Is Retirement Good for Men's Health?
Evidence Using a Change in the Retirement Age in Israel

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Abstract

This study examines the effect of employment on elderly men's health. A typical OLS analysis yields a positive relationship between employment and health for individuals in their sixties. Causality, however, is difficult to infer because healthier individuals are more capable of working than others. To overcome this endogeneity problem, this paper exploits the increase in the mandatory retirement age for men in Israel from sixty-five to sixty-seven in 2004. After this change, the employment rate of men in this age bracket jumped significantly compared to the last cohort that was able to retire at sixty-five. Using the new retirement law as an exogenous source of variation in the employment status of elderly men, a significant causal relationship in the opposite direction of the correlation is found: compulsory employment at older ages impairs health. These findings are found across a broad array of datasets and health outcomes. The results are significantly stronger among less-educated workers, suggesting that employment in physically demanding occupations is more detrimental to health. Placebo analyses using the years preceding the new retirement regime and other health measures unrelated to employment (e.g., dentist visits) reinforce a causal interpretation of our main findings.

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Introduction

Many countries are debating whether to raise the retirement age in response to aging populations that are straining the resources of national pension programs. The UK, for example, is considering an increase in the state pension age from sixty-five to sixty-six for both men and women and Germany plans to set its pension age at sixty-seven.[1] These measures may indeed prevent the collapse of a country's pension fund and mitigate the dependency of the retired population on those currently employed.

The postponement of retirement, however, may have unintended adverse consequences. This paper investigates whether delaying retirement affects the health of individuals who would be forced or encouraged to work later in life. Establishing a causal relationship between working and health is complicated by the endogeneity of one's decision to work. The health of people in their sixties is strongly and positively correlated with working. This correlation, however, reveals little about the causal relationship, since healthier individuals are more likely to continue working.

To overcome this simultaneity problem, this paper exploits a unique policy reform in 2004, whereby Israel raised its mandatory retirement age for workers in all sectors from sixty-five to sixty-seven. The reform triggered a dramatic increase in the labor-force participation of men in the relevant age bracket. Employment rates rose from 37 percent in the pre-reform years to 47 percent afterwards.[2] This policy-driven change in the labor-force status of men created exogenous variation in a person's working/retirement status. In particular, the analysis exploits the variation between the last cohorts that retired at age sixty-five and the first cohorts that had to wait until sixty-seven. The results show that the

[1] http://en.wikipedia.org/wiki/UK_State_Pension and http://en.wikipedia.org/wiki/Pensions_in_Germany.
[2] This analysis was based on the Labor Force Surveys of the Israel Central Bureau of Statistics (hereinafter: ICBS) for the years 2000–2011.

health of older men deteriorates when a legislative change forces them to work further into their 60's. Working at older ages is found to increase an index for poor health and the number of physician visits by one standard deviation, while overall healthcare expenditures increase by half of a standard deviation.

These findings are more pronounced for less-educated men relative to highly educated men, suggesting that working in physical demanding jobs is more detrimental to health. Importantly, the unaffected cohorts just before the change showed no deterioration in health. This placebo analysis reinforces the causal interpretation of my findings by demonstrating that the deterioration in health found among older men was not part of a pre-existing trend.

The existing empirical literature on the effect of retirement on health yields conflicting results. Dave et al. (2008), using panel data from the U.S. Health and Retirement Study (HRS), conclude that retirement leads to an increase in difficulties in mobility, daily activity, morbidity, and mental illness even among individuals who had been healthy before their retirement. Rohwedder and Willis (2010), exploiting variation in retirement policies across Europe and the U.S., find that early retirement is associated with a deterioration in cognitive ability among people in their early sixties. Kuhn et al. (2010) analyze a reform implemented after a steel crisis in the 1980s that affected certain regions in Austria. The reform allowed workers aged fifty and older who had a certain work history and resided in eligible regions to retire up to 3.5 years earlier than non-eligible workers. The authors find that early retirement increases the chances of premature death. Finally, Behncke (2012), using the rich English Longitudinal Study of

Ageing (ELSA) dataset and performing matching and instrumental variable methods, finds that retirement exacerbates the risk of incurring chronic health conditions.

Conversely, Ekerdt et al. (1983), comparing pre- to post-retirement changes in the health status of male retirees and males in the same age cohort who continue working, find that retirement does not impair health. Mein et al. (2003), comparing civil servants who retired at the mandatory age of sixty with peers who continued working, find that retirement improves mental health and does not affect physical functioning. Coe and Lindeboom (2008) use the Health and Retirement Study, and create an instrument for retirement based on employer incentives for workers to accept early retirement. They conclude that retirement is not harmful to health. Coe and Zamarro (2011), using the first wave of the SHARE data to exploit variation in the retirement age across several European countries, find that retirement lowers the probability of reporting a deterioration in health. Blake and Garrouste (2012), examining a pension reform in France that targeted private-sector employees, find that retirement improves health. Finally, Bloemen, Hochguertel, and Zweerink (2013), using an early retirement opportunity offered by central government employers in the Netherlands to encourage civil servants aged 55+ to retire, conclude that early retirement reduces the probability of death.

The aforementioned studies typically fall into one of several categories. The first strategy is to compare the health outcomes of retirees with those of non-retirees. The possible drawback of such an approach is that healthier individuals are more likely than others to choose to continue working. The second strategy exploits variation across countries in retirement ages. However, differences across countries in the retirement age

may be correlated with differences in the health of individuals across countries. The third strategy exploits reforms that allow certain employees or workers in specific sectors to retire earlier than other workers. This type of strategy could be problematic if the employees or workers in the selected sectors are different from the general population in terms of their health and work behaviors.

Therefore, the main contribution of this paper is the use of a plausibly exogenous source of variation in retirement status – one that targets all working sectors – to estimate the causal effect of working on health. Although I use data from only one country, this itself is advantageous in that variation in retirement status cannot be traced to cross-country differences in health outcomes, practices, and systems. In addition, since Israel has similar labor force characteristics as other OECD countries, the consequences of changing the retirement age may be relevant to other countries that consider rising their retirement age as well.

The paper is organized as follows: Section 2 describes Israel's mandatory retirement age reform. Section 3 presents the data and discusses the main variables of interest. Section 4 presents the econometric model, with the main results in Section 5. Section 6 conducts a placebo analysis and Section 7 concludes.

2. Reform in Israel's Mandatory Retirement Age

On April 1, 2004, Israel legislated a gradual increase in the male full retirement age from sixty-five to sixty-seven (Table 1) and the female retirement age from sixty to sixty-two.[3] According to the new law, men cannot receive their full pension until they turn sixty-seven. Consequently, for example, a man who retires early at age sixty-five loses 13.6 percent of his monthly pension.

The employment rates among men of different ages from 2000 through 2011 are displayed in Figure 1, which uses data from the Israel Labor Force Survey.[4] For men in the ages affected by the reform, the figure shows an upward trend in their employment rate since the reform was enacted in 2004. In contrast, the age groups above those affected by the reform (72+) and below (22-64) had similar employment rates before and after the reform in 2004.

Figure 2 contrasts the standardized employment rate of men in the affected cohorts with ages below and above the reform ages.[5] Again, this figure highlights the dramatic change in the employment rates of males between the ages of 65 and 66 (an increase of 40%), which were the exact ages that were incentivized to keep working after the reform in 2004.

[3] The early retirement age for men was not changed; that for women was raised from fifty-five to sixty. The study focuses on men due to the trend among women to reenter the labor market at all ages. Source: http://www.moit.gov.il/NR/exeres/DAE44655-250A-4229-ABA0-BE33D154046D.htm

[4] Figure 1 was created on the basis of Labor Force Surveys conducted between 2000 through 2011. Only Panel A, "New Individuals" is presented (in these surveys, individuals were interviewed several times, therefore only those who were interviewed for their first time (new individuals) are presented). Employment rates were calculated using the variable annual labor force characteristics, with individuals who worked sometime in the past year defined as employed.

[5] In the Health Surveys, ages are given in ranges, e.g., 65–69. The increase in employment in this age range occasioned by the reform is perceptible. See discussion in Section 3 ("Data").

3. Data

To analyze how the retirement reform affected working and subsequent health, I use three different datasets: the Israeli Health Surveys, the Israeli Household Expenditure Surveys, and the Survey of Health, Ageing, and Retirement in Europe (SHARE).[6]

My analysis of the Israeli Health Surveys includes the pre-reform years of 1996 and 1999–2000, joined with the post-reform survey from 2009. The Israeli Health Survey is a national sample of repeated cross-sections (around 14,000 men per survey), and contains individual-level information on health, employment, and other demographic and socio-economic conditions. The specific health measures used in my analysis include hypertension, diabetes, heart attacks, other heart problems, asthma, cancer, lung disease, ulcers, and strokes. In addition, individuals are asked whether they have seen a family doctor or any other medical specialist in the past two weeks.

The health–labor literature commonly creates and examines an indicator for the overall health of the individual, rather than looking at specific illnesses. Following Dwyer and Mitchell (1999), I construct a general health index that counts the number of the aforementioned health conditions that each individual reports. This index can take values between 0 and 9. In addition, I constructed a physician index scaled between 0 and 2 that counts whether an individual visited a family physician or a medical specialist in the past two weeks.

My second set of health measures comes from the Israeli Household Expenditure Surveys for 1997 through 2011, which are also repeated cross-sections.[7] Each survey contains expenditures on healthcare and other goods by approximately 5,000 households,

[6] For more detailed information on the data and the constructed variables, see the Data Appendix.
[7] The ICBS Health Surveys, Household Expenditure Surveys, and Labor Force Surveys were provided to me by the Israel Social Science Data Center (ISDC), The Hebrew University of Jerusalem.

as well as individual data on the employment and income of around 10,000 men per year. I constructed a "Healthcare Expenditure Index" by summing health insurance premiums, expenditures on healthcare services, and other health-related goods and services that each individual consumes (excluding dental care), and then dividing total household expenditures by the number of household members.[8]

Finally, I used the longitudinal dimension of the Survey of Health, Ageing, and Retirement in Europe – SHARE – conducted in two waves in Israel (Wave 1: 2005–2006, Wave 2: 2009–2010).[9] Wave 1 contains 2,598 individuals and Wave 2 includes 2,464. SHARE presents a wide range of detailed information on the health, socioeconomic status, and life habits of each respondent. To exploit the panel dimension of the SHARE data, my sample includes individuals who appeared in both waves. Individuals who reported different years and months of birth in the two waves were excluded from the analysis.[10] My final sample contains 582 men in each survey. Following Coe and

[8] For more information on the Expenditure Surveys and construction of the variables, see the Data Appendix.

[9] "This paper uses data from SHARE wave 1 and 2 release 2.6.0, as of November 29 2013 (DOI: 10.6103/SHARE.w1.260 and 10.6103/SHARE.w2.260). The SHARE data collection has been primarily funded by the European Commission through the 5th Framework Programme (project QLK6-CT-2001-00360 in the thematic programme Quality of Life), through the 6th Framework Programme (projects SHARE-I3, RII-CT-2006-062193, COMPARE, CIT5- CT-2005-028857, and SHARELIFE, CIT4-CT-2006-028812) and through the 7th Framework Programme (SHARE-PREP, N° 211909, SHARE-LEAP, N° 227822 and SHARE M4, N° 261982). Additional funding from the U.S. National Institute on Aging (U01 AG09740-13S2, P01 AG005842, P01 AG08291, P30 AG12815, R21 AG025169, Y1-AG-4553-01, IAG BSR06-11 and OGHA 04-064) and the German Ministry of Education and Research as well as from various national sources is gratefully acknowledged (see www.share-project.org for a full list of funding institutions)." For further information on the SHARE project, visit www.share-project.org/. For further information on the basic literature on SHARE research and methodology see: Börsch-Supan et al. (2005), Börsch-Supan and Jürges (2005), Börsch-Supan et al. (2008) and Börsch-Supan et al. (2013).

[10] Furthermore, according to those who conducted the Israel-SHARE project, immigrants from the former Soviet Union did not answer the subjective health questions properly, and therefore, were dropped from the sample.

Zamarro (2011),[11] I estimated the following equation to create a general measure of a person's overall health from the SHARE data:

$$(1)\quad Self\ Perceived\ Health_i = \alpha_0 + \alpha_1\ Objective\ Health\ Measures_i + \zeta_i$$

This estimation yields a general health index composed of all individual health conditions, each weighted by its importance in determining overall health.[12] For the subjective measure on the left-hand side, I used the question "Would you say your health is excellent, very good, good, fair, or poor?"[13] As objective measures on the right-hand side, I used the following variables: the number of chronic problems, limitations with activities, number of symptoms reported by each individual, number of limitations in activities of daily living, number of limitations in instrumental activities of daily living (preparing a hot meal; shopping for groceries; making telephone calls; taking medications, etc.), whether the individual was hospitalized in the past year, physical inactivity, number of limitations with mobility, arm function and fine motor function, grip strength, and a depression index.

The goal of the paper is to determine how these health measures are affected by employment status. Employment status is captured with a dummy variable for being employed, constructed from the labor-market information in the Health and Household Expenditure Surveys.

The health indices and employment rates of different age groups from the 2000 and 2009 Health Surveys and the Expenditure Surveys are presented in Table 2. The first

[11] I changed Coe and Zamarro (2011) objective health variables slightly and did not use obesity measures (BMI and BMI2) due to missing values.
[12] I calculated the predicted values of self-reported health on objective health variables using OLS.
[13] This question was available in both waves.

row in Panel A refers to the treated-group ages (65–69), while the remaining rows represent different control groups consisting of men in unaffected age ranges. Table 2 shows that the treated group experienced a sharp and statistically significant increase in their employment rate. A significant increase in morbidity, indicated by the Poor Health Index, is observed across all ages but is significantly greater in the treated group. In addition, the Physician Visits Index increased by 0.07 points in the treated group while small and statistically insignificant changes were observed in the control groups.[14]

Panel B in Table 2 presents similar patterns using health measures derived from the Expenditure Surveys.[15] [16] [17] Panel B shows that healthcare expenditures increase much more dramatically for the treated group relative to the control groups. Specifically, the control groups (ages 25–64 and 72–75) experienced increases of only NIS 62 and NIS 90, respectively, while the treated group sustained a larger increase of NIS 102. The findings in Table 2, using health expenditures, indicate that the reform produced a significant rise in the employment of the targeted population of men, while at the same time resulting in a deterioration of their health.

In contrast, these patterns are not found for dental care expenditures. As shown in Table 2, dental expenditures declined in the treated group by a statistically insignificant

[14] Ages below the reform ages, however, are affected indirectly by the reform. For example, a man aged sixty-three in 2000 who knew that he had to work until age sixty-five might have retired when he turned sixty-three. In 2009, a man who was sixty-three years old would have to work until he turned sixty-seven. Therefore, unless he remained in the labor market, his retirement funds would be seriously harmed, as discussed in Section 2.

[15] I chose ages 72–75 as a control group because men in this bracket were not affected by the reform in any way.

[16] The Expenditure Surveys have some issues that should be noted: In my analysis, I used ages 25–75. These ages were divided into two subgroups: treated (65–66) and control. Furthermore, since the reform was enacted in 2004 and implemented gradually (see Section 2), I examined the years 1997–2003 (before the reform) and 2007–2011 (after the reform) separately. I deleted 2004–2006 from my sample due to the way the reform was implemented. I dropped the year 2000 from this part of the analysis because I could not merge it with each individual due to technical issues.

[17] Since ages 67–71 in years 2010–2011 were also affected by the reform, they are omitted from Table 2.

NIS 31, while decreasing by NIS 6.7 for the control group of men between ages 25-64 and increasing by 5.76 for the control group in ages 72-75. Dental care is unlikely to be related to work-related health issues, and therefore, these findings reinforce the idea that the deterioration in health of the treated group resulted from their increased employment, rather than a spurious trend in overall health care habits and expenditures.

Overall, Table 2 presents evidence that the reform led to a deterioration in the health of those encouraged to work during their sixties. However, this is just a first pass through the data, and does not test for whether these patterns are significantly different from other groups and does not control for other confounding factors. This will be explored in the remainder of the paper.

4. Econometric Model

To identify a causal relationship between employment status and state of health, I estimate the following basic model:

$$H_i = \beta_0 + \beta_1 LFP_i + \beta_2 X_i + u_i \quad (2)$$

where H_i is the health outcome of person *i*, the LFP_i is the labor force status of person *i*, X_i is a vector of covariates including: marital status, whether the individual is insured through a health fund,[18] district of residence, number of persons dwelling in the household, education, and religion (presented in Table 3).[19] The u_i term represents unobservable characteristics of the individual. If employment status (LFP_i) is

[18] Israel has a National Health Insurance Law in which every citizen is eligible to receive health care services. However, around 1.5% of respondents in the Health Surveys answered in the "not insured"/"do not know" categories. Therefore I control for health insurance throughout the analysis, although it has little effect on the parameter estimates of interest. For more information on the Israeli National Health Insurance Law visit:
http://www.health.gov.il/English/Topics/RightsInsured/Pages/default.aspx

[19] In the Health Expenditures surveys I control for slightly different variables.

uncorrelated with unobservable characteristics u_i, OLS estimation of equation (2) will yield an unbiased and consistent estimator of β_1.

The econometric concern, however, is that health status almost certainly affects the decision to work or not. However, the direction of the effect is theoretically not clear. Poor health may induce individual *i* to leave the labor market. But, poor health may force individual *i* to keep his job in order to afford the medical care that he needs. Therefore, estimating (2) via OLS is likely to yield a biased and inconsistent estimator of β_1.

Using the increase in retirement age as an instrument variable can solve this potential endogeneity problem. Suppose individual i aged 65 is forced by law in year 2004 to continue working. The use of the interaction term of age 65, and the relevant new law enforcement year of 2004 can lead to a valid instrument (Z_i), as follows:

$$(3)\quad Z_i = \left\{ \begin{array}{c} 1 \quad if\ age = (65 \text{ or } 66)\ \ and\ year \geq 2004 \\ 0 \quad otherwise \end{array} \right\}$$

In other words, the instrument is an indicator defined as 1 for being in a group affected by the reform (ages 65-66 in the years after the reform) and 0 otherwise. Panel B of Table 4 summarizes my identification strategy.

I expect the ages affected by the reform to experience higher employment rates after the reform than all other ages, since this was the goal of the policy. Therefore, in order to be a valid instrument, the instrument described above must be correlated with employment status. Table 5 demonstrates that this is indeed the case, with F-statistics in the First Stage regressions above 10 (Staiger and Stock, 1997).

The identifying assumption is that there is no other factor or policy that affected this particular age group in this particular time other than the increased incentives to stay in the workforce. If this holds, a change in the health and employment status of

individuals in this particular age group during the affected years would be evidence in favor of a causal effect of working on an individual's health. I would support this claim by controlling for existing trends, using health measures unrelated to employment (e.g. dentist visits) as placebo tests, and other measures that should not be affected from the new policy (e.g. cloth expenditures) as additional robustness checks.

In the last part of my analysis, I use the instrumental variable strategy mentioned above including individual fixed effects by exploiting the SHARE dataset's longitudinal nature (as summarized in panel A of Table 4).

5. Results

A. Health Surveys

Table 6 displays the estimates for equation (2) from OLS, IV, and the reduced-form. The analysis uses three different samples, each one has the same treatment group but uses different age ranges for the control group. The first three columns use all age groups in the sample (including those below 65 and those above 69), and the OLS estimates reveal that working is positively correlated with health – a familiar pattern which most likely stems from healthier individuals being more able to work.[20] The 2SLS coefficients, however, reveal causal effects in the opposite direction. According to the two different health measures (the "Poor Health Index" and number of "Physician visits" in the past two weeks), working impairs health.

The second sample compares ages 65–69 with the rest of the labor market (ages 20–64). As previously found, for the two health measures: "Poor Health Index" and number of "Physician visits" in the past two weeks, OLS yields a positive correlation

[20] Since the outcome is the number of health conditions, a negative coefficient means fewer health conditions.

between working and health while 2SLS reveals an opposite causal relationship. The last sample in Table 6 compares those aged 65–69 with those aged 70–74. For the "Poor Health Index" and number of "Physician visits" in the past two weeks, although the coefficient is statistically insignificant, the sign and magnitude are the same as those using a broader age range as the control group. The imprecision is most likely due to a smaller sample size. Although not reported, using an additional control group (ages 40–74) yields similar results. Namely, the OLS coefficient is negative while the 2SLS and the reduced form coefficients are positive with similar magnitudes as the analysis using 20-64 year olds, but with p-values approximately 15 percent.

As a robustness check, panels A and B in Appendix Table 1 present the same 2SLS analysis as Table 6 but without control variables. As displayed in the table, the results are similar. The robustness of the results to the inclusion or exclusion of several control variables shows that my main findings are not due to the treatment variable being correlated with observable factors which affect health. This pattern is consistent with my identifying assumption that the treatment is not correlated with unobserved factors which may have affected the treatment group at the same time.

A possible concern about the results in Table 6 may be that individuals who are forced to work at ages sixty-five and sixty-six will try to shirk this obligation by reporting poor health. To address this issue, I constructed a "Severe Morbidity Indicator" that receives the value one for being diagnosed with one or more of the severe ailments – heart attack, stroke, diabetes, cancer, and whether an individual has visited a family physician or specialist physician in the last two weeks. These ailments require a precise physician's diagnosis, and therefore, are unlikely to be exaggerated or faked in an attempt

to get out of working. As Table 7 shows, the ages affected by the reform are more likely to suffer from acute problems, which indicates that the deterioration in reported health in Table 6 was serious as well as statistically significant. As displayed in the table, the results are robust to the inclusion or exclusion of several control variables, suggesting that my main findings are not due to the treatment variable being correlated with observable factors which affect health.

In addition, the Severe Morbidity Indicator among men of different ages before and after the reform is displayed in Figure 3. For men in the ages affected by the reform (ages 65-69), the figure shows a sharp increase in their Severe Morbidity Indicator. In contrast, the age groups above those affected by the reform (70-74), below (20-64), and all other age groups had similar severe morbidity rates before and after the reform in 2004.

As a placebo test, I examine whether the treated group paid more visits to the dentist between 2000 and 2009. The assumption is that work-related changes in health should not show up in dental problems, but personal and aggregate factors which affect health over time are likely to be reflected in dental care as well. Table 6 shows that working more years does not lead to more dentist visits, which supports the causal interpretation of my estimates showing a deterioration in health due to delayed retirement.

It is possible that the effect of employment on health may depend on the person's occupation and subsequent work conditions. Lacking data on retirees' occupations during their working life, I use education as a proxy for physical versus non-physical occupations. Table 8 shows that those who have more than twelve years of schooling do

not suffer from increased health problems in response to working more years, as reflected by the insignificant level of the Poor Health Index. In contrast, the less-educated group is more likely to suffer from health problems, indicated by the larger and significant coefficient for the Poor Health Index. A similar pattern is found with the second health outcome: "Physician Visits". Employment causes the less-educated group to visit physicians more often, but there is no effect for the more educated group.[21]

B. Expenditure Surveys

Table 9 presents a similar analysis but uses health measures based on healthcare expenditures from the Expenditure Surveys. (Since ages 67–71 may have been affected by the reform, I exclude them from the analysis).[22] Appendix Table 2 presents the first-stage results.

The 2SLS results indicate that working an additional two years due to the increase in retirement age increases healthcare expenditures while insignificantly lowering dental expenditures. These contrasting results strengthen my previous conclusions regarding the causal effect of employment on health. As a robustness check, I present a placebo analysis using expenditures on clothing instead of health-related issues. The affected population (ages 65–66 in 2007–2011) spent more money on clothes but the effect is statistically insignificant.

[21] The F-test in the first stage was 10.37 for those who have more than twelve years of schooling and 8.84 for the less-educated group.

[22] In the earlier analysis using the Health Surveys, ages are given in ranges, e.g., 65–69, therefore I could not separate working individuals from retirees (however the age range of 65-69 was affected by the reform).

Table 9 presents the results when controlling for immigration status, marital status and education. Again, the findings are robust to the inclusion or exclusion of the other explanatory variables.[23]

If employment at ages 65-66 impairs health, I would expect that those who were affected by the reform would continue to suffer from health problems even a few years after retirement. Since men aged 67-68 in years 2007-2011 were affected by the reform, while same age cohort men in years 1997-2003 were not, I define men ages 67-68 in years 2007-2011 as the treated group. Since men age 72-75 were required to work until the age of 65 before (years 1997-2003) and after (years 2007-2011) the reform, I define them as the control group. Panel C of Appendix Table 1 presents the health and dental expenditures for the treated group, compared to the control group. As displayed, healthcare expenditures increase much more dramatically for the treated group relative to the control group. These results suggest that employment lead to health deterioration even a few years after retirement.

Once again, these patterns are not found for dental care expenditures. Dental care is unlikely to be related to work-related health issues, and therefore, these findings reinforce the idea that the deterioration in health of the treated group resulted from their increased employment, rather than a spurious trend in overall health care habits and expenditures.

[23] In particular, following Coe and Zamarro (2011), one may argue that individuals aged 65–66 in 2007–2011 were affected by the Holocaust, since many of them arrived from Europe in the aftermath of World War II. Therefore, including an indicator for immigration status is designed to control for this phenomena.

C. SHARE Results

The last part of my analysis uses the SHARE data and exploits its panel structure. As I did in the last part of the Health Expenditures Surveys, I assume that if employment at ages 65-66 impairs health, individuals who were affected by the reform would continue to suffer from health problems even several years after retirement. Since the first wave of SHARE-Israel took place after the reform went into effect (Wave 1 in 2005–2006, Wave 2 in 2009–2010), I do not have data preceding the reform in 2004. However, since men aged 66–71 in Wave 2 (2009–2010) were forced to work longer than men in the same age group in Wave 1 (2005–2006), I can compare the health of the two groups. Thus, I define men age 66–71 in Wave 2 as the treated group and all other ages as a control group. These ages – below and above 66–71 – were required to work the same years in both waves. For example, men age 72–75 had to work until age sixty-five in Wave 1 and in Wave 2; men age 60–64 had to work until age sixty-seven in both waves. Panel A of Table 4 presents the SHARE treated versus control groups in detail.

I used the panel dimension of the SHARE data and included individuals who appeared in both waves. Therefore, my final dataset contained 582 men in each wave.

For the SHARE analysis, I estimate the following equation:

(4)

$$Overall\ Poor\ Health\ Index_i = \mu_1 + \mu_2 age_i + \mu_3 year_i +$$

$$\mu_4 (ages\ 66-71) \cdot (years\ 2009-2010)_i + \gamma_i$$

The dependent variable – the Overall Poor Health Index – was constructed by equation (1). The impact of the reform would be captured by the interaction term (μ_4) in equation (4) because those aged 66–71 in 2009–2010 had to work more years than

individuals in the same age cohort in 2005–2006 (Wave 1). I also take advantage of the panel nature of the survey by including individual fixed effects (γ_i). Fixed individual characteristics (e.g., education) are already subsumed in the individual fixed effects. Therefore I did not include other covariates in the estimation such as: education, marital status, and household size.

As mentioned above, the SHARE dataset contains rich health measures and allows one to determine whether those affected by the reform continue to suffer from health problems even several years after retirement. As indicated above, I construct an overall health index by regressing the individual's self-reported health status on objective symptoms and health-related behaviors. The results of equation (1) are presented in Panel A of Table 10. Of the ten objective health variables that compose the "Overall Poor Health Index", seven were found significantly related to the subjective health variable and carried the expected sign. Namely, more objective health problems lead to severe subjective levels of health.[24]

Panel B of Table 10 presents the estimated effect of retiring later on the Overall Poor Health Index using the SHARE data and controlling for individual fixed-effects. The estimated causal effect of working between the ages of 65 and 67 on being in poor health is positive and statistically significant, indicating that working an additional two years from 65 to 67 impairs one's health. These findings reinforce my previous conclusions that employment at older ages leads to a deterioration in health.[25, 26]

[24] That is, the subjective health measure is higher for people in poor health.
[25] I did not find higher mortality rates in the treated group.
[26] Data for the "years working in last job" variable are missing for around half of my observations. Therefore, the impact of the reform on employment is not presented for the SHARE dataset.

6. Pre-Existing Trends

Up to now, my analysis controlled for aggregate changes over time in health, habits, and expenditures, by using age cohorts that were not affected by the reform as control groups. However, if the trend for the affected cohort was significantly different relative to other age groups at the time the reform was enacted, my results would yield spurious results. To see whether my findings are consistent with differential time trends in health for the affected cohort, I use the 1996 and 2000 Health Surveys to test whether persons aged 65–69 experienced an increase in morbidity in the pre-reform years. Since the reform took place only in 2004, I would not expect a similar pattern in those years if my previous results are causal.

Due to slight differences between the 1996 and 2000 Health Surveys, I constructed a "Physician Visits Index" by counting whether individuals visited physicians for one or more of the following: hypertension, heart attack, diabetes, strokes, asthma, or cancer.[27] The index receives the value of 6 if individuals suffered from all six health problems, 1 if they had only one disease, etc. The difference in employment rates for ages 65–69 between 1996–2000 and 2000–2009 is presented in Panel A of Table 11. The employment rates of those aged 65–69 clearly did not increase between 1996 and 2000, but climbed significantly between 2000 and 2009 (during the period when the reform was enacted). Panel B of Table 11 shows that visits to the doctor by members of the treated group (ages 65–69) increased significantly between 2000 and 2009 after an insignificant

[27] These variables are comparable among all health surveys. For further details, see the Data Appendix.

increase during 1996–2000. Among the control group, neither employment rates nor physician visits changed significantly over the years.[28]

These findings demonstrate the absence of pre-treatment trends in morbidity and in employment for individuals acutely affected by the retirement age reform. These patterns provide further support for the causal interpretation of the results showing a deterioration in health for the affected cohort right after the reform was enacted.

7. Conclusions

This paper analyzes the causal effect of retirement versus employment on the health status of men in their sixties. In previous studies, this question ran into simultaneity problems (i.e., health affects employment decisions). Some studies attempt to deal with this issue by using variations in policies across countries. A cross-country analysis, however, may suffer from differences in health that have nothing to do with official retirement ages. Other studies use reforms that allowed certain workers to retire earlier or retirement windows offered by employers. However, these strategies shed light on only a particularly sector of the labor force, and workers affected by these policies may not be comparable to the untreated population.

This paper exploits a new policy enacted in 2004 in Israel that incentivizes men in all sectors to work for two more years, from age sixty-five to sixty-seven. The increase in the retirement age was an exogenous decision of the Israeli government, totally unrelated to the health status of any individual. I showed that the employment rate of men affected by the reform increased significantly and that no pre-existing trends in the employment rate for the affected group were found.

[28] Visiting the doctor is an indicator of more severe problems. (The questions in the 2000 and 2009 Health Surveys first asked for a general description of state of health. If individual the answered in the affirmative, he was asked whether he had also visited a doctor in regard to the health condition in question.)

Using several measures of health status from three different datasets, I found that employment at older ages – beyond 64 – due to the increased retirement age is associated with a deterioration in health status. The results were more pronounced among less-educated workers, suggesting that employment in physically demanding occupations is more detrimental to health. In addition, I found that those who were affected by the reform continue to suffer from poor health even a few years after retirement.

A causal interpretation of these findings is supported by several placebo analyses using different variables as: dental care expenditures, clothing expenditures and dentist physician visits. These variables are unlikely to be affected by work-related health issues, and therefore, the lack of any effect on these measures supports the idea that the deterioration in health of the treated group resulted from their increased employment, rather than a spurious trend in overall health care habits and expenditures.

Given that the findings in the literature on this subject are quite varied, my study provides new causal evidence that working during these ages adversely affects health, particularly among less-educated men.

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7. Israel Social Science Data Center
   http://isdc.huji.ac.il/
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   www.health.gov.il

Figures

Figure 1 - Employment Rates for Men by Year and Age Group

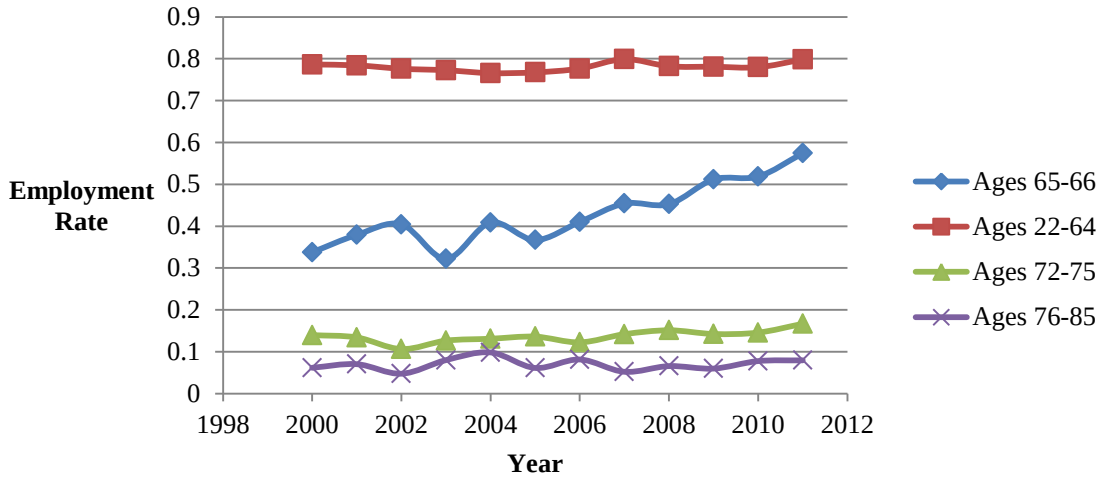
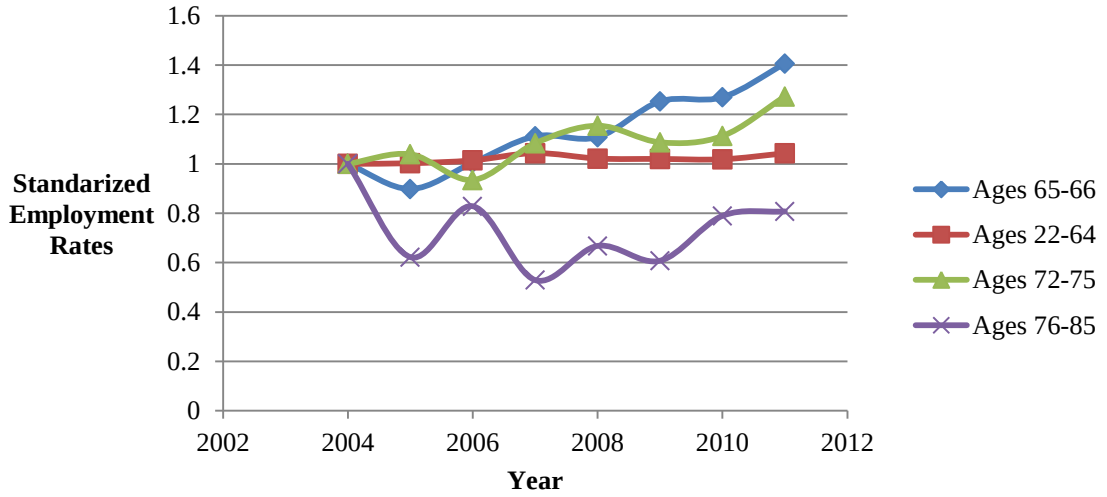


Figure 2 - Standarized Employment Rates for Men by Year and Age Group



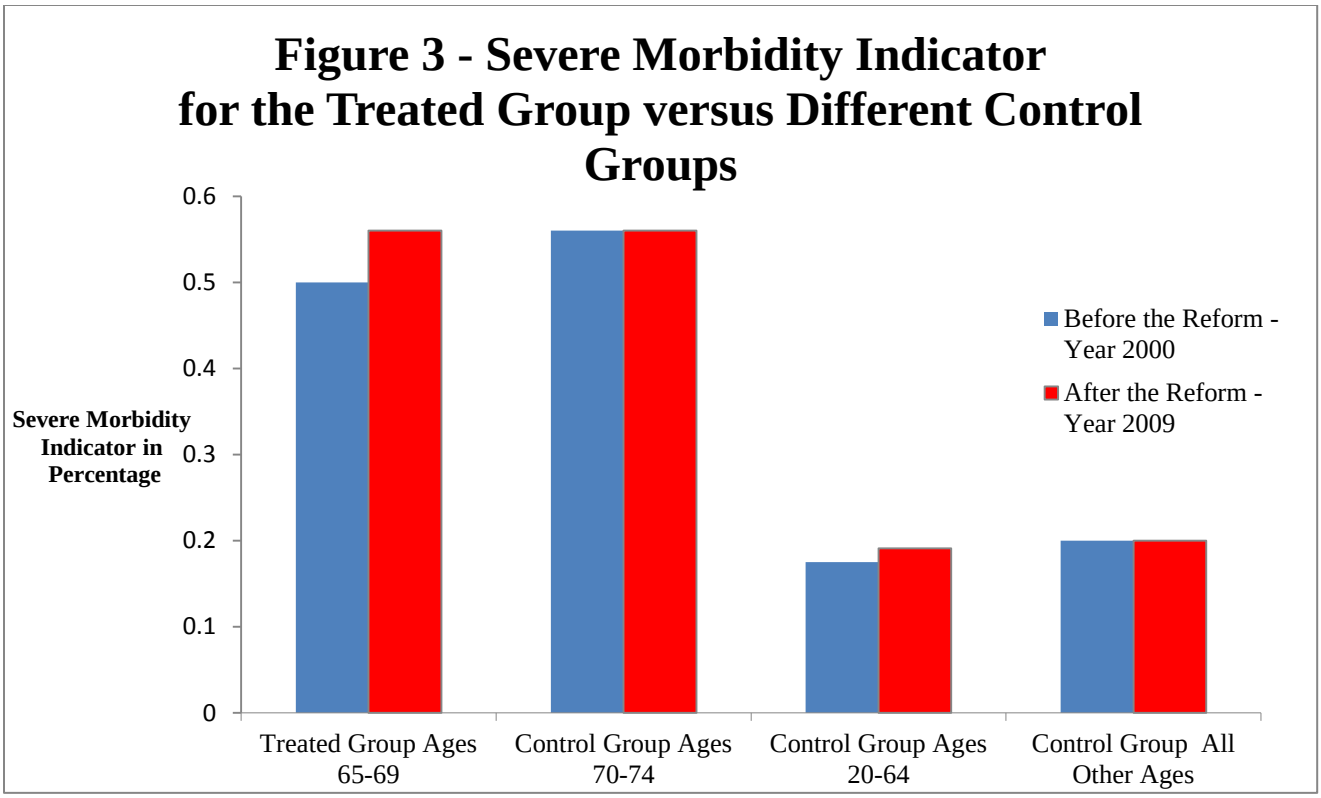


Figure 3 - Severe Morbidity Indicator for the Treated Group versus Different Control Groups
Severe Morbidity Indicator in Percentage
0.6
0.5
0.4
0.3
0.2
0.1
0
Before the Reform - Year 2000
After the Reform - Year 2009
Treated Group Ages 65-69
Control Group Ages 70-74
Control Group Ages 20-64
Control Group All Other Ages

Table 1 – Full Retirement Age for Men by Year and Month of Birth

Month and Year of Birth	Full Retirement Age in Years	Retirement Age Before the Reform	Retirement Age After the Reform
Until March 1939	65 years old	65	65
April 1939 until August 1939	65 years and 4 months	65	65.4
September 1939 until April 1940	65 years and 8 months	65	65.8
May 1940 until December 1940	66 years	65	66
January 1941 until August 1941	66 years and 4 months	65	66.4
September 1941 until April 1942	66 years and 8 months	65	66.8
After April 1942	67 years	65	67

Notes: The first two columns were taken from the Israeli ministry of Industry, Trade and Labor Internet site: http://www.moit.gov.il/NR/exeres/DAE44655-250A-4229-ABA0-BE33D154046D.htm

Table 2 – Employment and Health Measures Before and After the Reform across Treated and Control Groups

	Employment Rate			Poor Health index			Physician Visits		
	Before Reform Year 2000	After Reform Year 2009	Difference	Before Reform Year 2000	After Reform Year 2009	Difference	Before Reform Year 2000	After Reform Year 2009	Difference
Panel A. Health Survey									
(1) Treated Group Ages 65-69	0.27 (0.02) [447]	0.41 (0.02) [411]	0.14*** (0.03)	1.01 (0.04) [447]	1.22 (0.05) [411]	0.21*** (0.07)	0.32 (0.02) [447]	0.39 (0.02) [411]	0.07* (0.03)
(2) Control Group All Other Ages	0.45 (0.004) [13,501]	0.44 (0.004) [13,752]	-0.005 (0.006)	0.24 (0.005) [13,501]	0.3 (0.006) [13,752]	0.05*** (0.008)	0.17 (0.003) [13,501]	0.16 (0.003) [13,752]	-0.006 (0.004)
(3) Labor Market Ages 20-64	0.79 (0.004) [7,170]	0.77 (0.004) [7,493]	-0.02*** (0.006)	0.25 (0.006) [7,170	0.32 (0.008) [7,493]	0.07*** (0.01)	0.14 (0.004) [7,170]	0.14 (0.004) [7,493]	0.002 (0.006)
(4) Ages 70-74	0.15 (0.01) [374]	0.16 (0.01) [358]	0.009 (0.02)	1.18 (0.05) [374]	1.23 (0.06) [358]	0.04 (0.08)	0.42 (0.03) [374]	0.42 (0.03) [358]	0.002 (0.04)
Panel B. Expenditures Surveys									
	Healthcare Expenditures						Dental Care Expenditures		
years	1997-2003	2007-2011	Diff				1997-2003	2007-2011	Diff
(5) Treated Group Ages 65-66	229.41 (18.59) [623]	331.54 (16.21) [585]	102.127*** (24.67)				128.68 (19.53) [623]	97.68 (19.48) [585]	-31 (27.59)
(6) Control Group Ages 25-64	105.04 (1.42) [20,495]	167.01 (2.21) [18,821]	61.97*** (2.63)				63.66 (1.49) [20,495]	56.93 (1.79) [18,821]	-6.72*** (2.33)
(7) Control Group Ages 72-75	308.24 (20.51) [923]	398.50 (17.17) [920]	90.25*** (26.75)				93.04 (9.89) [923]	98.8 (14.92) [920]	5.76 (17.91)

Notes: This table presents t – test for differences between means. The Healthcare and Dental care Expenditures are in 2011 terms according to the following formula: prices in 2011 terms = prices in current year $\cdot \frac{\text{CPI in 2011}}{\text{CPI in current year}}$. Standard errors are in parenthesis. Numbers of observations are in brackets. * indicates significance at the 10% level. ** indicates significance at the 5% level. *** indicates significance at the 1% level.

Table 3 – Descriptive Statistics

Panel A. Health Surveys

	Treated Group Ages 65-69			Control Group All Other Ages		
	Before Reform Year 2000	After Reform Year 2009	Difference	Before Reform Year 2000	After Reform Year 2009	Difference
(1) Insured through Health Fund	0.98 (0.005) [447]	0.98 (0.006) [411]	-0.001 (0.008)	0.98 (0.0009) [13,501]	0.98 (0.001) [13,752]	-0.004*** (0.001)
(2) Married	0.86 (0.01) [447]	0.88 (0.01) [411]	-0.01 (0.02)	0.42 (0.004) [13,501]	0.41 (0.004) [13,752]	-0.01** (0.005)
(3) High Education	0.34 (0.02) [442]	0.47 (0.02) [410]	0.12*** (0.03)	0.4 (0.004) [13,446]	0.49 (0.004) [13,676]	0.08*** (0.006)

Panel B. Expenditures Surveys

	Treated Group Ages 65-66			Control Group Ages 25-75 (excluding 65-71)		
(4) Employed	0.29 (0.01) [848]	0.52 (0.02) [594]	0.22*** (0.02)	0.76 (0.002) [31,929]	0.76 (0.002) [22,781]	0.003 (0.003)
(5) Immigration	0.77 (0.01) [848]	0.63 (0.01) [594]	-0.13*** (0.02)	0.41 (0.002) [31,929]	0.33 (0.003) [22,781]	-0.07*** (0.004)
(6) B.A Holders	0.25 (0.01) [848]	0.29 (0.01) [594]	0.04* (0.02)	0.28 (0.002) [31,929]	0.31 (0.003) [22,781]	0.03*** (0.003)
(7) Married	0.89 (0.01) [848]	0.87 (0.01) [594]	-0.02 (0.01)	0.8 (0.002) [31,929]	0.77 (0.002) [22,781]	-0.03*** (0.003)
(8) Age	65.44 (0.01) [848]	65.49 (0.02) [594]	0.04* (0.02)	43.17 (0.06) [31,929]	44.3 (0.08) [22,781]	1.12*** (0.1)

Notes: This table presents t – test for differences between means. In panel B ages 67-71 were excluded. Standard errors are in parenthesis. Numbers of observations are in brackets. * indicates significance at the 10% level. ** indicates significance at the 5% level. *** indicates significance at the 1% level.

Table 4 - SHARE Treated and Control Groups and Instrumental Variable Identification Strategy

Panel A: SHARE Treated and Control Groups*

Group/Year	Age	Wave 1 Years 2005-2006 "Before"	Wave 2 Years 2009 – 2010 "After"
$Treated\ Group^a$	66-71	Work fewer years than same age individuals at Wave 2	Work more years than same age individuals at Wave 1
$Control\ Group^b$ $Older\ Ages$	72 and older	Worked until 65	Worked until 65
$Control\ Group^c$ $Younger\ Ages$	65 and younger	Work in Both Waves	Work in Both Waves

Panel B: Health and Expenditure Surveys' Instrumental Variable Identification Strategy**

Row	Sample	Before the Reform (Before 2004)	After the Reform (After 2004)
1	Treated Group - Ages 65-66	$Instrument\ z = 0$	$Instrument\ z = 1$
2	Control Group - Other Ages	$Instrument\ z = 0$	$Instrument\ z = 0$

Notes: *Panel A presents SHARE's treated and control groups. ^{a} Individuals who were born before April 1939 were not affected by the reform, therefore were excluded from the treated group. Ages 66 in wave one (years 2005-2006) were born until 1941, therefore worked until the age of 66. Ages 66 in wave two (years 2009 – 2010) are required to work until the age of 67. Therefore ages 66 were included in the treated group. Ages 67 in wave one, were born until 1940 therefore worked until the age of 65 and 8 months. Ages 67 in wave two, were required to work at least up to the age of 66 and 8 months. Therefore ages 67 were included in the treated group. Ages 68-69 in wave 2 were affected by the reform, while ages 68-69 in wave 1 were not. Ages 70-71 in wave 1, were not affected by the reform, while ages 70-71 in wave 2, who were born after March 1939 were affected by the reform, therefore were included in the treated group.

^{b} – Ages 72 and older were required to work until the age of 65 in both waves.

^{c} – Ages 65 and younger were required to work in both waves.

** Panel B presents the Instrumental Variable identification strategy in the Health and Expenditures Surveys.

Table 5 - First Stage Regressions Health Surveys 2000 and 2009

Dependent Variable	Labor Force Participation			
Sample	(1) All Ages Without Control Variables	(2) All Ages With Controls	(3) Ages 20-69	(4) Ages 65-74
Instrument	0.15***	0.15***	0.16***	0.13***
	(0.03)	(0.03)	(0.03)	(0.04)
Control Variables	No	Yes	Yes	Yes
Observations	28,110	27,953	15,467	1,576
R- squared	0.56	0.57	0.15	0.11
F- test	23.75	23.57	23.87	11.33

Notes: This table presents the "First Stage" regression using OLS. The dependent variable takes the value 1 if individual ϵ work and zero otherwise. All the regressions include age and year dummies. The covariates: religion, marital status, number of persons dwelling in household, district of living, health fund and education were included in columns 2-4. Robust Standard errors are in brackets. * indicates significance at the 10% level, ** indicates significance at the 5% level, *** indicates significance at the 1% level.

Table 6 - OLS vs. 2SLS Results in Health Surveys 2000 and 2009

Column	All Ages			Ages 20-69			Ages 65-74		
	(1) OLS	(2) 2SLS	(3) Reduced Form	(4) OLS	(5) 2SLS	(6) Reduced Form	(7) OLS	(8) 2SLS	(9) Reduced Form
Panel A. Health Outcome:									
Poor Health Index	-0.16***	1.09*	0.17**	-0.18***	1.009*	0.16**	-0.34***	1.32	0.18
	(0.01)	(0.56)	(0.07)	(0.01)	(0.55)	(0.07)	(0.06)	(0.96)	(0.11)
Observations	27,953	27,953	27,953	15,467	15,467	15,467	1,576	1,576	1,576
Panel B. Health Outcome:									
Physician Visits	-0.04***	0.5*	0.07**	-0.04***	0.45*	0.07*	-0.15***	0.49	0.06
	(0.007)	(0.27)	(0.03)	(0.008)	(0.26)	(0.03)	(0.03)	(0.45)	(0.05)
Observations	27,953	27,953	27,953	15,476	15,467	15,476	1,576	1,576	1,576
Panel C. Health Outcome:									
Dentist Physician	0.007	-0.13	-0.02	0.007	-0.1	-0.01	0.006	-0.32	-0.04
	(0.004)	(0.12)	(0.01)	(0.004)	(0.12)	(0.01)	(0.01)	(0.21)	(0.02)
Observations	27,953	27,953	27,953	15,567	15,467	15,467	1,576	1,576	1,576

Notes: This table presents OLS, 2SLS and Reduced Form results for each sample. The explanatory variable in the OLS and 2SLS columns is LFP and the explanatory variable in the Reduced Form regressions is the instrument's coefficient. All the regressions include age and year dummies and control variables. The control variables are: religion, district, marital status, health fund, number of persons dwelling in house and education. Robust standard errors are in parenthesis. *indicates significance at the 10% level, ** indicates significance at the 5% level, *** indicates significance at the 1% level.

Table 7 – Severe Health Conditions – Health Survey

Sample	All Ages – Without Controls			All Ages – With Controls		
	(1) OLS	(2) 2SLS	(3) Reduced Form	(1) OLS	(2) 2SLS	(3) Reduced Form
Panel A- Health outcome:						
Severe Morbidity Index	-0.05*** (0.007)	0.4* (0.24)	0.06* (0.03)	-0.06*** (0.007)	0.42* (0.24)	0.06* (0.03)
Observations	28,110	28,110	28,110	27,953	27,953	27,953

Notes: This table presents OLS, 2SLS and Reduced Form results. The explanatory variable in the OLS and 2SLS columns is LFP and the explanatory variable in the Reduced Form regressions is the instrument's coefficient. All the regressions include age and year dummies. The control variables are: religion, district, marital status, health fund, education and number of persons dwelling in house. Robust standard errors are in parenthesis. *indicates significance at the 10% level, ** indicates significance at the 5% level, *** indicates significance at the 1% level.

Table 8 – Health Outcomes by Education – Health Surveys

	Less Than Twelve Years of Schooling			More Than Twelve Years of Schooling		
Column	(1) OLS	(2) 2SLS	(3) Reduced form	(4) OLS	(5) 2SLS	(6) Reduced Form
Panel A. Health outcome:						
Poor Health Index	-0.18***	1.91*	0.23**	-0.09***	0.44	0.07
	(0.01)	(1.15)	(0.1)	(0.01)	(0.7)	(0.11)
Observations	15,397	15,397	15,397	12,577	12,577	12,577
Panel B. Health Outcome:						
Physician Visits	-0.04***	1.13*	0.13***	-0.01**	-0.05	-0.008
	(0.009)	(0.6)	(0.05)	(0.01)	(0.34)	(0.05)
Observations	15,397	15,397	15,397	12,577	12,577	12,577

Notes: This table presents OLS, 2SLS and Reduced Form results. The explanatory variable in the OLS and 2SLS columns is LFP and the explanatory variable in the Reduced Form regressions is the instrument's coefficient. All the regressions include age and year dummies. Robust standard errors are in parenthesis.*indicates significance at the 10% level, ** indicates significance at the 5% level, *** indicates significance at the 1% level.

Table 9 - OLS vs. 2SLS Results in Expenditures Surveys 1997-2011

	(1) OLS		(2) 2SLS		(3) Reduced Form	
	Without Controls	With Controls	Without Controls	With Controls	Without Controls	With Controls
Health Outcome:						
Healthcare Expenditures Index	59.62***	48.11***	211.29*	209.09*	45.34*	44.6*
	(3.57)	(3.47)	(118.43)	(118.53)	(25.22)	(25.02)
Observations	42,367	42,367	42,367	42,367	42,367	42,367
Panel C. Health Outcome:						
Dental care Expenditures	17.94***	17.05***	-103.94	-104.83	-22.3	-22.36
	(3.42)	(3.34)	(134.71)	(135.32)	(28.72)	(28.69)
Observations	42,367	42,367	42,367	42,367	42,367	42,367
Panel D. Outcome:						
Clothes Expenditures	52.5***	45.92***	69.91	65.14	14.49	13.49
	(2.71)	(2.71)	(75.93)	(75.73)	(15.85)	(15.78)
Observations	44,349	44,349	44,349	44,349	44,349	44,349

Notes: This table presents OLS, 2SLS and Reduced Form results. The Healthcare, Clothes and Dental Care Expenditures are in 2011 terms according to the following formula: prices in 2011 terms = prices in current year $\cdot \frac{\text{CPI in 2011}}{\text{CPI in current year}}$. The explanatory variable in the OLS and 2SLS columns is LFP and the explanatory variable in the Reduced Form regressions is the instrument's coefficient. All the regressions include age and year dummies. The controls are: marital status, immigration and education. Robust standard errors are in parenthesis. Ages that were included are 25-75 (excluding ages 67-71) in all estimations.* indicates significance at the 10% level, ** indicates significance at the 5% level, *** indicates significance in the 1% level.

Table 10 – SHARE Results

Panel A: Estimating the Subjective Health Index

	Dependent variable: Subjective Health Status
Number of Chronic Problems	0.18*** (0.02)
Number of Symptoms	0.05** (0.02)
Mobility Limitations	0.04* (0.02)
Limitations with activities of daily living	-0.08 (0.05)
Limitations with instrumental activities of daily living	0.03 (0.04)
Limitations with activity	0.56*** (0.07)
Physical inactivity	0.37*** (0.08)
In hospital in last 12 months	0.1 (0.08)
Max. of grip strength measure	-0.008*** (0.003)
Depression scale – high is depressed	0.05*** (0.01)
Observations	995
R- Squared	0.39

Panel B:	Dependent variable: Overall Poor Health Index
Interaction^{a}	0.43* (0.22)
Age Dummies	Yes
Year Dummies	Yes
Individual Dummies	Yes
Observations	998

Notes: Panel A presents equation (1) in the text using OLS. The subjective health status is a categorical variable that was defined on the following ascending scale: excellent, very good, good fair or poor. Panel B presents equation (4) in the text. Robust standard errors are in brackets. *indicates significance at the 10% level ** indicates significance at the 5% level, *** indicates significance at the 1% level. ^{a}– The interaction indicates the μ_4 coefficient in equation (4) in the text.

Table 11 – Employment and Health Measures - Placebo Analysis

	Health Surveys					
	(1) Year 1996	(2) Year 2000	(3) Difference Between 1996 to 2000	(4) Year 2000	(5) Year 2009	(6) Difference Between 2000 to 2009
Panel A. Employment Rate						
(1) Placebo Treated Group Ages 65-69	0.28 (0.02) [467]	0.27 (0.02) [447]	-0.01 (0.02)	0.27 (0.02) [447]	0.41 (0.02) [411]	0.14*** (0.03)
(2) Control Group All Other Ages	0.47 (0.004) [14,027]	0.45 (0.004) [13,501]	-0.01*** (0.006)	0.45 (0.004) [13,501]	0.44 (0.004) [13,752]	-0.005 (0.006)
Panel B. Physician Visits						
(3) Placebo Treated Group Ages 65-69	0.62 (0.03) [467]	0.68 (0.03) [446]	0.05 (0.05)	0.68 (0.03) [446]	0.8 (0.04) [410]	0.12*** (0.05)
(4) Control Group All Other Ages	0.13 (0.003) [14,024]	0.14 (0.003) [13,493]	0.002 (0.005)	0.14 (0.003) [13,493]	0.17 (0.004) [13,744]	0.03*** (0.005)

Notes: This table presents t- test for differences between means. In panels C and D ages 67-71 were excluded. Standard errors are in parenthesis. Numbers of observations are in brackets. * indicates significance at the 10% level. ** indicates significance at the 5% level. *** indicates significance at the 1% level.

Supplemental Materials

Data Appendix

A. Health Surveys

In this study I used three Health Surveys covering the years 1996, 1999–2000, and 2009; these are repeated cross-sections that contain around 28,000 men and women per survey. The core questions examine trends and developments in morbidity in Israel. The surveys contain individual specific health, employment, and sociodemographic measures.[29] In the 2009 survey, the questions were: "Has a doctor ever diagnosed a member of your households with any of the following illnesses: hypertension, myocardial infarction, other cardiac disease (including cardiac rhythm disorder), stroke, diabetes, asthma, chronic lung disease, chronic digestive problem, [or] malignant disease (cancer)?"[30] For the physician visit index, were asked, "In the past two weeks, has anyone in your household visited a doctor or consulted with a doctor over the phone (including house calls; not including dentists and telephone consultations for prescriptions)?"[31]

The other covariates in the 2000 and 2009 surveys were:

Health Fund – an indicator variable that receives the value of 1 for being insured with a health fund, and 0 if uninsured or unknown.

Married – an indicator variable that receives the value of 1 for being married and 0 otherwise.

[29] More information on the Health Surveys can be found at:
http://www.cbs.gov.il/reader/?MIval=cw_usr_view_SHTML&ID=598

[30] In the 2000 survey, the question was: "Has a doctor ever diagnosed a member of your family with any of the following illnesses: hypertension, myocardial infarction, other cardiac disease (including cardiac rhythm disorder), stroke, diabetes, asthma, lung disease, digestive system ulcer, [or] malignant disease (cancer)?"

[31] For the 2000 survey, I asked, "In the past two weeks, has anyone in your household visited a doctor (not including a dentist or an orthodontist)?"

Number of persons in household – a categorical variable that indicates the number of persons in the household.[32]

District – a categorical variable that indicates the district of residence.

Religion – a categorical variable that indicates individuals' religion.

Education - was defined as an indicator variable that receives the value one if years of schooling are over twelve years. The less educated were defined as those who reported on zero to twelve (including) years of schooling.

The 1996 survey, instead of asking "Has a doctor ever diagnosed a member of your household with any of the following illnesses?" as in the 2009 survey, asks: "[Are you] visiting a doctor for specific health conditions?"[33] To compare the 1996–2000 period with 2000–2009, I used the 2000 and 2009 questions about visiting a doctor for a specific health condition.[34] Therefore, I defined an indicator variable that receives the value of 1 for those who reported having visited a physician for hypertension, heart attack, diabetes, stroke, asthma or cancer, and 0 if the individual did not visit a physician (irrelevant or missing). In this manner I constructed the Physician Visits Index in Section 6 – Placebo Analysis.

For the treatment variable – *employment* – I used "status at work" as an indicator variable in all Health Surveys. The indicator variable receives the value of 1 if status at work is salaried worker/employee, employer with 1–2 or 3+ workers, self-employed,

[32] Since households with more than seven persons were defined in one category (+7) in the 2000 survey, I followed this practice for the 2009 survey as well.

[33] As can be seen at the Israel Social Sciences Data Center (ISDC) site: http://geobase.huji.ac.il:8080/catalog/?dataset=0738

[34] For the 2009 survey, I asked, "In the past two weeks, has anyone in your household visited a doctor or consulted with a doctor over the phone (including house calls; not including dentists and telephone consultations for prescriptions)?" and for the 2000 survey I asked, "In the past two weeks, has anyone in your household visited a doctor (not including visits to dentists or orthodontists)?"

member of cooperative, member of kibbutz (collective), or unpaid family member, and 0 otherwise.

B. Healthcare Expenditure

In addition, I use the Household Expenditure Surveys for the years 1997–2011. I used responses to the question "How much did you spend on various health problems?" as another proxy for morbidity. Since the Household Expenditure Surveys express expenditure in household terms and not individual ones, I merged each individual with his household's expenditure.[35] Afterwards, I divided household expenditure by the number of members in the household[36] to obtain "average expenditure per member of household" (i.e., the same mean value for each member of the household). I kept only individuals who were defined as heads of household or spouses. (Since I excluded women, I obtained only one "owner" per household.) I defined 1997–2003 as the pre-reform years and 2007–2011 as post-reform. Ages 65–66 were defined as the treated group, and ages 25–75 (excluding 67–71) as the control group. I dropped the year 2000 from this part of the analysis because I could not merge it with each individual due to technical issues. Healthcare expenditure was aggregated by the following four items: health insurance (including all kinds of health insurance that an individual may hold), dental care, expenditure on healthcare services, and other health-related expenditure. Therefore, the Healthcare Expenditure Index that I constructed excluded dental care from the calculations. Thus, the Healthcare Expenditure Index is composed of health insurance, expenditure on healthcare, and other health-related expenditure. Since

[35] It was done by using the variable: "household number."
[36] It was done by using the variable: "number of individuals."

expenditure is reported in nominal prices, I deflated it by the ICBS Consumer Price Index into 2011 terms.[37]

I controlled for the following variables by means of estimations:

Marital status – an indicator variable that received the value of 1 for being married and 0 for bachelor, divorced, widowed, living separately, or unknown.

Bachelor's degree holders – an indicator variable that received the value of 1 if "Type of last school" was an academic institute, and 0 otherwise.

Israeli natives – an indicator variable that received the value of 1 if individual did not report a year of immigration and 0 otherwise.

For the treatment variable, employment, I used the variable of "status at work". The indicator variable received the value of 1 if status at work was salaried worker or self-employed and 0 otherwise.

C. SHARE

The last dataset I used is that of the Survey of Health, Ageing and Retirement in Europe (SHARE), a longitudinal dataset produced in two "waves" (Wave 1: 2005–2006, Wave 2: 2009–2010). Wave 1 contains 2,598 individuals, Wave 2 includes 2,464. SHARE presents a wide range of detailed individual-level information on health, socioeconomic status, and various life habits. I used the panel dimension of the SHARE data and included individuals who appeared in both waves. Individuals who reported different years and months of birth in the two waves were excluded from the analysis.

[37] According to the following formula: prices in 2011 terms = prices in current year $\cdot \frac{\text{CPI in 2011}}{\text{CPI in current year}}$
The average CPI index for 2010 was defined as the baseline. My calculations were based on the average CPI found at: http://www.cbs.gov.il/reader/prices_db/PricesDB.html

Furthermore, according to those who conducted the Israel-SHARE project, former Soviet Union immigrants did not answer properly the general questions on state of health. To maximize accuracy, I dropped those interviewed by a Russian-speaking enumerator. Therefore, my final dataset contained 582 men in each wave.

In this analysis, I used the following health variables:

Chronic problems: This variable counts the following diseases that were diagnosed by a doctor: A heart attack including myocardial infarction or coronary thrombosis or any other heart problem including congestive heart failure; high blood pressure or hypertension; high blood cholesterol; stroke or cerebral vascular disease; diabetes or high blood sugar; chronic lung disease such as chronic bronchitis or emphysema; asthma; Arthritis including osteoarthritis or rheumatism; osteoporosis; cancer or malignant tumour, including leukaemia or lymphoma, (but excluding minor skin cancers); stomach or duodenal ulcer, peptic ulcer; Parkinson's disease; Cataracts; and hip fracture or femoral fracture. Since additional variables – other fractures, Alzheimer's disease, and benign tumor – were added to the list of chronic problems in the second wave, I presented the results only when the same diseases were compared in both waves. However, similar results were obtained when the chronic variable was used as-is.

Limitation with activities: This variable is divided into two categories: limited or not limited.

Symptoms: This variable counts the symptoms of problems that bothered each individual in the past six months: pain in back, knees, hip, or other joint; heart trouble or angina, chest pain during exercise; breathlessness (difficulty breathing); persistent cough; swollen legs; sleeping problems; falling down; fear of falling down; dizziness, fainting,

or blackouts; Stomach or intestine problems, including constipation, air, Diarrhea; and incontinence or involuntary loss of urine. Since fatigue was added as a symptom in Wave 2, I presented the results when only the same diseases were compared in both waves. However, similar results were obtained when the symptom variable was used as-is.

Number of limitation with activities of daily living: This variable includes the following daily activities- dressing, including putting on shoes and socks; walking across a room; bathing or showering; eating such as cutting up food; getting in and out of bed; and using the toilet including getting up or down.

Number of limitations with instrumental activities of daily living: Using a map to figure out how to get around in a strange place; preparing a hot meal; shopping for groceries; making telephone calls; taking medications; doing work around the house or garden; and managing money such as paying bills and keeping track of expenses.

Hospitalization: whether an individual was hospitalized overnight in the past year.

Physical inactivity: never or almost never engaging in either moderate or vigorous physical activity.

Mobility problems: the number of limitations with mobility, arm function and fine motor function due to health problems which includes the following: walking 100 meters; sitting for about two hours; getting up from a chair after sitting for long periods; climbing several flights of stairs without resting; climbing one flight of stairs without resting; stooping, kneeling, or crouching; reaching or extending your arms above shoulder level; pulling or pushing large objects like a living room chair; lifting or carrying weights over 10 pounds/5 kilos like a heavy bag of groceries; and picking up a small coin from a table.

Grip strength: Two grip strength measurements on each hand were recorded with a dynamometer during the interview.

Depression index: This index was constructed by using the Euro-D scale, which counts the following mental problems: depression; pessimism; suicidality; guilt; sleep; interest; irritability; appetite; fatigue; concentration; enjoyment; and tearfulness.

Age: This variable was constructed by subtracting the year of birth from the year of survey.

In all the constructed variables, answers in the "refused" or "do not know" categories were ignored. Namely, each of the aforementioned state of health variables received the value of 0 if the individual did not suffer from the health problem and a positive number if the individual suffered from one or more health problems.

Appendix Tables

Appendix Table 1

Estimations Without Controls

Panel A. Health Outcome:	OLS	2SLS	Reduced From
Poor Health Index	-0.15*** (0.01)	1.05* (0.56)	0.16** (0.07)
Sample	All Ages	All Ages	All Ages
Observations	28,110	28,110	28,110
Panel B. Health Outcome:			
Physician Visits	-0.03*** (0.007)	0.49* (0.27)	0.07** (0.03)
Sample	All Ages	All Ages	All Ages
Observations	28,110	28,110	28,110

Panel C. Expenditures Surveys

	Health Expenditures			Dental Expenditures		
	Before Years 1997-2003	After Years 2007-2011	Diff	Before Years 1997-2003	After Years 2007-2011	Diff
Ages 67-68	217.437 (13.30) [541]	359.045 (25.48) [526]	141.6*** (28.74)	125.4 (18.20) [541]	109.34 (15.47) [526]	-16.05 (23.88)
Ages 72-75	308.24 (20.51) [923]	398.50 (17.17) [920]	90.25*** (26.75)	93.04 (9.89) [923]	98.8 (14.92) [920]	5.76 (17.91)

Notes: Panels A and B present 2SLS estimations as presented in the text when not controlling for additional covariates. All the regressions include age and year dummies. Robust standard errors are in parenthesis. Panel C presents t – tests for differences between means. * indicates significance at the 10% level. ** indicates significance at the 5% level. *** indicates significance at the 1% level.

Appendix Table 2 – First Stage Regressions - Health Care Expenditures

Dependent Variable	LFP	
	Without Controls	With Controls
Instrument	0.2*** (0.02)	0.2*** (0.02)
R - Squared	0.16	0.18
Observations	44,869	44,869
F- Test	56.51	57.38

Notes: This table presents First Stage Regressions. All the regressions include age and year dummies. Ages that were included are 25-75 (not including ages 67-71). Years included 1997-2003 and 2007-2011. The covariates are: marital status, education and immigration. Robust standard errors are in parenthesis. * indicates significance at the 10% level, ** indicates significance at the 5% level, *** - indicates significance at the 1% level.