 (0.0584)	−0.0281 (0.0406)	−0.0109 (0.068)
Post65 × Post2006	0.0467 (0.0252)	−0.0242 (0.0465)	−0.0221 (0.0266)	0.0293 (0.0423)	0.0218 (0.0278)	0.0051 (0.0476)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	10,889	4,684	10,889	4,684	10,889	4,684
Number of clusters	4,911	2,336	4,911	2,336	4,911	2,336

Notes: This table presents heterogeneity of the effect of Part D eligibility on full-time and part-time work by health status. The dependent variable of the first two columns is full-time work; for columns 3–4, it's part-time work; and for columns 5–6, it's any work. The subsample of each column is detailed in the column's heading, where "sick" and "healthy" groups are defined in the text. All columns control for age and time fixed effects, age and time fixed effects interacted with having retiree health insurance (RHI) only up to age 65, an indicator for being single, a set of indicators for each of the census divisions, and a fifth-order polynomial in nonhousing household wealth. The first row provides the triple-differences estimates of Part D eligibility for individuals with employer sponsored RHI only until age 65. The third row provides the estimates of the effect of Part D eligibility for the control group of individuals with RHI unlimited by age.

these regressions). This pattern is consistent with Part D being the driving force behind the observed effects through its relaxation of retirement lock.[34]

[34] Another dimension of heterogeneity that has been used to estimate job lock is availability of spousal health insurance (Madrian and Beaulieu 1998). Almost all employer plans cover spouses (Kaiser Family Foundation 2017) but Part D does not. Thus, while Part D relaxed the retirement lock of unmarried individuals, or those whose spouses were unlikely to need expensive drugs, those who worked to keep their spouses covered remained locked. Consistent with this, I find larger responses among individuals with no sick spouse versus those with a sick spouse.

V. A Test of Distortion Due to Retirement Lock

In this section, I give context for the empirical findings above, comparing the effect of Part D on the labor supply of the elderly to the effect of Social Security wealth. This tests for labor market distortion due to insurance market failure and provides an estimate of the willingness to pay for Part D on the part of retirees.

The estimates in Section IV show that Part D had a large effect on the full-time work of individuals without RHI after age 65. However, as shown in Section I, a reduction in labor in response to the subsidy is insufficient for concluding that any *distortion* existed before the policy change. Rather, a large effect of the subsidy to insurance relative to retirement income can provide such evidence. In terms of the framework in Section I, a constraint on individual insurance optimization can be inferred from $R - 1 > 0$, which also implies a willingness to pay more for the Part D subsidy than for a simple transfer of income.

To measure the effect of a dollar of Part D subsidy on labor supply, I first establish how many dollars of subsidy are actually distributed. I calculate that the present value of the subsidy to a 65-year-old in 2006 was about \$25,000.[35] Section IV estimates that $s(dG/ds) = 0.0836$; together these give the numerator of R.

For the effect of Social Security wealth on labor supply, I turn to the literature on Social Security.[36] Much of that literature finds small or statistically insignificant effects of Social Security benefits on labor supply (e.g., Burtless 1986, Krueger and Pischke 1992, Costa 1998). Recent analysis of exogenous changes in Social Security due to changes in the calculation of benefits using administrative data provides the most precise estimate available, to my knowledge (Gelber, Isen, and Song 2016). These authors estimate that a \$6,126 increase in lifetime Social Security wealth (discounted at 3 percent annually) led to a decline of labor participation of 0.4 percentage points; i.e., $b(dG/db) = 0.004$.

These numbers yield $R = (8.36/25{,}000)/(0.4/6{,}126) = 5.12$, substantially larger than 1.[37] In other words, the effect of a dollar of drug insurance subsidy on labor supply is about 5 times as large as the effect of a dollar of Social Security.[38]

[35] The life expectancy of a 65-year-old in 2006 was 17 years. In 2006, the benefits per capita from Part D were \$1,708, and these are projected to increase to \$3,188 a year by 2023, 17 years later (Medicare Board of Trustees 2014). These benefits include the premiums enrollees pay themselves. Subtracting the average premium from these benefits and summing the resulting *net* benefits from 2006 to 2023, discounted at a rate of 3 percent annually, gives \$25,000 in 2006 present value. Of course, the true expected discounted sum of Part D subsidies will vary by individual; this calculation is an approximation.

[36] The relation of Social Security to retirement has been extensively studied. For overviews, see Krueger and Meyer (2002); Feldstein and Liebman (2002); and Blundell, French, and Tetlow (2016).

[37] Assuming there is only sampling error in my own estimate of $s(dG/ds)$, R is significantly larger than 1 at a 95 percent level of significance (the confidence interval is $[1.36, 8.88]$).

[38] Calibrations of R by other estimates of Social Security's effects are similar. van der Klaauw and Wolpin (2008) considers a 25 percent reduction of benefits, averaging a reduction of annual benefits by \$2,667 in 2010 dollars. This is estimated to increase full-time work by 4.6 percentage points (averaged over men and women ages 62–69). Compared to the annual net subsidy of Part D, \$1,588 in 2010 (Medicare Board of Trustees 2014), R is $(8.4/1{,}588)/(4.6/2{,}667) = 3.07$. Alternatively, Hurd and Boskin (1984) finds that increasing benefits by \$10,000 in 1969 would lead to 7.8 percentage points decrease in participation, using a 6 percent discount rate. When Part D benefits are discounted at this rate and the 1969 dollars are inflated to 2010 dollars, $R = 3.25$. Finally, Samwick (1998) estimates that a 20 percent reduction in Social Security PIA, (i.e., decreased Social Security wealth of \$20,000 in 2010 dollars) would decrease retirement by 1 percentage point, resulting in $R = 6.7$.

This large *R* is evidence of a lack of an efficient individual drug insurance market: if it were possible to buy a dollar's worth of insurance in exchange for a dollar, providing a dollar of insurance should have precisely the same effect as providing a dollar of income.

As shown in Section I, *R* is essentially the willingness to pay for drug insurance subsidies out of Social Security income, as measured by the response of full-time work.[39] This estimate seems large relative to the small average welfare gains from Part D estimated by Engelhardt and Gruber (2011). These authors examine the distribution of out-of-pocket spending on prescription drugs with and without Part D coverage and calculate utility gains from reduction of risk. That approach does not account for gains among individuals who were similarly insured both before and after Part D. Such individuals may replace private insurance with public insurance, but there is no change in their spending distribution. However, the results in Section IV suggest that there may be welfare gains even when public insurance crowds out ESI. These gains do not come from a more favorable distribution of out-of-pocket spending, but rather from the flexibility of labor supply afforded by the public alternative. Such gains are neglected by an analysis focusing on out-of-pocket spending.[40]

Online Appendix C shows how this estimate of *R* can be used to inform estimates of the social welfare costs and benefits of Part D, under some assumptions: that Part D has no welfare effects on those who do not currently benefit from it (e.g., those who work full-time with an employer providing ESI); that there are no program interactions of Part D impacting the government budget (e.g., that the effect on retirement does not affect Social Security's costs, either because claiming ages are unchanged or because the actuarial adjustment of benefits is fair); and the external validity of the estimated *R* (e.g., to those who have no ESI, or who are older than the 65–68-year-olds in the estimation sample). Under these strong assumptions, I estimate the marginal value of public funds in Part D to be about \$2 for every \$1 of Part D subsidies.

VI. Conclusions

Part D was the largest expansion of public health insurance in forty years at the time of its implementation. While primarily considered a safety net for uninsured elderly faced with high prescription drug costs, it also had the effect of aiding individuals who were already insured through their employers who would have liked to retire but for the loss of their coverage.

This paper provides evidence of retirement lock stemming from employer sponsored prescription drug insurance. It does so by focusing on individuals who had employer sponsored RHI only until Medicare eligibility at age 65. At that age

[39] This estimate reflects the willingness to pay of individuals ages 65–68. Assessment of welfare for individuals older than that based on the current setting is an out-of-sample extrapolation. Nevertheless, if individuals are forward looking, the willingness to pay calculated here incorporates the future costs and benefits these individuals expect at older ages.

[40] Similar observations have been noted in Gruber (1996), Greenberg (1997), Chetty and Looney (2006), Greenberg and Robins (2008), and Fadlon and Nielsen (2019).

before 2006, such individuals would have had to remain in (typically full-time) work to maintain their drug coverage. After 2006, drug coverage was no longer contingent upon work.

Estimates based on this change in 2006 at age 65 show that individuals indeed reduced their labor supply substantially, decreasing their full-time work rate by about 8.4 percentage points, with no significant effect for a control group of individuals with RHI to any age. Seventy percent of this reduction occurs on the intensive margin, moving from full-time to part-time work. The remaining 30 percent consist of individuals moving from full-time work directly into full retirement. The entire effect is concentrated among individuals with chronic health conditions, consistent with their elevated a priori likelihood of being retirement locked.

To test for distortion from retirement lock, I compare the effect of the drug insurance subsidy to the effect of Social Security benefits. The magnitude of labor decline in response to a dollar of subsidy is equivalent to the decline that would be expected from $5 of additional Social Security benefits, demonstrating that individuals work for ESI above and beyond what they are willing to work for income. This suggests that Part D may have benefited older workers through relaxation of retirement lock.

The high willingness to pay for prescription drug insurance implied by my estimates is in stark contrast to low estimated willingness to pay for health insurance found in recent work by Finkelstein, Hendren, and Luttmer (forthcoming) and Finkelstein, Hendren, and Shepard (2019). These differences may be driven by the fact that the population gaining drug coverage through Part D is both older and richer than the low-income prime-age population studied in those contexts; and because those studies find significant crowd-out of uncompensated care due to health insurance expansions (e.g., in emergency rooms, as shown in Garthwaite, Gross, and Notowidigdo 2018), which is less likely in the prescription drug context given the limited availability of such care in emergency room settings and through charity. The substantial estimated value of insurance benefits that are independent of employment should be taken into account when assessing other public programs that increase flexibility in labor supply.

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