Income Effects on Health: Evidence from Union Army Pensions

Shari Eli

To what extent do rising income levels explain the decline in adult mortality rates experienced in the United States a century ago? I explore this question by investigating the income effect of the country’s first wide-scale entitlement program: the Union Army pensions. Documenting that Republican Congressional candidates boosted pensions to secure votes, I exploit exogenous increases in income stemming from patronage politics to estimate the semi-elasticity of disease onset with respect to pensions. Income effects are large for cardiovascular, gastrointestinal, and respiratory illnesses.

At the end of the nineteenth and early twentieth centuries, crude death rates from infectious illnesses fell dramatically in the United States. Between 1900 and 1930, the crude death rate from tuberculosis dropped by 63 percent and that of influenza and pneumonia fell by 82 percent (U.S. Census Bureau 1960, p. 26). The substantial decreases in mortality from infectious illnesses were followed by large gains to life expectancy and decreases in death rates for all age groups. Between 1900 and 1925, the infant mortality rate fell by 53 percent, and the death rate for those aged 45 to 54 fell by 15 percent (U.S. Census Bureau 1960, p. 29). In this research, I investigate to what extent rising individual income levels explain these rapid and profound declines in morbidity and mortality experienced by the United States.

The literature on the U.S. mortality transition between 1870 and 1930 attributes the sharp decrease in death rates from infection illnesses to the emergence of public health interventions in the period (Cutler and The Journal of Economic History, Vol. 75, No. 2 (June 2015). © The Economic History Association. All rights reserved. doi: 10.1017/S0022050715000674

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Miller 2005; Ferrie and Troesken 2005)]. With the acceptance of the germ theory of disease during the 1880s and 1890s, macro public health initiatives to improve sanitation, construct sewer systems, and chlorinate or filter municipal water sources have been credited as the drivers of the large declines in mortality. The literature on the U.S. mortality transition has also focused on the great advances in medical science during the early twentieth century and its role in reducing morbidity and mortality. While there were few effective medical treatments in the late 1800s, vaccines, which were discovered in the 1920s, and sulfa drugs, which were used in the 1930s, have been shown to contribute to early twentieth century declines in mortality (Jayachandran, Lleras-Muney, and Smith 2010; McKinlay and McKinlay 1977).

Abstracting from public health interventions and medical advances, I focus on the effects of improvements in socioeconomic status stemming from increases in individual income and show how profoundly income effects contributed to the mortality decline experienced between 1893 and 1906 in the United States. I contribute to a wide literature of income effects both on health and on channels, which led to changes in health status. As shown by Dora Costa (1995), increased income in this period led to a rise in retirement rates and allowed individuals to exit hazardous work environments. Increases in income also allowed individuals to alter their living arrangements and reduce crowding (Costa 1997), which decreased prevalence rates of contagious illnesses. In addition, rising income led to improved nutrition levels: Thomas McKeown (1976) argued that improvements in nutrition explain the majority of the mortality decline before 1900 in the industrialized world. Similarly, Robert Fogel (2004) shows that improvements in socioeconomic status over time led to increases in net nutritional status, body size, and longevity. However, Fogel emphasizes that increases in socioeconomic status come not just in the form of increased wages but also in the form of medical care to the individual as well as public health interventions. Fogel, therefore, highlights the difficulty in disentangling the effect of income on health as opposed to the effects of public health interventions or advancements in medical science.

By estimating the effect of income on health status at a time when effective medical care for the individual is largely absent, and by

1 Newly discovered vaccines, which were available in the 1920s, included those for diphtheria, pertussis, tuberculosis, tetanus, and yellow fever.

2 With specific regard to declining rates of tuberculosis, McKeown (1976) asserts that the 80 percent decrease in the rate of tuberculosis, which transpired before the advent of any effective treatments for the illness, was likely the result of improved nutrition in the population.
finding an exogenous source of variation in income which does not operate on health status through public health interventions, I disentangle confounding effects to estimate the impact of income on health. I investigate the contribution of increases in individual income to the decrease in adult morbidity and mortality by examining the cohort of Union Army soldiers who enlisted in the American Civil War. There are several advantages to using the Union Army dataset to examine my research question. First, the data set includes demographic and socioeconomic information for veterans until death. Second, the data include detailed information regarding a veteran’s health that is not self-reported but rather recorded by surgeons employed by the Pension Bureau. Third, Union Army veterans received sizable and varying pension payments from the United States government that were documented from the time of a soldier’s enlistment until his death. Finally, Union Army veterans are a representative sample of Northern white men of their birth cohort (as described in the Data section).

One drawback of using the Union Army dataset in this analysis is that veterans received pensions for disabilities. Since I test whether increases in pension income caused a decrease in morbidity and mortality, using cases in which veterans receive higher pensions for new disabilities will bias estimates. To circumvent the problem of reverse causality, the case in which an individual’s health could influence his income, I use an exogenous source of variation in pension income: the presence of close elections for Republican Congressional candidates in the individual’s district. As I show, there is evidence that Republican Congressional candidates boosted the pension income of veterans in an effort to secure votes in elections. The exclusion restriction is the following: Republican Congressional candidates affected the health of Union Army veterans through changes in pension amounts and not through another channel.3 By providing quantitative and qualitative evidence, this analysis highlights the pervasiveness of patronage politics in the nineteenth century United States and shows how it helped shape the largest entitlement program the country had ever seen, one which absorbed 40 percent of the federal budget by 1890.

I use a proportional hazard model to estimate the effect of monthly pension income on the probability of contracting or dying from a set of illnesses. Estimating income semi-elasticities and proportional hazards for a variety of diseases, I find that the onset of respiratory, cardiovascular,

3 In the section titled Robustness Checks, several tests are presented to validate the exclusion restriction.
and digestive conditions are significantly responsive to income. In particular, I find that an extra dollar of monthly pension income, which is equivalent to 4 percent of a farm laborer’s monthly earnings in 1900 (Preston and Haines 1991, p. 212–20) or a 9 percent increase in the average veteran’s monthly pension income, decreases the likelihood of developing a cardiovascular illness by an average of 25 percent, reducing the prevalence rate from 49 per 1,000 to 37 per 1,000.

When exploring the mechanisms by which pension income operates on health, I find that augmented pensions caused veterans to increase the number of children present in the household, which has differential effects on health depending on occupation of the veteran. The evidence suggests that the health of farmers improved most, relative to those in other occupations, because their children likely made positive contributions both within the household and on the farm. Conversely, an increasing number of children in the household worsened the health of manual laborers, most likely because these children did not contribute positively to the household. Instead, the children of manual laborers likely just contributed to crowding, which resulted in increased morbidity and mortality from contagious conditions. With regard to an increase in caloric intake, which has been highlighted in the literature as a main channel through which income affects health, I do not find evidence that veterans used increased pension income to improve their health through increased caloric intake. Finally, the estimates presented in this analysis are conditional on whether the veteran lived in an urban or rural setting and thus the effects found are not the result of differences in the urban/rural disease environment.

THE UNION ARMY PENSION IN THE PATRONAGE ERA

The U.S. political climate at the end of the nineteenth century was defined by protectionist tariffs, patronage politics, and, with the exception of the Cleveland administrations, Republican domination of the White House. In nearly every election cycle, Republicans and Democrats fought over how to spend the budget surplus, which resulted from the high tariffs, and the proper way of reforming the patronage system. In this political environment, the Union Army pension system, initiated in 1862 to provide assistance to veterans suffering from war-related disabilities, evolved into a large-scale entitlement program. When campaigning,
Republicans advocated spending the budget surplus on augmenting the Union Army pensions. As benefits increased, the budget surplus quickly became a deficit. Therefore, the Republican strategy to boost votes led to a budget deficit and thus the loss of a Republican majority in Congress in 1891.

**PASSAGE OF THE GENERAL LAW OF 1862 AND INVALID PENSIONS ACT OF 1890**

During the Civil War, 2.7 million men enlisted, and 87 percent of recruits survived the war (Dyer 1959). These veterans could qualify for public assistance. Under the General Law passed on 14 July 1862, veterans became eligible for a pension if their physical disabilities were shown to result from the war experience. Therefore, the General Law excluded veterans whose disabilities resulted from aging or any incidents unrelated to the war. Disabilities pensioned under the General Law included loss of sight, hearing, limbs or their usage, and illnesses caught while in the camps. To determine whether a causal link existed between illnesses/injuries and war experience, veterans were required to undergo medical examinations by physicians, called examining surgeons, hired by the Pension Bureau. Upon completion of each exam, examining surgeons filled out a surgeons’ certificate, which was then sent to the Pension Bureau and added to the veteran’s application for pensions.

Under the Invalid Pensions Act of 27 June 1890, which awarded pensions to veterans with disabilities from causes other than the war, previously excluded veterans were finally able to claim pensions on the basis of disability status (Glasson 1918, p. 126). The passage of the Invalid Pensions Act of 1890 enabled thousands of veterans to begin receiving pensions causing the federal government to go into deficit. By 1892, expenditure on veterans’ compensation and pensions accounted for approximately 40 percent of the federal budget (U.S. Census Bureau 1960, p. 718).

In 1907, the pension system changed once again, by granting payouts based on the veteran’s age. Pension payments were substantial for the time and large enough to allow veterans to retire (Costa 1995). In 1900, the average pensioner received $10.02 per month, which was approximately one-half of the average monthly income of a farm laborer (Preston and Haines 1991, pp. 212–20). By 1890, the Union Army pension was comparable to today’s Social Security Program in that approximately 42 percent of income is replaced by the Social Security Administration upon retirement.
During the 1870s and 1880s, many Republican candidates began boosting pensions awarded to veterans in their constituency in an effort to secure the veteran vote. Evidence of this practice is abundant. Perhaps the best set of evidence regarding Republican practices at the Pension Bureau comes from the testimony and evidence given at the 48th Congress’ session of the Committee on the Payment of Pensions, Bounty and Back Pay, in which the adjudication of pensions was investigated.\textsuperscript{5} The Committee Report, which contains more than 400 pages of testimony of the Commissioners, special examiners, clerks, and medical reviewers of the Pension Bureau between 12 January 1885, and 27 February 1885, provides detailed information on the process by which pensions were boosted during election seasons.

To secure the votes of Union Army veterans, Republican Congressional candidates wrote letters to the Commissioner of the Pension Bureau on behalf of constituents who had previously applied for pensions. For cases in which a veteran was still waiting for his application to be processed by the Bureau, the candidate asked that the Commissioner speedily review the case and send special examiners to the district if there were remaining questions about a veteran’s claim. For veterans already receiving pensions and having applied for an increase, candidates requested that the Commissioner either increase the pensions or that he similarly send a special examiner to the district to reassess the case. In their letters, Republican candidates frequently were found expressly stating that they were interested in boosting pensions in order to win the upcoming election.\textsuperscript{6}

\textsuperscript{5} The Committee met during the second half of Republican President Chester Arthur’s term (lame duck session) during which Congress was controlled by the Democrats. The split of power between the parties allowed for an investigation of Republican practices by Democrats. Additional qualitative evidence on the process by which pensions were boosted is provided in the online Appendix at http://individual.utoronto.ca/shari_eli/research.html in the section Patronage Practices at the Pension Bureau.

\textsuperscript{6} Letters from Congressmen to the Commissioner of the Pension Bureau can be found in Republican Abuses at the Pension Bureau. For example, on 4 August 1884, the Republican Congressman H. L. Morey from Ohio wrote the following to Commissioner Dudley: “My dear sir: I am just entering upon my canvass for Congress[...] Mr. H. H. Wallace, special examiner, just sent to Springfield, Illinois, is one of the best ‘button-holers.’ If he could be adjacent to Butler and Preble counties, he would be of the very greatest assistance to me. [...] He is thoroughly well acquainted in Butler and Preble and can do me great good [...] He does not know of me writing this letter, and I write it solely in the interest of my canvass and of a Republican House of Representatives” (p. 82). Congressman Morey’s request was granted as H. H. Wallace is listed as one of the special examiners in the “List of Special Examiners on Leave in Ohio During October and November, 1884” (\textit{House Committee Report} 1885, Part II, p. 134).
The Commissioner, who hired mostly Republican clerks at the Bureau, gave priority to the processing of the claims of veterans in the Republican candidate’s district, especially during election seasons. Clerks were not in charge of deciding the amount of pension paid to veterans. Instead, the Medical Reviewers of the Pension Bureau decided the dollar amount (United States Pension Bureau 1882, p. 9). Clerks had the power to stall the application of veterans during election seasons to prevent the loss of votes to Democrats. Clerks also had the power to reject an application but were advised not to do so during election seasons. Therefore, the Pension Office remained a Republican political machine used to secure the veteran vote throughout the patronage era.

Democratic candidates for Congress were unable to boost the pension payments of their veteran constituents because of the partisan nature of the Bureau during this period. Additionally, there is evidence that veterans supporting Democratic candidates were threatened with the loss of their pension or with having their applications stalled indefinitely by special examiners sent from Washington into districts during election season to aid the Republican cause.

The partisan practices of the Pension Bureau continued through the late 1880s and into the 1890s. During the election of 1888, Civil War veteran and New York commander of the Grand Army of the Republic James

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7 In 1886, for example, there were 1,472 Republican clerks employed by the Pension Bureau as opposed to only 28 Democratic clerks (Campaign Book 1888, p. 7).
8 In 1885, during the session of the House Committee on the Payment of Pensions, Bounty and Back Pay (H.D. 2683, 48th Congress, 2d Session), several clerks employed by the Pension Office were sworn to testify about the ways in which pensions were adjudicated. In the testimony of these clerks, each one says that he was informed by the Commissioner not to reject any cases whatsoever before the election of 1884 for “political purposes.” One clerk in particular testified that when he asked the assistant-chief (his superior at the Bureau) about rejecting a specific case, he was told “No no no; we are not rejecting any cases now. If you reject that case it will lose us six votes” (House Committee Report of Pensions Bounty and Back Pay, p. 26 of Part II).
9 There are numerous examples beginning on page 49 of The Campaign Book of the Democratic Party, 1886: “John M. Mattingly, being first sworn, deposes as follow: […] I have resided in Olney, Ills., for the past four years; I am forty-two years of age, and a laborer by occupation. […] my claim has been pending since the year 1879. During the political contest of 1884, […] my case was in the hands of Special Examiner Epert for the purpose of examination. When he came to me to take my preliminary statement, he asked me my politics; when I told him I was a Democrat, he told me I was on the wrong side to ever get a pension: that I never would get a pension as long as I voted the Democratic ticket, but that if I voted the Republican ticket my claim for pension would be allowed” (p. 50).

Other veterans testified before the House Committee in 1885 that they were told that they would not get their pension if they voted the Democratic ticket. In particular, veterans George Starkey and George Forshey of Ohio swore that they were coerced to vote the Republican ticket in order to receive a pension. (House Committee Report on the Payment of Pensions Bounty and Back Pay, p. 6 and 8 of Part II.)
Tanner “had, by his own account, ‘plastered Indiana with promises’ of more generous pensions under the Republicans. ‘God help the surplus revenue!’ the new commissioner declared, as he set about handing out new and readjusted pensions with gusto.”

**DATA**

I study these issues using the Union Army data set, which consists of information about the military experience, post-war health conditions, and socioeconomic status of veterans (uadata.org). The dataset is compiled from three sources: the military, pension and medical records; surgeons’ certificates; and census records. The military records contain demographic and socioeconomic information about the soldier at the time of his enlistment. The pension and medical records provide detailed information regarding the veterans’ experiences with the Union Army pension.

Since two-thirds of white men between the ages of 20 and 45 served in the Union Army, more than two million men who served were representative of the general population of white men in the North (Costa and Kahn 2008, pp. 2–3). Of the men who served in the Union Army, records for 35,570 white men were compiled. Regiments in the Union Army were comprised of ten companies. The Fogel group compiled information for men within 303 companies selected at random.

Figure 1 shows the total number of exams undergone by veterans applying for new pensions and pension increases between the years 1865 and 1935. Since veterans underwent many physical examinations, either

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10 It is unclear whether Republican presidents used the Pension Bureau to maintain Republican power in the White House and Congress or whether Congress was primarily acting alone. Evidence on the practices of the Union Army Pension Bureau suggests that some presidents did make appointments at the Pension Bureau to ensure Republican control. For example, once Republican President Harrison took office, Tanner was appointed as Commissioner of the Pension Bureau (Skocpol 1992, p. 128). In addition, testimony before Congress from pension applicants shows that they were coaxed to vote the Republican ticket, which included presidential, congressional candidates, and state candidates. See James (2006) for an analysis of patronage politics in the era.

11 Black recruits were not included in this research for several reasons: (1) black recruits were not representative of the general population. Instead, they were likely to be ex-slaves who were runaways or taken as contraband by the Union Army. (2) Information on their morbidity conditions is sparse and often unusable. This is because examining surgeons often underreported the illnesses of black veterans (see Wilson 2007). (3) Black veterans primarily lived in border and southern states, which passed laws to disenfranchise blacks. Therefore, the exogenous source of variation in pension income—Congressional vote share in a pensioner’s district—is not applicable. In addition, close elections do not predict changes in pension amount (the coefficient on pension amount is not statistically significant). See Eli and Logan (2014) for further analysis.
to apply for the first time or to apply for an increase, health information is available over time. One applicant could undergo as many as 30 exams. Of those on the pension rolls by 1893, each veteran had an average of 5.45 exams. The average exam year for all veterans in the sample was 1892 (the median was 1891), coinciding with the passing of the 1890 Act, which awarded pensions for disabilities. Given the dramatic spike in the number of exams shortly after 1890, it is unlikely that over 4,000 veterans suddenly contracted pensionable illnesses between 1890 and 1892. Instead, veterans were most likely reporting illnesses they faced during the 1870s and 1880s, which were not pensionable under the Law of 1862. Because veterans underreported non-war-related illnesses during the 1870s and 1880s, I focus on the sub-sample of those who were pensioned by 1893. When constructing the dependent variables of disease onset, I use information on illnesses reported prior to 1893 as a control but not as an outcome variable of disease onset because these illnesses were likely to be reported years after the veteran faced initial symptoms. Disease conditions for the sample are sometimes the result of the war experience and sometimes the result of aging. I choose the year 1893, as opposed to 1891 or 1892, as the starting year so that the dataset includes nearly all eligible veterans and not just the set of veterans who managed to get onto the rolls right after the passage of the 1890 law. I
also restrict the subsample to those who were admitted onto the pension rolls.\footnote{Date of death and causes of death information is largely unavailable for veterans who were not on the pension rolls. Including veterans who did not collect pensions in the sample would bias estimates. This is because there is no disease information for veterans who were not on the pension rolls. If veterans who did not collect pensions died before having the opportunity to claim pensions, their inclusion in the sample will bias estimates. Therefore, I restrict the sample to veterans who applied by the baseline year 1893 and who were admitted onto the pension rolls by that year.}

Table 1 shows summary statistics for the veterans who were on the pension rolls by 1873 and for those who were on the rolls by 1893 to highlight the way in which changes in pension laws affected the type of veteran who became a pension recipient in the late nineteenth century. For those on the rolls in 1873, 4.4 percent suffered from respiratory conditions, 6.91 percent suffered from digestive conditions, and 3.43 percent suffered from a cardiovascular condition in 1873. For those on the rolls in 1893, 7.22 percent suffered from a respiratory condition, 24.75 percent suffered from digestive conditions, and 30 percent suffered from cardiovascular conditions. Veterans of both the 1873- and 1893-sample were likely to be farmers or retired. Few veterans were professionals of type 1 or 2.\footnote{The Professional (1) category includes manufacturers, educators, attorneys, and other professionals. The Professional (2) category includes clerks, merchants, and salesmen.} Veterans from both samples come largely from New York, Pennsylvania and the Midwest. Table 2 presents probit regressions in which I use wartime indicators to predict the likelihood that a veteran would be in the 1873- or 1893-sample in the baseline year. Veterans were 20.7 percent more likely to be on the pension rolls by 1873 if they were wounded or had a wartime illness. Of veterans in the 1873-sample, 76.51 percent were wounded or suffered a wartime illness as compared with only 34.12 percent in the 1893 sample. From Table 2, it is clear that veterans on the pension rolls by 1893 were on average two years younger than those on the rolls by 1873.

In order to determine the Republican Congressional vote share of each veteran’s district, it was first necessary to determine the district of residence for veterans in each year. Since the Union Army sample contains the state and city of residence as well as the exact street address of veterans from first pension receipt until death, I matched each veteran to his associated Congressional district between 1870 and 1910.\footnote{If a veteran moved to a new location in a given year, he appears in the data as being in the destination district for that year.} Since district boundaries change rapidly during this period, veterans had to be rematched to districts to account for border changes. Even during years in which a veteran remained at the same residence, he could still be counted as being in a new district if boundaries were redrawn.
To account for changes in district lines, I matched each veteran’s address to district maps for census years 1870 to 1910.\footnote{District boundaries change after each census year. Therefore, district boundaries from 1870 to 1879 can be determined by looking at the boundaries of the 1870 census. District boundaries for the maps were found in Kenneth Martis’ \textit{Historical Atlas of the United States Congressional Districts: 1789–1983}.} Having matched veterans to their district in each year, I then collected election returns for each district in the United States between the years 1870 and
1910. It was necessary to collect this new dataset of Congressional Election returns by district and to create new district maps, as opposed to using the Inter-university Consortium for Political and Social Research (ICPSR) Historical Election Returns Series and Geographic Information System (GIS) county maps from the National Historical Geographic Information System’s (NHGIS) Historical state and county boundary files (1790–2000), for two reasons. First, while the ICPSR data set links counties to districts, it is not possible to be certain of a veteran’s district simply by determining his county. This is because many counties were split into multiple districts. Therefore, matching a veteran’s address to a county, using GIS county maps, and linking the county to an associated

\[ \text{Income Effects on Health} \]

**Table 2**

PROBIT REGRESSION—PREDICTING VETERAN’S ENTRY ONTO PENSION ROLLS

<table>
<thead>
<tr>
<th>Date of Entry onto the Pension Rolls</th>
<th>1873</th>
<th>1893</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Coefficient (% points)</td>
<td>Mean (%)</td>
</tr>
<tr>
<td>War injury /illness</td>
<td>.207*** (37.00)</td>
<td>76.51</td>
</tr>
<tr>
<td>Private</td>
<td>.007 (0.18)</td>
<td>71.82</td>
</tr>
<tr>
<td>Sergeant</td>
<td>.014 (0.34)</td>
<td>10.29</td>
</tr>
<tr>
<td>Lieutenant</td>
<td>.015 (0.34)</td>
<td>2.29</td>
</tr>
<tr>
<td>Musician</td>
<td>.003 (0.08)</td>
<td>.97</td>
</tr>
<tr>
<td>Birth year</td>
<td>–.003*** (–9.88)</td>
<td>1836</td>
</tr>
<tr>
<td>Number of veterans on rolls</td>
<td>1,750</td>
<td></td>
</tr>
<tr>
<td>Total number of veterans</td>
<td>16,013</td>
<td></td>
</tr>
</tbody>
</table>

Notes: t statistics in parentheses. *p < 0.05, **p < 0.01, ***p < 0.001.

Source: Author’s calculations.
district, using the linkages provided in the ICPSR’s Historical Election Returns, 1824–1968, is an inaccurate approach.\footnote{To access the ICPSR data, see the following source: Haines, Michael R., and Inter-university Consortium for Political and Social Research. Historical, Demographic, Economic, and Social Data: The United States, 1790-2002. ICPSR02896-v3. Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor], 2010-05-21. http://doi.org/10.3886/ICPSR02896.v3}

Second, the ICPSR series contains vote totals by \textit{county} as opposed to district. While counties are matched to districts in this series, it is not possible to compute the Republican vote share for a district by summing Republican votes over the counties in a district (and dividing by total votes) because one county could lie in \textit{two} different districts. Therefore, I collected a new series of election returns by district that are linked to a new set of GIS maps with historical Congressional district boundaries.

In the analysis, I use county-level data from the ICPSR’s Historical Demographic, Economic and Social Data: The United States, 1790–2002. In particular, I use farm values per acre as a proxy for changing wealth levels in the county. Information on the population size of U.S. cities is from the U.S. Census Bureau.\footnote{The U.S. Census Bureau’s Population of the 100 Largest Cities and Other Places in the United States: 1790 to 1990 is available at http://www.census.gov/population/www/}

\textbf{IDENTIFICATION}

Under the pension acts prior to 1907, veterans could only receive pensions based on health status. Because of this fact, pension income is endogenous to health outcomes. Since veterans receiving larger pensions were likely to be in worse health and likely to have continually deteriorating health, relative to those who received smaller pensions, it is difficult to identify the true effects of income on health without finding an exogenous source of variation in pension income. In addition, it is likely that estimates of the effect of pension income on health will be biased downward causing it to seem as though pensions increase the chance of sickness in veterans. However, in actuality, it may simply be that poor health status initially has overshadowed the effects of income. While controlling for prior health status partly controls for this effect, it is preferable to use an exogenous source of variation in pension income. I use variation in Republican Congressional vote share.

Figure 2 shows graphs of the relationship between the average monthly pension amount and the Republican Congressional vote share in a district both with and without 95 percent confidence intervals. In particular, it
Figure 2

DISTRICT VOTE SHARE AND PENSIONS FOR ELECTION YEARS 1894–1906

Source: Author’s calculations.
is clear that as the Republican Congressional vote share increases, the average monthly pension amount awarded to veterans first increases and then usually tapers off once the vote share nears 40 percent. Since third party candidates sometimes absorbed nearly 10 percent of the vote share in a Congressional election, a Republican vote share of 40 percent would represent a close race for a Republican candidate. While pension amounts are maximized when Republican vote share is above 40 percent, veterans living in staunchly Republican districts with Republican vote shares of more than 60 percent received more pension income than their counterparts living in staunchly Democrat districts. Therefore, it appears that the highest pension awards went to those living in close districts, lower awards went to those in staunchly Republican districts, and the lowest awards went to those in Democratic districts. I construct a panel over individuals and years. Since Congressional elections are bi-annual (with the exception of special elections), the Republican vote share in non-election year \( t \) is equal to the Republican vote share in election year \( t - 1 \). To capture the upward-trending curve seen in Republican vote share, I use the following first-stage specification in which the Republican vote share variable enters quadratically:

\[
p_{it} = \gamma_0 + \gamma_1 v_{it} + \gamma_2 v_{it}^2 + \gamma_3 v_{i,t-1} + \gamma_4 v_{i,t-1}^2 + \Gamma C_{it} + e_{it},
\]

where \( p \) = monthly pension income in 1907 dollars, \( v \) = Republican congressional vote share, \( i \) = individual, \( t \) = year, \( C \) = set of demographic and socioeconomic controls, and \( e \) is the error term. Controls include rank while in the army, birth year, time trend, county population, average farmland value per acre, prior health status, prisoner-of-war status, battle wounds, population of the county, and state fixed effects. Each of these can affect income. High-ranking veterans received higher pensions than their lower-ranked counterparts. It is important to control for birth year because incidence of disease increases with age. County-level average farmland values per acre are used to control for increasing wealth in the county. County-level population counts (per 100,000), which are only available for census years, were linearly interpolated for years between census years.\(^{19}\) Controlling for population size of the veteran’s county and including state fixed effects is necessary because the ability to access the Pension Bureau and attain a

\(^{19}\) The data for county-level population size comes from Michael Haines’ ICPSR dataset entitled, “Historical, Demographic, Economic, and Social Data: The United States, 1790–2002.”
pension could vary between rural and urban counties as well as across states.\textsuperscript{20}

For the purpose of hypothesis testing, I cluster the standard errors by district and year in the first stage. This is because the unit of observation is the individual in a particular year. However, the vote share variables are the same within a district-year. Therefore, they are perfectly correlated across all individuals within a district-year.

HAZARD MODEL USING CONTROL FUNCTION APPROACH

A seemingly simple method to estimate elasticities of income on health in this setting would be to use a two-stage least squares approach. However, this approach will lead to biased estimates because 33.5 percent of veterans who are on the rolls by 1893 died prior to 1906.\textsuperscript{21} If a two-stage least squares approach is used, results will be biased to reflect the disease and mortality experience of veterans who live the longest and are thus observed for more periods in the sample.

Therefore, to estimate semi-elasticities, I use a Weibull proportional hazard model to predict the effect of an extra dollar of monthly pension income on the probability of mortality (or disease onset). Using specific disease outcomes in the analysis, I group the morbidity and mortality events (failures) into one of the following six categories: respiratory, cardiovascular, digestive, endocrine, genito-urinary, and hernia.\textsuperscript{22} Therefore, I estimate the probability of failure $f$, when multiple causes of failure are possible, by treating failures other than $f$ as censored cases (by right-censoring on death). An assumption of the model is that competing risks are independent.\textsuperscript{23}

\textsuperscript{20} One reason why veterans in rural areas may have had difficulty attaining a higher pension award relative to their urban counterparts is because they may not have had access to pension attorneys, who would aid them in the pension application process, or because they may have found it cumbersome to travel long distances to visit examining surgeons.

\textsuperscript{21} In 1907, the Pension Bureau awards pensions based on age primarily. Because of this, few veterans undergo exams after 1906 and so there is no new information on health status. Therefore, I restrict the sample to observations prior to the year 1907.

\textsuperscript{22} Respiratory illnesses include abscess, allergy, asthma, atelectasis, bronchiectasis, bronchitis, cavity, edema, emphysema, fibrosis, hemoptysis, pleuritis, pneumonia, pneumonitis, pneumothorax, tracheitis, and tuberculosis. Digestive illnesses include constipation, diarrhea, dysentery, dyspepsia, dysphagia, enlarged liver, gallstones, gastroenteritis, malassimilation (malabsorption), and nausea. Cardiovascular illnesses include arteriosclerosis, cyanosis, dyspnea, enlarged heart, impaired circulation, murmur, cardiac edema, and palpitation. Endocrine illnesses include diabetes, goiter, and enlarged spleen. Genito-urinary illnesses include cystitis, enlarged prostate, nephritis, urethral obstruction, and uremia.

\textsuperscript{23} Note: When failure is the onset of health condition $f$ (a morbidity, not mortality, condition) then other morbidity conditions (some of which may have onset during the time interval of study) are included as covariates. Thus, the onset of conditions other than $f$ is not considered a competing risk but instead a covariate.
Because the model is non-linear, I choose not to use instrumental variables. Instead, I use the control function approach in which the monthly pension income, control variables, and the residual from the first stage is included in the hazard regression.

In the second-stage, I choose
\[ \theta = \{ \beta, \alpha, \phi \} \]
to maximize the likelihood function
\[
\sum_{i=1}^{N} \sum_{b=1893}^{1906} \{ a_i \log(e^{k_i \beta} \alpha^{t_{i-1}}) + (1 - a_i)(-e^{k_i \beta}) \},
\]
where
\[
k_i \beta = \beta_j p_{it} + \sum_{j=1}^{N} \beta_j C_{jt} + \phi_j \hat{u}_{it}.
\]

The term \( a_i \) is an indicator variable equal to one if the veteran is alive and 0 if dead or if \( t = 1907 \). Veterans who have died do not contribute to the likelihood after their death although the hazard model treats them as at risk for the illnesses that they have not yet had.

Since the model is a proportional hazard model, it is possible to interpret the effects of a change in income on the hazard. A negative coefficient indicates that the time until failure increases as income increases, which is equivalent to a negative effect on the hazard or the probability of disease onset.

The standard errors of the second stage do not take account of the fact that the residual from the first stage, which enters in the second stage, is estimated with error. Because of the difficulty in making the usual correction for this in a non-linear setting, I bootstrap the standard errors. However, bootstrapping is only necessary if the coefficient on the residual is significantly different from zero in the second stage (Imbens and Wooldridge 2007, p. 9).

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24 Since the second-stage is non-linear, \( E(\|v\|) \) must equal zero instead of \( E(\|u*\|) = 0 \), which is the weaker assumption necessary for two-stage least squares. Combined with the assumption that the first-stage is linear (necessary for the Control Function [CF] approach), the CF approach is more efficient than two-stage least squares. More importantly, two-stage least squares are inconsistent when the model is not linear in the parameters. For a more detailed explanation of the Control Function versus Instrumental Variables Approach, see Imbens and Wooldridge (2007).

25 Conditional on covariates, the true time to failure is independent from the censoring event. Because I code death due to a particular disease as a morbidity, this should be valid.
RESULTS

First-stage results are presented in Table 3. The magnitude of the coefficients on Republican vote share and the square of Republican vote share are similar. This suggests that pension income is maximized when elections are close. When the controls are included, an increase in the Congressional Republican vote share of a veteran’s district from 0 percent to 48 percent (the maximum) is associated with an approximately $0.75 increase in monthly pension income. The coefficients on Republican vote share in period \( t - 1 \) and Republican vote share squared in period \( t - 1 \) suggest that living in previously close election districts are rewarded by pension boosts in the current period.\(^{26}\)

Table 4 shows hazard regression estimates of disease onset, which is separated into anatomical groups. Regressions with and without the control function are presented as well as the prevalence rate for each disease condition, which is equivalent to the sample mean of the morbidity indicator. Table 4 shows that respiratory conditions are income sensitive using the control function approach to correct for reverse causality. Correcting for reverse causality, an extra dollar of monthly pension income, which was usually a permanent increase, decreases the probability of the onset of a respiratory condition by 63.2 percent, which is equivalent to a reduction in the prevalence rate of 6.1/1000. Of veterans with a respiratory condition, 32 percent had asthma, and 13 percent had bronchitis. As shown in Table 4, an extra dollar of monthly pension income lowers the onset of asthma by 27.3 percent and bronchitis by 40.5 percent. Both of these disease conditions are usually chronic and brought on by infection and/or poor air quality associated with crowding.

Table 4 also shows effects for the onset of other disease conditions. In particular, an extra dollar of monthly pension income causes a 32.1 percent decline in the prevalence rate of the onset of any digestive condition. As with respiratory illnesses, digestive illnesses in this period were often the result of infection.

With regard to cardiovascular illnesses, an extra dollar of pension income decreases the likelihood of the onset of a cardiovascular illness by 24.9 percent a decrease of 12.2 per 1,000 in the prevalence rate. Cardiovascular conditions such as arteriosclerosis and enlarged heart are

\(^{26}\) The first stage takes on a quadratic functional form in that vote share \((v)\) and the square of vote share \(v^2\) are included in the regression. Then, the first-stage would have the following form: \(p = a_1v + a_2v^2 + K\) where \(p\) is pension income and \(K\) is all other variables held constant. In order to maximize pension, (taking the derivative), \(v = -0.5(a_1/a_2)\). Therefore, pension amount is maximized when vote share is \(-0.5*(3.16/-3.32) = 0.48.\)
conditions associated with affluence and seen in high rates in developed nations. However, individuals who suffered from acute infections, such as rheumatic fever, could sustain damage to their heart valves. Therefore, inasmuch as income could ward off infectious illnesses, it could also prevent the likelihood of heart disease resulting from acute infections.

Table 5 shows the effects of income on the crude death rate (any-cause mortality) and on cause of death. I find that an extra dollar of pension
Income Effects on Health

Table 4
HAZARD REGRESSION ESTIMATES OF THE ONSET OF DISEASE

<table>
<thead>
<tr>
<th></th>
<th>% Decline in Hazard Due to $1 Increase</th>
<th>Prevalence Rate</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>= (1 – exp(β)) * 100</td>
<td>(Sample Mean)</td>
</tr>
<tr>
<td>Cardiovascular</td>
<td>1.88*** (0.003)</td>
<td>24.87*** (.048)</td>
</tr>
<tr>
<td>(N=7,653)</td>
<td></td>
<td>0.049</td>
</tr>
<tr>
<td>Arteriosclerosis</td>
<td>–0.20 (.004)</td>
<td>16.97* (.107)</td>
</tr>
<tr>
<td>(N=12,216)</td>
<td></td>
<td>0.06</td>
</tr>
<tr>
<td>Digestive</td>
<td>2.64*** (.004)</td>
<td>32.09*** (.057)</td>
</tr>
<tr>
<td>(N=8,474)</td>
<td></td>
<td>0.15</td>
</tr>
<tr>
<td>Diarrhea</td>
<td>1.98*** (.006)</td>
<td>16.88*** (.111)</td>
</tr>
<tr>
<td>(N=9,292)</td>
<td></td>
<td>0.08</td>
</tr>
<tr>
<td>Endocrine</td>
<td>0.50 (.004)</td>
<td>24.11*** (.110)</td>
</tr>
<tr>
<td>(N=11,081)</td>
<td></td>
<td>0.08</td>
</tr>
<tr>
<td>Genito-urinary</td>
<td>2.08*** (.005)</td>
<td>20.78*** (.094)</td>
</tr>
<tr>
<td>(N=11,712)</td>
<td></td>
<td>0.09</td>
</tr>
<tr>
<td>Hernia</td>
<td>1.88*** (.006)</td>
<td>17.30*** (.063)</td>
</tr>
<tr>
<td>(N=11,075)</td>
<td></td>
<td>0.05</td>
</tr>
<tr>
<td>Respiratory</td>
<td>0.20 (.004)</td>
<td>63.18*** (.145)</td>
</tr>
<tr>
<td>(N=10,936)</td>
<td></td>
<td>0.09</td>
</tr>
<tr>
<td>Asthma</td>
<td>1.59 (.010)</td>
<td>27.31* (.121)</td>
</tr>
<tr>
<td>(N=12,095)</td>
<td></td>
<td>0.02</td>
</tr>
<tr>
<td>Bronchitis</td>
<td>0.20*** (.005)</td>
<td>40.54*** (.112)</td>
</tr>
<tr>
<td>(N=11,557)</td>
<td></td>
<td>0.05</td>
</tr>
</tbody>
</table>

Specification Hazard Regression Hazard with C.F. Approach

Note: Standard errors of coefficient in parentheses. * p < 0.05, ** p < 0.01, *** p < 0.001. Controls: County population, state dummies, time trend, first-stage residual, birth year, county-level average farm value per acre, rank in the army, prisoner-of-war status, war wounds, and previous illnesses. Clustered by year and district. Source: Author’s calculations.

Income lowers the unconditional hazard rate by 27.0 percent from 27 per 1,000 to 20 per 1,000. I also find large income effects for respiratory, digestive and cardiovascular illnesses. However, I do not find significant income effects for endocrine and genito-urinary conditions.

While the morbidity and cause-specific mortality results are informative, the most robust estimate is that of the coefficient on pension income when mortality from any cause is the dependent variable in the regression. This is because of the following reasons: (1) Most obviously, death from any cause is easy to determine; (2) Cause of death is often omitted
from death certificates in the period; (3) Specific cause of death can be difficult to ascertain if a physician is not present at the time of the veteran’s death; and (4) Even if a physician is present at the veteran’s time of death, it may still be difficult to distinguish the cause of death from contributing causes of death. For these reasons, I focus on death from any cause when considering the mechanisms by which income can affect health in the next section.

MECHANISM

Income changes can alter an individual’s standard of living and thus their health by allowing individuals to improve their nutritional status. Fogel (2004) emphasizes the relationship between body mass index (BMI) and mortality. Individuals have benefitted from increased control over their environment (food production, in particular), have become more physically robust, and have developed progressively stronger vital organs over the past 300 years. To explore whether changes in BMI may

| TABLE 5 | HAZARD REGRESSION ESTIMATES FOR MORTALITY |
|------------------|------------------|------------------|
| % Decline in Hazard Due to $1 Increase | Unconditional Hazard Rate |
| $= (1 – \exp(\beta)) \times 100$ | (Sample Mean) |
| Any-cause mortality | -1.36*** | 27.02*** | .027 |
| | (.001) | (.048) | |
| Cardiovascular | -1.11 *** | 25.70*** | .013 |
| | (.001) | (.074) | |
| Digestive | -1.61** | 55.11*** | .0004 |
| | (.007) | (.269) | |
| Endocrine | -2.60*** | 31.54 | .001 |
| | (.009) | (.551) | |
| Genito-urinary | -0.30 | 9.24 | .001 |
| | (.013) | (.394) | |
| Respiratory | -1.51*** | 36.36*** | .002 |
| | (.003) | (.141) | |

Note: Standard errors of coefficient in (). Number of vets: 12,091. * p < 0.05, ** p < 0.01, *** p < 0.001. Controls: County population, state dummies, time trend, first-stage residual, birth year, county-level population average farm value per acre, rank in the army, prisoner-of-war status, war wounds, and previous illnesses. Clustered by year and district. Source: Author’s calculations.
be driving the results of this research, I use the same first-stage specification outlined previously but instead replace monthly pension income with BMI as the dependent variable and include individual fixed effects.\textsuperscript{27} The coefficient on vote share was not significant, which suggests that changes in net caloric intake amongst veterans varied independently of vote share. Since the average BMI in the sample is 22.7 in 1893, and since less than 5 percent of veterans had a BMI below 18.5, veterans were likely not suffering from calorie deprivation. It may be however that increased income operated on health by improved \textit{quality} of food consumed as opposed to the quantity measured in calories.

Other possible ways in which pension income may have operated on health is through changes in living arrangements and by increasing the likelihood of retirement as Costa has shown (Costa 1997; Costa 1995). To determine whether living arrangements might be a mechanism by which income affects health, I first consider whether increases in pension income cause the number of children living in the household to increase in 1900. Instrumenting for pension income, I find that an extra dollar of monthly pension income causes the number of children in the household to increase by approximately 0.1 children.\textsuperscript{28} Then, I explore the differential effects of increases in the number of children in the household on health outcomes by veteran’s occupation type.\textsuperscript{29}

Table 6 shows the coefficient estimates on pension income when the sample is split by occupation type and when household composition controls are included. The two controls for household composition are the following: (1) the ratio of the number of children under age six in the household relative to the total number of people in the household. This is a measure of dependents to workers in the household; (2) Dummy variables for the number of individuals in the household ranging from 1, 2, …14, and 15 or more. Table 6 shows that when I control for household composition, the percent decline in the hazard shifts from 25.4 percent to 23.2 percent for farmers, which suggests that pension income operates on health through the increase in the number of children in the household. In

\textsuperscript{27} Information on the height and weight of veterans is contained in surgeons’ certificates. BMI was computed using the heights and weights on these certificates. BMI was imputed for missing years, which are years when the veteran did not undergo an exam.

\textsuperscript{28} More specifically, I regress pension income on Republican vote share in 1900, the square of Republican vote share in 1900, birth year, farm value, Republican vote share in 1898, the square of Republican vote share in 1898, indicator variables for previous illnesses, county-level population, rank in the army, prisoner-of-war status, and war wounds. In the second stage, I regress the number of children in the household on the fitted values of pensions.

\textsuperscript{29} When I control for occupation type using the same econometric specifications used to produce the estimates in Tables 4 and 5, there is no change in the estimates. Therefore, pension income does not operate on health through occupational choice.
the case of farmers, increasing the number of children in the household may be beneficial if children can provide labor on the farm or in the household itself. Conversely, when considering manual laborers, controlling for household composition increases the coefficient on pension income, which suggests that increasing the number of children in the household hurts the health of manual laborers. This perhaps may be because children cannot contribute to the household and instead only drain resources. However, given data limitations, it is not possible to directly observe the contributions of children to the households of farmers or manual laborers.

ROBUSTNESS CHECK—MIGRATION

The results of this research hinge on the assumption that sick veterans do not migrate to districts in which Republican Congressional candidates face close elections in order to secure higher pensions. For simplicity, I check whether veterans that move are likely to move to districts in which a Republican Congressman may perceive the election to be “close” and
therefore define “close” as a Republican vote share lying between 0.3 and 0.7 inclusive. Candidates were likely to perceive an election as being close simply if the race had a candidate from another party present. Therefore, I use the wide span of 30 percent to 70 percent to define a “close” vote. Of veterans that move to new districts, an average of 10.44 percent (st. dev. = 0.306) migrate from districts in which Republican vote share was below 30 percent or above 70 percent to districts in which Republican Congressional candidates faced close elections. Therefore, veterans who moved chose districts with closer elections. However, to more concretely determine whether sicker veterans migrated to more Republican districts, I use a simple conditional logit model of migration. The dependent variable is the probability that a veteran moves to a new district with close elections when the originating district did not have close elections. The independent variable is the incidence of any disease in the year \( t - 1 \). The model has the following form:

\[
\text{Prob}(\text{move}_{it} = 1) = \frac{1}{1 + \exp[-(\beta_0 + \beta_1 s_{\text{sick}, it-1} + G_i)]},
\]

where \( s_{\text{sick}} \) is a dummy variable equal to 1 if a veteran has any illness in a given year and \( C \) are a set of controls. Controls include a quadratic time trend, BMI, rank in the army, marital status, occupation, retirement status, prisoner-of-war-status, and war wounds.\(^{30}\)

The results presented in Table 7 show that if a veteran was sick with any illness in the previous period, he is not more likely to move to a district in which the Republican Vote Share lies between 0.3 and 0.7. Therefore, while many veterans were migration in the period, they did not appear to be moving to close election districts upon falling sick.

**ROBUSTNESS CHECK—FARMLAND VALUES**

If Republican Congressional candidates were able to raise income and wealth levels in their districts through means other than increasing the pensions of Union Army veterans, then the identification strategy of this research breaks down. One check on the candidates’ abilities to affect the health of their constituents through practices, which increased income or wealth, other than boosting pensions is to determine the effect of close elections on farmland values. Farmland values are a particularly good measure of overall economic prosperity in a veteran’s district because the majority of veterans lived in rural areas.

\(^{30}\) I cluster by year and district.
Data on the average value of farm land per acre stratified by county over the years 1870 to 1930 come from Michael Haines’ ICPSR’s Historical, Demographic, Economic and Social Data: The United States, 1790–2002, Dataset 2896 Part 106. I match each county to its associated district and Republican Congressional vote share. I use the following specification:

\[ \logf \text{farm}_{dt} = \gamma_0 + \gamma_1 \text{vote}_{dt} + \Gamma C_{dt} + \epsilon_{dt}, \]

where \( \logf \text{farm} \) is the log of average farmland value per acre, \( \text{vote} \) is an indicator variable equal to 1 if Republican vote share is between 0.3 and 0.7 inclusive, \( C \) is a set of controls including year fixed effects, district time trends, and district fixed effects, \( d \) denotes the district, and \( t \) denotes time.

There is no significant effect of the closeness of elections for Republican Congressional candidates on farmland values per acre. The coefficient on the close-election indicator variable is \(-0.006\) and is not significant.32

ROBUSTNESS CHECK—FREQUENCY OF EXAMS ACROSS DISTRICTS

In order to use the presence of close elections for Republican Congressional candidates in a pensioner’s district as an exogenous source

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31 In cases where one county overlaps two districts, I map the county to the district in which most of its area lies.

32 There were 163,020 observations, and the t-statistic for Republican Congressional vote share is \(-1.26\).
of variation in pension income, it cannot be the case that the likelihood to apply for a pension (and then undergo exams) varies between close election districts and non-close election districts. Since the quadratic first-stage specification does not easily translate graphically, I once again define a “close” election to be one in which the Republican vote share lies between 0.3 and 0.7 inclusive. Figure 3 shows the frequency of exams per year for those already on the pension rolls. Between 1893 and 1906, which are the exam years in the estimation sample, there is little difference in the frequency of applications between close and non-close election districts.

Robustness Check—Fraudulent Medical Claims

The empirical strategy of this research hinges on the validity of the statements testifying to the diseases faced by veterans on surgeons’ certificates. It does not affect this research if examining surgeons embellished the severity of illnesses faced by veterans because I am testing the effect
of pension income on the *first* incidence of the disease.\textsuperscript{33} Physicians were instructed to rate the severity of the illness as opposed to stating opinions as to how much money a veteran deserved based on his illness.\textsuperscript{34} In data cleaning, disease conditions preceded by adjectives such as “severe” or “acute” were ignored as were explicit descriptions. For example, if a surgeon states that a veteran has “severe” asthma and had “prolonged inspiration, shortened expiration, and whistling heard all over the lungs” during an exam, as was the case for veteran Henry Greeley during his first exam in 1888, the dummy variable for asthma would be one for the year 1888 until Greeley’s death. Alternatively, if the examining surgeon had simply described Greeley’s condition as “asthma caught at camp Banks at Georgetown,” the dummy variable would still equal one. Therefore, the asthma variable is simply a dummy equal to one if the surgeon diagnoses the veteran with asthma, using the exact word “asthma,” and zero otherwise.

It is, however, imperative that surgeons report the correct disease conditions as opposed to fabricating a story of illness when the veteran is perfectly healthy. If surgeons made honest mistakes when diagnosing patients, this would just create noise in the dependent variable but would not bias coefficient estimates. However, if surgeons diagnosed veterans with fictitious illnesses when in fact they were perfectly health, then this would bias income effects downward because otherwise healthy veterans with high pension incomes would look sick. However, since veterans did not see the same surgeon at each exam, and since surgeons did not receive disease information for a veteran prior to each exam, it is possible to test whether veterans exhibit the same disease conditions across exams.

To test whether veterans exhibit the same disease across exams, it is necessary to consider an illness that cannot resolve between exams. Therefore, I check whether the presence of any type of hernia, which was

\textsuperscript{33} While physicians may have exaggerated the severity of illnesses faced by veterans in an effort to secure higher pensions for veterans, these exaggerations did not lead to increases in pension amounts. See the *Instructions to Examining Surgeons*, which were manuals published by the Pension Bureau and frequently updated to reflect changes in the pension law. These manuals were circulated to all examining surgeons. Instructions relevant to Civil War veterans are known to have been published in the years 1871, 1877, 1884, 1891, 1895, 1901, 1905, and 1908, and most of these copies can be found at the Regenstein Library at the University of Chicago. There is evidence that other instructions to surgeons may exist between the years 1871 and 1908, however, at the time of this research, additional instruction manuals have not been found.

\textsuperscript{34} An example can be found in the *Instructions* published in 1884, p. 8: “All disability which cause a lesser degree of disability than that resulting from anchylosis [the stiffening of joints] of the wrist or ankle should be determined by comparison with anchylosis of the wrist or ankle, and stated by fractions of totals, as 1/8, 1/4, 3/8, 1/2, 5/8, 3/4, or 7/8 of total.” Therefore, examining surgeons rated disability but did not determine the payouts awarded to veterans.
normally permanent amongst the Union Army cohort, changes within an individual’s record over time (as individuals undergo new exams). Of veterans who had ever had a hernia period to their last exam, 97.4 percent still had the hernia at the time of their last exam. Other conditions such as arteriosclerosis are considerably more difficult to diagnose across exams. Of those who had arteriosclerosis prior to their last exam, only 84.4 percent still had the condition on their last exam. Similarly, of those with emphysema, which is an incurable lung condition, prior to their last exam, 87.9 percent are shown to still have the condition on their last exam.

ROBUSTNESS CHECK—UNDERREPORTING OF NEW ILLNESSES AFTER RECEIPT OF PENSION

One concern is that those receiving large pensions by 1893 would not be incentivized to report new illnesses because they would not be able to attain another increase in pension income. Conversely, veterans who were receiving relatively low pensions might be more incentivized to go for a new exam in hopes of increasing their pension awards. If veterans were incentivized to apply for increases, and therefore go for more exams, conditional on their previous pension awards, then the results of the disease onset regressions presented in Table 4 could be biased upwards.

To test this concern, I use a probit model to test whether the monthly pension amount received at the end of 1892 predicts a new exam in 1893, 1894, and 1895. The results are reported in Table 8. Receiving an extra dollar of monthly pension income in 1892 is associated with a 0.5 percent decrease in the chance that a veteran has an exam in 1893. In 1894 and 1895, the pension amount received in 1892 does not significantly predict whether a veteran will have an additional exam. Therefore, while receiving a lower pension income does predict an increased chance of a new exam in 1893, the effect is small and disappears in subsequent years.

ROBUSTNESS CHECK—“OVER-REPORTING” ON SURGEONS’ CERTIFICATES

In this research, I show that veterans living in close elections districts received increased pension awards. This may be because surgeons more closely examined veterans from close election districts, and thus described more of the veteran’s health conditions on the surgeons’ certificate, in order to help the veteran secure a pension for disability.
By contrast, in landslide districts surgeons may have only included a minimal number of conditions on the surgeons’ certificate because providing a pension increase and courting the veteran’s vote was not a priority in these districts. Therefore, this type of bias present among examining surgeons would downwardly bias the coefficient on pension income in Tables 4 and 5. This is because it would appear as though living in a close election district brought about an increased number of illnesses relative to the number faced by those living in landslide districts. However, the bias, if present, strengthens the main results of the article.

**CONCLUSION**

In this research, I explore the impact of income on adult health at the end of the nineteenth and early twentieth century. To circumvent reverse causality—the scenario in which health influences income—I use an exogenous source of variation in pension income: the presence of close elections for Republican candidates of a pensioner’s Congressional district. I find large effects and conclude that increases in income, in the absence of public health interventions or medical advancements, profoundly influenced the health trajectories of male cohorts born in the mid-nineteenth century for the better. In particular, I do not find that increased pension income operated on health through increased caloric intake, which is a mechanism widely discussed in the literature on the U.S. mortality transition (Fogel 2004; McKeown 1976). Instead, I find that increased pension income caused veterans to have more children, which had differential

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### Table 8

PROBIT REGRESSION—PREDICTING NEW EXAMS FOR THOSE ON ROLLS BY 1893

<table>
<thead>
<tr>
<th></th>
<th>t=1893</th>
<th></th>
<th>t=1894</th>
<th></th>
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<tr>
<td></td>
<td>Coefficient</td>
<td>Mean</td>
<td>Coefficient</td>
<td>Mean</td>
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<tr>
<td>Pension amount in 1892</td>
<td>-.005***</td>
<td>$10.13</td>
<td>-.0007</td>
<td>$9.77</td>
<td>-.0001</td>
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<td>Birth year</td>
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<td>.0013***</td>
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<td>State fixed effects</td>
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<tr>
<td>% of vets having exam:</td>
<td>14.34</td>
<td></td>
<td>15.31</td>
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<td>11.92</td>
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</table>

$t$ statistics in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Number of veterans: 11,755. Source: Author’s calculations.
effects on health depending on the occupation of the veteran. In particular, the health of farmers was more improved relative to the health of those in other occupations.

The results of this research shed light on current debates regarding the health benefits of cash transfers to adults living in developing nations in which the socio-economic status (SES) gradient in health is large, as was the case in the United States a century ago. Entitlement programs providing cash transfers, of particular interest today, are the South African pensions and Mexico’s Oportunidades system. Anne Case (2004) uses data on the South African pension system to determine the effect of income on health status, but is unable to make specific assessments regarding the health of recipients because health status is self-reported in the sample. Investigators of income’s effect on health using data from the Oportunidades program suffer from the same problem as most information on health status is similarly self-reported. For regions in which individuals suffer from high rates of infectious illnesses, the results of this research suggest that cash transfers have large benefits. In light of the results, it appears that the tradeoffs between providing cash transfer to adults in developing countries as opposed to public health interventions is narrower than previously thought.

The results of this research are also pertinent to discussions of the relationship between income and retirement, as income may act on retirement through health. First, the design of pension systems in countries with wide SES gradients in health must account for positive income effects on longevity. Second, since income affects current health status and expected longevity, individual decisions regarding labor force participation and retirement are ambiguous and would be an interesting avenue for further research.

REFERENCES


