

THE WAR ON POVERTY'S EXPERIMENT IN PUBLIC MEDICINE:  
COMMUNITY HEALTH CENTERS AND THE MORTALITY OF OLDER AMERICANS

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Abstract:

This paper uses the rollout of the first Community Health Centers (CHCs) to estimate the long-term health effects of increasing access to primary care. The results show that CHCs reduced age-adjusted mortality rates among those 50 and older by almost 2 percent within 10 years. The implied 6- to 8-percent decrease in one-year mortality risk among the treated amounts to 18 to 24 percent of the 1966 poor-nonpoor mortality gap for this age group. Large effects for those 65 and older suggest that increased access to primary care has long-term benefits, even for populations with near universal health insurance.

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In 1965 U.S. policymakers began a bold experiment in the provision of health care to the poor. Unlike the era's large Medicare and Medicaid programs, which subsidize the purchase of health care from private providers, Community Health Centers (CHCs) used federal funds to *deliver* primary care to underserved populations.<sup>1</sup> From the outset, CHCs complemented federal health insurance programs by increasing the availability and convenience of care while reducing the cost to patients: CHCs charged on a sliding-scale but also located in disadvantaged neighborhoods, offered home visits, and provided transportation to appointments.

Like today's Patient Protection and Affordable Care Act (ACA), the CHC model met with considerable opposition. Critics referred to the program as "a step toward socialism" (Sardell 1988: 61). Doctors feared competition from CHCs and expressed concerns about the quality of CHC care owing to their greater staffing with nurses and social workers (rather than professional physicians) and the development of practice guidelines by "lay persons" (62). In 1966, the president of the American Medical Association (AMA) spoke against the program, arguing that the government's role in health care should be limited to "the overall stimulation and support of private enterprise, rather than undertaking specific operational or directional capacities" (Hudson 1966: 99).

Despite these objections, CHCs' model of primary care has survived for almost 50 years and has recently enjoyed a significant expansion in funding.<sup>2</sup> In 2010 the Affordable Care Act (ACA) appropriated \$11 billion over five years to establish them as one of the pillars of health care reform—infrastructure intended to help meet the service needs of the millions of Americans projected to gain health insurance under the ACA's provisions. Part of the rationale for CHCs' expansion relies on claims that they improve access to primary care *while* curbing health care costs. In fact, previous research is almost unanimous that CHCs increase the use of preventive care, improve the management of chronic conditions, and, ultimately, reduce emergency room visits for the low-income and uninsured populations (Cunningham 2006, Falik et al. 2006, Rust et al. 2009)—all of which are argued to reduce total system costs (Hawkins and Schwartz 2003).

Yet the limitations of existing studies caution against these strong causal claims. Not only have studies had difficulty identifying CHCs' effects on health care utilization, but very few

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<sup>1</sup> Although the earliest health centers were called "neighborhood health centers," we use the term "community health centers" throughout the paper. Today CHCs include "Federally Qualified Health Centers" (FQHC) and so-called "Look-Alike FQHCs."

<sup>2</sup> Between 2001 and 2007, the Federal Health Center Growth Initiative doubled health center funding from \$1 to \$2 billion (US DHHS 2008). The 2009 American Reinvestment and Recovery Act allocated funds to build 126 new facilities and expand 1,100 existing CHCs (Inglehart 2010).

studies have considered their effects on health. Even taking for granted that CHCs increase service use, the program need not improve health or reduce health care costs if—as articulated by the program’s early critics—its services are lower quality than those available from private and non-profit providers (covered by Medicare and Medicaid for many CHC patients). Moreover, if lower quality services gradually erode health, CHCs could ultimately *raise* health care costs. Claims about the costs and benefits of funding CHCs, therefore, depend crucially on how they affect health—especially in the longer term.

This paper uses the rollout of the first CHCs from 1965 to 1974 to provide the first evidence on their long-term health effects and, more generally, of increasing access to primary care. An important benefit of this historical vantage point is that we can evaluate the *cumulative* impact of primary care to the poor up to 15 years after local CHCs began, while using the “great administrative confusion” at the Office of Economic Opportunity (OEO) as our source of identification (Levine 1970). We focus on the mortality of adults 50 and older for substantive and practical reasons. Substantively, adults 50 and older account for an important and rising share of U.S. health care costs, and their health (and mortality) is sensitive to the provision of medical care.<sup>3</sup> A practical reason is that adults 50 and older comprise over 80 percent of deaths in the U.S. during our period of interest (which provides large sample sizes), and their mortality is consistently and precisely measured in *every* year at the county-level.

Our main results show that *when* communities received a CHC program predicts off-trend reductions in older-adult mortality sustained over 15 years. One decade after CHCs began, age-adjusted, older-adult mortality rates remained almost 2 percent lower than pre-program levels, mostly due to decreases in deaths from cardiovascular-disease-related causes. These estimates are unchanged and often strengthened by including state-by-year fixed effects, county-level federal per-capita medical spending, and linear county-level trends, as well as by reweighting the distribution of observed characteristics in untreated locations. Consistent with a causal interpretation of our estimates, a falsification test shows that CHCs had no impact on accident-related mortality. The test results also stand contrary to an alternative explanation: that the establishment of CHCs proxies for improvements in emergency care or the growth in local hospital capacity. The translation of our intention-to-treat effect into a treatment effect on the treated implies a 6- to 8-percent reduction in age-adjusted mortality among the 50-and-older

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<sup>3</sup> Infant mortality responds differently to the CHC program, which we address in a companion paper.

poor—an effect equal to 18 to 24 percent of the 1966 poor-nonpoor mortality gap for the same age group.

Interestingly, the largest reductions in mortality were achieved among the Medicare eligible without an accompanying increase in Medicare spending. Our analysis of the restricted Survey of Health Services Utilization and Expenditures highlights two important reasons for this. As CHCs brought primary care to underserved areas, the share of older, poor adults reporting a “regular source of care” increased by over 20 percent. In addition, substantially discounted medications at CHCs’ in-house pharmacies (not covered by Medicaid or Medicare) reduced the share of the older poor with any prescription drug expenditures by almost 40 percent. These findings—and the likelihood that reductions in mortality understate the broader health benefits of CHCs—highlight the value of interventions that increase access to primary care, even for populations with near universal health insurance coverage.

## **I. BACKGROUND AND EXPECTED EFFECTS OF COMMUNITY HEALTH CENTER PROGRAM**

### *A. Brief History of Community Health Centers*

In his first State of the Union Address in January 1964, President Lyndon B. Johnson declared an “unconditional war on poverty.” Central to his war was the 1964 Economic Opportunity Act (EOA) (P.L. 88-452, 78 Stat. 508), which aimed to “eliminate the paradox of poverty in the midst of plenty.” The Office of Economic Opportunity (OEO) was created to administer the EOA initiatives and made direct grants to local organizations.<sup>4</sup>

Initially, the OEO focused on programs to promote human capital and community development like Head Start and Job Corps. But OEO administrators soon discovered that health problems and access to care limited participation in their programs. In the 1950s and 1960s, few charity or reduced-cost providers existed in many parts of the U.S., especially rural areas. Even in cities, few office-based practitioners remained since private physicians followed the suburbanizing middle class (Sardell 1986: 45). Doctor Raymond Wheeler, a member of the Citizen’s Board of Inquiry into Hunger in the U.S. from 1967 to 1971, vividly described the limited access to medical care among the poor:

*We saw hundreds of people whose only hope of obtaining medical care was to become an emergency which could not be turned away. We heard countless*

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<sup>4</sup> OEO funding was intended to ensure “the maximum feasible participation” of the poor and empower those who had been excluded from local politics to create positive changes in their community. This direct-to-local-organization granting made the War on Poverty programs controversial, because they challenged traditional political structures and entrenched local interests.

*stories of driving 50 or 100 miles to a city general hospital after refusal of care at a local hospital.* (1971: 2)

And, even if an outpatient department existed locally, the “four-hour wait, multiple referrals, incredible discontinuity of care and various other indignities” (Knowles 1969: 178) certainly would have deterred many of the poor from seeking care there for non-emergencies.

To address the health needs of the poor, the OEO initiated the CHC program in 1965, which aimed to deliver affordable, comprehensive care to disadvantaged populations. The initial wave of CHC grants established eight demonstration projects administered through medical schools, hospitals, and boards of health. The program expanded with the 1966 amendments to the EOA and, again, as part of the 1967 Partnership for Health Amendments, which allocated additional funds for the Department of Health Education and Welfare (DHEW) to initiate CHCs (Davis and Schoen 1977: 163). By 1974 CHCs existed in 117 counties nationwide with each site serving an average of 13,330 registered patients annually (178).

*B. What Did Neighborhood Health Centers Do and Whom Did They Serve?*

Because OEO administrators believed that existing health services had failed to reach the poor, CHC grants were channeled to “alternative” delivery strategies.<sup>5</sup> The OEO model allowed *any* organization to apply and receive funding. Administrators initially lacked clear funding guidelines and report having received applications from “various and sundry groups” that often had little to do with the spirit of the legislation (Gillette 1996: 196, quoting Theodore M. Berry, assistant director of the OEO). Finding proposals that fit the objectives of the CHC program was no small task. Awardees tended to be “one leading-edge, creative person who managed to get enough resources together...pulling them [others at the organization] kicking and screaming into something that they really didn’t want to be in, but that had lots of dollars attached to it” (May et al. 1980: 587).

The breadth of CHCs’ ideals and activities makes it difficult to quantify their implementation. One 1973 financial audit provides a snapshot of their medical services (Davis and Schoen 1978: 187; reprinted in appendix table B1). Overall, CHCs provided medical care on average 2.6 times per year per registered patient and medical prescriptions 2.5 times per year per registered patient at CHCs’ in-house pharmacies. Laboratory tests were performed an average of

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<sup>5</sup> Lee Schorr, an administrator responsible for developing the CHC program, noted that, “we very quickly decided that if the OEO was going to spend any substantial amounts of money on health, it would be directed to changing the organizational framework through which health services were being delivered to poor people” (May et al. 1980: 585).

1.8 times per year, dental care was provided 0.6 times per year, and X-rays 0.3 times per year per registrant. In addition, more than 90 percent provided home health care and transportation to appointments (Health Services Administration 1974: 60). Often, counties programs had multiple clinics and mobile sites.<sup>6</sup>

Two waves of the Survey of Health Services Utilization and Expenditures (SHSUE) provide a unique longitudinal perspective on how CHCs affected local health care use and expenditures in the late 1960s. The 1963 SHSUE interviewed households before the CHC program began, and the 1970 SHSUE interviewed households *in the same* primary sampling unit (PSU) after CHCs were operating in 17 out of the 73 PSUs in the sample (see Appendix A for details on the data). This allows us to examine how clinic use changed in communities receiving CHCs by 1970 and how these changes varied by income group in the following differences-in-differences (DiD) specification:

$$(1) \quad Y_{ijt} = \mathbf{X}'_{ijt}\boldsymbol{\beta}_1 + D_t\beta_2 + \mathbf{I}'_{ijt}\boldsymbol{\beta}_3 + D_j^*\mathbf{I}'_{ijt}\boldsymbol{\beta}_4 + D_tD_j^*\mathbf{I}'_{ijt}\boldsymbol{\beta}_5 + f_j + \varepsilon_{jt}.$$

$\mathbf{X}_{ijt}$  is a column vector including a constant and individual-level characteristics: indicators for sex, 5-year age-groups, race, education and area-size.  $\mathbf{I}_{ijt}$  is a column vector of dummy variables for households in different income groups (below 100 percent, 100 to 299 percent, 300 to 449 percent; 450 percent and above is omitted), and  $f_j$  is a set of PSU fixed effects.  $D_t$  is a dummy variable for 1970, and  $D_j^* = 1(T_j^* < 70)$  equals one if PSU  $j$  had a CHC before 1970. Time-invariant, cross-sectional differences in the use of clinics before the arrival of CHCs will be captured in the PSU fixed effects, and national changes in clinic use will be captured by the dummy for 1970. Standard errors are clustered at the PSU level.

The dependent variable is equal to 1 if the respondent reported a “clinic” as their usual source of care. Although “clinics” included sources of care other than CHCs, the DiD specification only attributes to the CHC program differential changes in the use of clinics in areas receiving CHCs. The point estimates of interest,  $\boldsymbol{\beta}_5$ , capture the differential change from 1963 to 1970 in the use of clinics in areas receiving CHCs by household income. Consistent with

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<sup>6</sup> The 1972 Directory of CHCs (DHEW 1972), for instance, reports that the CHC program in Denver, Colorado, had two health centers, 11 satellite health “stations,” a staff of over 800 members, and was supposed to serve a population of 287,869 (almost 106,000 patients were enrolled in 1971). The CHC program in Oakland, California, provided comprehensive ambulatory care 24 hours per day, employed 52 physicians and aimed to serve a population of 40,000 (17,289 were enrolled in 1971). This directory provides a rare snapshot of these programs. On average, reporting CHCs targeted populations of 71,000 and had about 21,500 patients enrolled. Part of the population-enrollment difference reflects the fact that many reporting CHC programs were not yet fully operational. Using the enrollment to target population ratios for fully operational CHCs, we estimate that annual enrollment would reach around 45 percent of the target population, or an average of 32,000 patients per county.

the CHC program increasing clinic use among those with lower incomes, the differential change in clinic use in treated PSUs for the poorest households was 16 percentage points (0.16, s.e. 0.05), an increase of 166 percent over the 1963 mean for this group (0.096). The differential increase was relatively smaller and statistically insignificant for higher income households—9 percentage points for those at 100 to 299 percent and 0.2 percent among those at 300 to 449 percent of the poverty line. The differential increase in treated PSUs from 1963 to 1970 averaged across all income groups was 14 percentage points (0.14, s.e. 0.023) and even larger, at 16 percentage points (0.16, s.e. 0.06), when excluding locations that were not treated.

*C. The Expected Effects of Community Health Centers on Older-Adult Mortality*

The CHC program rolled out during a period of dramatic declines in U.S. mortality. Age-adjusted mortality rates for 50+ year olds fell from 3,292 deaths per 100,000 in 1960 to 2,372 deaths per 100,000 in 1988—a decline of almost 28 percent (figure 1A; see section II.A for details on variable construction and appendix A for cause coding). Figures 1B, 1C, and 1D show that longevity also increased, as age-specific mortality rates for ages 50–64, 65–79, and 80+ fell by 30, 28 and 25 percent, respectively, over the same period. Much of the decline in mortality rates was driven by the reduction in deaths from major cardiovascular causes (CVD), which includes both diseases of the heart as well as cerebrovascular causes like strokes (see appendix figure B1 for cause-specific trends), which, in 1960, accounted for over half of all deaths among those ages 50+. From 1960 to 1988, CVD-related mortality had fallen by almost 50 percent. Deaths due to disease of the heart had fallen by 42 percent (676 deaths per 100,000) and other CVD-related causes by 60 percent (288 deaths per 100,000).

Important innovations in anti-hypertensive drugs, including diuretics and beta-blockers, shaped these declines (Crimmins 1981: 244, Cutler and Kadiyala 2003). After randomized trials by the Veterans Administration Cooperative Group (VACG) demonstrated the effectiveness of these drugs in reducing hypertension-related mortality and morbidity, the National Institutes of Health launched a campaign to promote awareness of hypertension as the “silent killer.” People were encouraged to “know your [blood pressure] number,” and physicians were encouraged to screen for and treat the disease.<sup>7</sup> During the 1970s and 1980s, the share of hypertensives who

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<sup>7</sup> The first VACG study randomly assigned 73 middle aged men with moderate hypertension (diastolic blood pressure between 115 and 129 mm Hg) to a treatment group that received three hypertension medications; 73 men were also assigned to a control group that received placebo medications. Patients were followed for 4 and 24 months. The control group experienced 27 morbid events and 4 deaths while the treatment group experienced only two morbid events and zero deaths (VACG 1967). Researchers terminated the study for moderate hypertensives after six months due to the large observed treatment effects. The second VACG study used the same methodology but focused on 380 men with low hypertension (diastolic blood pressure between 90 and 114

learned their blood pressure number increased from 50 to over 70 percent and the share on anti-hypertensive medication increased from 35 to over 55 percent (Cutler and Kadiyala 2003: figures 12 and 13).

Over the same period, mortality due to infectious diseases, diabetes, and accidents also fell steadily. In contrast, cancer-related deaths increased. The causes of these increases in cancer and diabetes are not well understood but are often linked to worsening diet, increases in smoking, changes in cancer reporting, and reductions in heart disease and stroke, which were the main competing mortality risks.

CHCs potentially affected these mortality trends by reducing both the financial and “non-financial costs” of receiving primary care, including discounted appointments and drugs, reduced time costs, and reduced interpersonal barriers (community members were hired to do health education and outreach). A standard model of health care utilization predicts that CHCs’ delivery of lower-cost care and prescription drugs should impact older adult mortality risk through two main channels. First, lower financial and non-financial costs should (weakly) increase the use of primary care on both the extensive and intensive margins.<sup>8</sup> This effect should be largest among those least able to afford (without insurance, for instance) or to travel to receive medical care. Greater use of primary care should reduce mortality by increasing early detection of health problems, especially asymptomatic but lethal conditions like hypertension. Complementing detection, drug coverage may have been particularly important, because Medicare did not reimburse these costs (Finkelstein and McKnight 2008) and Medicaid rarely covered drugs (Davis and Schoen 1977: 55).<sup>9</sup> Reduced-cost drugs (like anti-hypertensive medications) should (weakly) increase compliance with recommended treatments and also reduce mortality risk. Finally, CHCs may have reduced the cost of learning about Medicare and Medicaid, which should also decrease mortality through better treatment in hospitals (Chay et al. 2011).

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mm Hg). The study followed patients for an average of 3.3 years during which time 21 control patients and 10 treatment patients died. In addition, more than twice as many control patients had assessable morbid events (VACG 1970).

<sup>8</sup> Okada and Wan (1980) provide evidence for this using the OEO’s before-and-after survey of CHC catchment areas in five cities (Boston, Charleston, South Carolina, Atlanta, Kansas City, and Palo Alto). In their comparison of average annual visits to physicians before and after the introduction of CHCs, they show that the 65 and older group is the *only* group in which average annual physicians’ visits had increased by more in CHC catchment areas than in the U.S. overall between 1969 and 1975. According to their table 5 (524), the unadjusted difference in visits for those 65 and older in areas with CHCs *had fallen* by 66 percent over this period and remained constant for those 45 to 64, whereas the unadjusted difference had grown considerably for those under 44.

<sup>9</sup> The incidence of cardiovascular-related mortality was over 47 percent higher among whites with less than 8 years of schooling relative to whites with at least one year of college (Kitagawa and Hauser 1973: 76). This is also consistent with the RAND Health Experiment’s finding that free care significantly improved the control of hypertension and reduced mortality among hypertensives (Newhouse et al. 1993).

A second channel through which CHCs may have diverted care from other sources. The effect of this diversion on mortality, however, depends upon the relative quality of CHC care. Many accounts suggest that CHCs provided higher quality care than did hospital out-patient departments, but for many patients the relevant alternative was care from private providers that were covered under Medicaid or Medicare. If CHCs provided *lower* quality care than alternatives, then the diversion of care would tend to increase shorter- and longer-term mortality risk. On the other hand, if CHCs provided relatively *higher* quality care than these private alternatives, then the diversion of care to CHCs would reduce mortality risk. These opposing effects make the overall effects of CHCs on mortality theoretically ambiguous.

Further complicating these predictions is the possibility that greater use of primary care and the diversion of care could have important, community-level externalities. Treating infectious disease, for instance, may reduce mortality among non-CHC patients. Health information and education has the same potential for community-level spillovers if, for instance, CHC patients share this information with their spouses and neighbors who are not CHC patients. Finally, the diversion of less-urgent cases from emergency departments may have improved treatment of urgent cases, which could also decrease community-level mortality risk among non-CHC patients.

In summary, both the sign and the magnitude of CHCs' effects on mortality are theoretically ambiguous. After reviewing previous studies, this paper presents and uses a new reduced-form empirical strategy to quantify the program's effects on mortality through the combination of these channels.

#### *D. Previous Studies of the Effects of Community Health Centers*

Although a large body of research has examined CHCs, these studies' reliance on empirical methodologies with limited internal and external validity limits causal inferences about CHCs' health effects.<sup>10</sup> The literature's use of cross-sectional variation is tenuous, because communities with CHCs differ in a number of observable ways from non-CHC communities. Table 1 shows that counties with CHCs established in the period we consider (we call these "treated" counties)

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<sup>10</sup> Much of the literature examines a single CHC or a narrow geographic area and, therefore, cannot separate the effects of CHCs from changes in local policies or circumstances. For examples of case studies, see Bellin et al. (1969) and Moore et al. (1972) on Boston; Chabot (1971) on Denver; Hochheiser et al (1971) on Rochester; Gordis (1973) on Baltimore; and O'Conner et al. (1990) on an unspecified area in rural North Carolina. Two studies compare outcomes in five cities (Okada and Wan 1980, Freeman et al. 1982). Focusing on later periods, Deprez et al. (1987) use data from 36 CHCs in Maine in 1980, Ulmer et al. (2000) study medical records from 20 clinics across 10 states, and Epstein (2001) focuses on clinics in Virginia only. In studies of CHCs' effects on diabetes, Chin et al. (2000) examine 55 Midwestern cities and Bell et al. (2001) study 14 clinics in North Carolina.

tended to be more urban, more affluent (greater share of households with incomes above \$10,000), have lower poverty rates (smaller share of households with incomes below \$3,000, the 1960 poverty line for a family of four), have a larger share of nonwhites, and have more active MDs per capita. They also tended to have a medical school. Although this seems contrary to the program's mission of reaching the underserved, it is consistent with local advocates submitting grant proposals on behalf of needier residents. More affluent, urban areas were best situated to do this, which implies that CHCs were established in communities with greater health disparities. These observable differences in communities with CHCs suggest they may have differed in *unobserved* ways as well.

More recent studies of CHCs use panel data to account for these cross-sectional differences using fixed effects.<sup>11</sup> Using a 1996 to 2006 county panel and a specification with clinic and year fixed effects, Lo Sasso and Byck (2010) provide the best evidence to date that increases in CHC funding raise service availability and staffing and reduce uncompensated care. But, rather than quantifying the causal effects of CHC funding, their results could reflect omitted variables that affect both CHC performance and administrator decisions. For instance, Lo Sasso and Byck's results could capture administrator decisions to defund poorly performing CHCs or increase CHC funding as part of a community development effort. Both would lead Lo Sasso and Byck's empirical strategy to overstate the effects of CHCs. On the other hand, if administrators increased funding to help failing CHCs or in response to declining local investments in community health, Lo Sasso and Byck's estimates could understate the effects of CHC funding. To avoid both sources of endogeneity, this paper's empirical strategy relies only upon variation in the date CHCs were established.

In addition to these methodological shortcomings, another limitation of the literature is that few studies consider CHCs' *health* effects. The handful that do consider them focus on infants (Chabot 1971, Goldman and Grossman 1988, Shi et al. 2004a, and Shi et al. 2004b) or specific health conditions (Gordis 1973 for rheumatic fever; Dignan 1979 for cardiovascular disease; Hicks et al. 2006 for asthma; Chin et al. 2000 and Hicks et al. 2006 for diabetes; O'Connor et al. 1990 and Hicks et al. 2006 for hypertension; Hedberg et al. 1996 and Falik et al. 2001 for preventative

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<sup>11</sup> In one of the earliest studies of this type, Goldman and Grossman (1988) looked at infant mortality rates in the 678 largest counties between 1970 and 1978 and shows that an increase in the number of CHCs is associated with an overall reduction in infant mortality rates, particularly among blacks. Other recent examples include Shi et al. (2003) and O'Malley et al. (2005).

care). None, to our knowledge, has considered CHCs' cumulative or longer-term impacts on older-adult mortality, which is the focus of this study.

## **II. DATA AND EMPIRICAL STRATEGY: USING THE ROLLOUT OF COMMUNITY HEALTH CENTERS TO IDENTIFY THEIR MORTALITY EFFECTS**

Newly compiled data on *when* and *where* CHCs were established facilitate this paper's empirical strategy. The following sections describe our data on CHCs and county-level mortality rates, present empirical evidence motivating our identification strategy, and then discuss our event-study specification.

### *A. Data on Health Centers and County-Level Mortality Rates*

To document the rollout of CHCs from 1965 to 1974, we collected information on their establishment from two sources. Information on CHCs established by the OEO comes from the National Archives Community Action Program (NACAP) files, and information on CHCs established by the DHEW was hand-entered from annual Public Health Service (PHS) Reports. After verifying this information against other primary sources (see Appendix A), our final database contains information on (1) the county where CHCs delivered services, which allows each federal grant to be linked to county-level mortality rates; and (2) the date that each county received its *first* CHC *services* grant (this excludes planning grants), which provides a consistent proxy for the year that each CHC began operating.<sup>12</sup>

Figure 2 shows the rollout of the CHC program between 1965 and 1974 by the year and county where CHCs were established (in most cases, county or local governments were not grantees). Counties with CHCs established in fiscal years 1965 to 1966 are outlined without shading. The first CHCs in Columbia Point (Boston), Massachusetts, and Mound Bayou, Mississippi, were established in fiscal year 1965 in collaboration with Tufts Medical School. As the CHC program expanded, it achieved broad geographic coverage. All U.S. regions had CHCs: 48 were established in the South, 21 in the Northeast, 22 in the Midwest, and 26 in the West. There is also considerable within-state variation in CHC establishment dates. At least one CHC was established in each of 40 states; two or more were established in 28 states; three or more were established in 21 states.

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<sup>12</sup> Although some CHCs opened after 1974, we have not located data that show when these programs began. We have, however, used additional data on federal grants to test robustness to excluding counties that get CHC programs after 1978. Excluding these counties changes our estimates very little.

We link the CHC database to county mortality rates calculated from the 1959 to 1988 Vital Statistics Multiple-Cause of Death Files (US DHHS and ICPSR 2007), which contain the universe of civilian deaths reported in the U.S. by cause, age, and county of residence of the decedent.<sup>13</sup> These data are aggregated to create age-specific and age-adjusted mortality rates. The age-specific mortality rate,  $ASMR_{ta}$ , in year  $t$  is the count of deaths for age group  $a$  (50–54, 55–59, ..., 75–79, 80–84, and 85+) divided by the population in age group  $a$  in year  $t$  per 100,000. The age-adjusted mortality rate in year  $t$  is a weighted sum of age-specific mortality rates,  $AMR_t = \sum_{a=1}^8 s_a ASMR_{ta}$ , where  $s_a$  is the 1960 national population share of age group  $a$  (among those 50 and older).<sup>14</sup> By holding  $s_a$  fixed, changes in age-adjusted mortality rates reflect changes in the likelihood of dying rather than changes in population age structure. We also break down the  $AMR$  by six leading causes of death: 1) diseases of the heart, (2) other cardiovascular disease,<sup>15</sup> (3) cancer, (4) infectious disease, (5) diabetes, and (6) accidents. We include accidents, the sixth leading cause of death in 1960 for those 50 and older, as a falsification test, because accidental deaths should not be affected by the establishment of CHCs.<sup>16</sup>

### B. Empirical Specification

Our empirical strategy uses variation in *when* CHC programs were established to evaluate their effects on mortality. A key identifying assumption in our framework—that the timing of establishment is uncorrelated with other determinants of changes in older-adult mortality—is supported by two empirical tests. First, most 1960 socio-demographic characteristics in table 1 fail to predict *when* a CHC was established among those receiving one in our period of interest (appendix table B2).<sup>17</sup> Significant exceptions are urban share and share of MDs per county population. This is not surprising: Larger, denser places had more resources and organizations

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<sup>13</sup> The exception is 1972, which contains a 50 percent sample. In 1981 and 1982, we use Mortality Detail files, which only contain data on the underlying cause of death, because the Multiple Cause files contain a 50 percent sample of deaths for some states. Since we only use information on the underlying cause this does not affect the comparability of our mortality measures between years.

<sup>14</sup> Denominators for these rates were constructed by linearly interpolating population between the 1950 and 1960 censuses (Haines and ICPSR 2005) and the 1969 to 1988 Surveillance Epidemiology and End Results (SEER 2009) data. The age-group-specific mortality rates used in this analysis are age-adjusted by 5-year groups.

<sup>15</sup> Together, diseases of the heart and other cardiovascular disease constitute “major cardiovascular disease” (CVD). We include general arteriosclerosis in “diseases of the heart.”

<sup>16</sup> See Appendix A for details on cause of death coding and Appendix B for mortality trends by cause of death. Our age-adjusted rates for these six causes trend smoothly through two ICD-revisions (1968 and 1977).

<sup>17</sup> We choose these characteristics, because they have been shown to predict the timing of the implementation of other War on Poverty programs. Almond, Chay and Greenstone (forthcoming) and Hoynes and Schanzenbach (2009) report statistically significant relationships between these characteristics and the timing of Medicare certification and the initiation of the food stamps program. The power of our study relative to theirs is limited as we do not have information on the month of program initiation and not all of the counties in the U.S. received CHCs. For these reasons, we additionally use these demographic characteristics to construct additional control variables.

that could apply for funding, and CHCs were set up in locations with physicians to staff them. To account for these potential threats to the internal validity of our analysis, we include urban-group-by-year fixed effects and linear trends in total MDs in our primary specifications described below.

Second, we examine whether CHC program establishment is correlated with pre-program mortality rates or trends for those 50 and older. This could be the case if, for instance, proposals originated sooner in locations with higher mortality rates or the OEO prioritized locations based upon their mortality rates or early 1960s mortality trends. Figure 3, which plots the *AMR* in 1965 and changes in the *AMR* from 1960 to 1965 against the year of CHC program establishment in funded communities, shows no evidence that either was the case (see appendix figure B2 for results by age groups). Both levels and changes in *AMRs* are uncorrelated with the establishment of CHCs. Overall, the lack of a systematic correlation between CHC program establishment, most socio-demographic characteristics, and mortality rates is consistent with oral histories' characterization of a "wild" funding process.

Our empirical strategy exploits variation in the timing of CHC program establishment dates within a flexible event-study framework (Jacobson et al. 1993),

$$(2) \quad Y_{jt} = \theta_j + \gamma_{u(j)t} + \delta_{s(j)t} + \mathbf{X}'_{jt}\boldsymbol{\beta} + \sum_{y=-7}^{-2} \pi_y D_j 1(t - T_j^* = y) + \sum_{y=0}^{15} \tau_y D_j 1(t - T_j^* = y) + \varepsilon_{jt}.$$

Here  $Y_{jt}$  is a mortality outcome in county  $j$  in year  $t = 1959, \dots, 1988$ .<sup>18</sup>  $\theta_j$  is a set of county fixed effects, which absorbs time-invariant differences in observable (table 1) and unobservable characteristics and allows consistent estimation of  $\pi$  and  $\tau$  even in the presence of differences between treated and untreated locations.  $\gamma_{u(j)t}$  is a set of urban-group-by-year fixed effects (defined as year dummies interacted with five groupings of share of a county's population in urban areas,  $u$ :  $0$ ,  $0 < u < 25$ ,  $25 \leq u < 50$ ,  $50 \leq u < 75$ ,  $75 \leq u \leq 100$ ), which captures the differential diffusion of medical technologies and changes in health in areas with varying levels of urbanization.  $\delta_{s(j)t}$  is either a set of year fixed effects or state-by-year fixed effects, which

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<sup>18</sup> To ensure the parameters of the model are well estimated, we specify the dependent variable in levels rather than logs (many counties have zero deaths for a particular age or cause grouping) and limit the sample of counties to those with at least 100 residents over the age of 80 in every year. This restriction eliminates 425 counties out of 3,063 (12,840 county-year observations out of 91,980). Only two of the eliminated counties are treated (Costilla and Sauguache counties in Colorado). The mean of the excluded counties' mortality rates are similar (2,752 versus 2,757), but they have a higher standard deviation (845 versus 465) because their mortality rates are more volatile. In addition, the excluded counties sometimes have zero population in the age-groups we consider. This exclusion allows us to estimate models on a balanced set of counties. Because all models are weighted by the relevant 1960 population (to minimize the importance of noisier mortality rates in small counties), we exclude New York, Los Angeles and Chicago (these places had two million *more* residents in 1960 than the next largest counties). Both restrictions leave us with 112 of the 117 counties with CHCs and 2,523 counties without CHCs.

captures time-varying national or state-level changes in Medicare and Medicaid and the implementation of the Civil Rights Act (Almond, Chay, and Greenstone forthcoming).  $X_{jt}$  is a column vector including a constant, the interaction of 1960 characteristics with linear time trends (share of population in urban area, in rural area, under 5 years of age, 65 or older, nonwhite, with 12 or more years of education, with less than 4 years of education, in households with income less than \$3,000, in households with incomes greater than \$10,000, total active MDs), and annual county-level per capita measures of government transfers from the Bureau of Economic Analysis Regional Information System (REIS) (cash public assistance benefits such as Aid to Families with Dependent Children, Supplemental Security Income, and General Assistance; medical spending such as Medicare, Medicaid, and military health care; and cash retirement and disability payments).<sup>19</sup>

To explore the sensitivity of our results, we add covariates sequentially, estimate models with county-specific, linear time trends ( $\theta_j t$ ) (rather than parameterizing county trends using table 1 characteristics), and reweight the untreated counties using an estimate of the propensity of receiving a CHC to balance the characteristics of treated and untreated counties in table 1 (DiNardo, Fortin and Lemieux 1996, Heckman et al. 1998). Specifically, we construct propensity scores by estimating a probit with the binary dependent variable equal to 1 if a county received a CHC from 1965 to 1974 using flexible functional forms for a rich set of covariates. This yields estimates of the propensity of receiving a CHC for each county,  $p_j = P(D_j=1|Z_j)$ . We then reweight untreated counties using the ratio,  $p_j(1-q)/(1-p_j)q$ , where  $q$  is the share of counties that receive CHCs. Thus, this reweighting strategy combines information from multiple observables to give untreated counties that were ex ante most likely to have received a CHC more importance in the comparison group and weight down those that were ex ante less likely to have received a CHC. Columns 7 and 8 of table 1 show that, except for population size and location in the Northeast (which our models account for in our urban-by-year and state-by-year fixed effects), the treated county characteristics are statistically indistinguishable from those in reweighted untreated counties. As recommended by Crump et al. (2009), we also trim our estimation sample to use counties with propensity scores of at least 0.10 and no more than 0.90.

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<sup>19</sup> For the purposes of this analysis, these covariates are fairly comprehensive because OEO administrators would have had limited information beyond them. The county characteristics in  $X$  are comparable to specifications in Almond et al. (2008: 15) except that we also add information on the number of physicians per capita.

Appendix figure B4 provides full details on the construction of the propensity scores and their distribution in treated and untreated counties for the full and trimmed estimation samples.

We use a binary indicator of treatment,  $D_j$ , equal to one if the county ever received a CHC grant, because some CHCs built upon existing resources and others required the construction of new facilities and got substantially larger start-up grants. Thus, variation in grant amounts compensated for larger infrastructure needs rather than indicating larger treatment. In our heterogeneity analysis (section III.C), we show that the effects of larger and smaller per capita grants are comparable, which supports this empirical approach.<sup>20</sup> The estimates characterizing CHCs' effects are the coefficients on the interaction of  $D_j$  with  $1(t - T_j^* = y)$ , which is equal to one when the year of observation is  $y = -6, \dots, 0, \dots, 14$ , years from  $T_j^*$ , the date when a CHC was received in county  $j$  ( $y = -1$  omitted). Observations more than 6 years before or more than 14 years after CHC program establishment are captured by dummies,  $1(t - T_j^* \leq -7)$  and  $1(t - T_j^* \geq 15)$ . The point estimates,  $\pi_y$ , describe the evolution of mortality in eventually treated counties before CHCs began *net* of changes in untreated counties after adjusting for model covariates;  $\tau_y$  describes the divergence in outcomes  $y$  years *after* the CHC was established net of changes in untreated counties after adjusting for model covariates. Because  $y = -1$  is omitted, our point estimates describe the intention-to-treat effects of CHCs on mortality relative to the year before the CHC began (event-year  $-1$ ).

This specification provides several advantages over the more standard DiD methodology. Estimates of  $\pi_y$  allow a visual and statistical evaluation of the *evolution* of pre-treatment unobservables in CHC communities (rather than assuming that  $\pi_y = 0$  for  $y < 0$ ) that may bias estimates of  $\tau_y$ . Estimates of  $\pi_y$  also allow an explicit test of whether the “effects” preceded the treatment even by a few years—an important falsification test. Another advantage is that the less-restrictive event-study specification describes the dynamics of the treatment effects. This is especially important since we only observe the date CHCs were established (by first grant) and not when they began operating. An abrupt shift in  $\tau_y$  following the establishment of a CHC would suggest that they began operating immediately after receiving a grant whereas a break in trend would suggest a delay in CHCs becoming fully operational.

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<sup>20</sup> A practical reason for our choice of a binary treatment indicator is that the NACAP data are missing grant amounts for 1969, which we have not been able to recover from other sources.

After presenting the event-study estimates, we summarize their magnitudes and joint statistical significance in a DiD specification that replaces the individual year exposure dummies with year groups,

$$(3) \quad Y_{jt} = \theta_j + \gamma_{u(j)t} + \delta_{s(j)t} + \mathbf{X}'_{jt}\boldsymbol{\beta} + \sum_{g=-2}^{-1} \tilde{\pi}_g D_j D_j^g + \sum_{g=0}^3 \tilde{\tau}_g D_j D_j^g + \varepsilon_{jt}.$$

Here  $D_j^g$  is a binary variable equal to 1 if county  $j$  is treated and observed in event-year ( $y$ ) group  $g$ , where  $g = -2, -1$  indexes  $y \leq -7$  and  $-6 \leq y \leq -2$  and  $g = 0, 1, 2,$  and  $3$  index the categories  $0 \leq y \leq 4, 5 \leq y \leq 9, 10 \leq y \leq 14,$  and  $y \geq 15,$  respectively ( $g = -1$  is omitted). For both the event-study and DiD specifications, tables and figures present only coefficients estimated using a *balanced* set of counties (only event-years  $-6$  to  $+14$ ; not all treated counties are observed for  $y \leq -7$  or  $y \geq 15$ ). In all specifications, standard errors are corrected for heteroskedasticity and an arbitrary within-county covariance structure (Arellano 1987).

### III. EVENT-STUDY ESTIMATES OF COMMUNITY HEALTH CENTERS' EFFECT ON MORTALITY

#### A. Results for Age-Adjusted Mortality Rates

Figure 4A plots weighted, event-study estimates for three models that use the *AMR* as the dependent variable. Model 1 includes only county fixed effects,  $\theta_j$ , and urban-group-by-year fixed effects,  $\gamma_{u(j)t}$ . Model 2 adds state-by-year fixed effects,  $\delta_{s(j)t}$  and county-level covariates,  $\mathbf{X}_{jt}$ . Model 3 adds state-by-year fixed effects and REIS covariates to model 1, but reweights the comparison group using the propensity of receiving a CHC (see appendix figure B4). Each of the models shows that CHCs had a significant effect on mortality. The econometric model captures well the wide-spread declines in the *AMR* in the pre-period (urban-by-year effects presented in appendix figure B3), and estimates of  $\pi$  are small in magnitude and statistically indistinguishable from zero. This means that the *AMRs* were not trending differently in eventually treated counties relative to untreated counties *before* the CHC program began. The establishment of a CHC, however, corresponds to a noticeable and statistically significant trend break in the *AMR*. In the four years after CHCs were established, the *AMR* fell more sharply relative to the modeled counterfactual and fell at roughly the same rate as the modeled counterfactual after event year 5. The trend break and the magnitudes of the effects are affected little as covariates are added.

Difference-in-differences summary estimates show the robustness of these effects (table 2). Model 2's addition of covariates and state-by-year effects to model 1 (column (2)) reduces the magnitudes of the estimates in the pre-period and *increases* the absolute value of the negative estimates in years 0 to 4 by 30 percent (from  $-29.5$ , s.e. 13.7, to  $-40.2$ , s.e. 9.6) and in years 5

to 9 by 20 percent (from  $-58.8$ , s.e.  $17.3$ , to  $-69.6$ , s.e.  $14.7$ ). Consistent with the lack of a pre-treatment trend in treated counties in figure 4A, the effects are robust to the addition of over 2,600 county-specific linear trends (column (3)). These trends alter the estimates for years 0 to 4 by fewer than 2.3 deaths and, in years 5 to 9, by fewer than 9 deaths. Reweighting untreated counties to balance their observable characteristics with treated counties (column (4)) reduces the estimates slightly, but neither set of estimates (column (3) or (4)) is statistically distinguishable from model 2.<sup>21</sup> The robustness of the estimates in the reweighted sample is particularly helpful in narrowing the scope of omitted variables bias. Because the reweighted sample has a slightly *larger* number of physicians per capita and is slightly *more* likely to have a medical school (though neither difference is statistically different, table 1 columns (7) and (8)), it is hard to argue that the diffusion of hospital treatments (such as the development of bypass surgery in 1968) disproportionately affected locations with CHCs. Overall, the results imply that CHCs reduced mortality by around 2 percent over the pre-program *AMRs* in treated counties within 10 years of program establishment.

*B. Translation of Intention to Treat Effects into Average Treatment Effects on the Treated*

These intention-to-treat estimates (ITT) average the effect of CHCs over all residents in a county 50 and older regardless of whether they benefited from CHCs' services. What do these ITTs imply about CHCs' treatment effects on the "treated"? (By "treated" we mean those who, as a consequence of CHC establishment, obtained direct or indirect benefits they would not have otherwise received.) We use two approaches to construct the implied average treatment effect on the treated (ATET) in order to gauge the magnitude of our estimates. The first approach assumes that *everyone* who was elderly and poor in a county with a CHC program was "treated"—even if they did not use the CHC facility. This approach assumes that CHCs affected both patients and the community at large through externalities (eradication of infectious disease, knowledge spillovers from public health education and information, and reduced crowding of emergency departments), but that these externalities are limited to the poor. Dividing the reduction in *AMR* of 60 per 100,000 (table 2, average over columns (1) to (4) for years 5 to 9) by the 25 percent of the 50+ county population that was below the poverty line in 1965 yields an ATET of 240 deaths per 100,000.

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<sup>21</sup> Trimming our sample using the Crump et al. (2009) recommended propensity score cutoffs of 0.10 and 0.90 increases column 4's estimates. See appendix figures B4 and B5 and table B3 for more details.

The second approach more narrowly assumes that only CHC *patients* were “treated.” Because our mortality estimates reflect the cumulative impact of CHCs on mortality, we use the 1970 SHSUE to approximate the share of 50+ residents in treated counties who had used CHCs over a five-year period. As a starting point, the SHSUE shows that 1.4 percent (1,358/100,000) of those 50 and older had used “a clinic not connected with a hospital—such as a Board of Health clinic or neighborhood health center.” This national estimate implies that 5.4 percent of residents 50 and older in counties with CHCs had used them by 1970. We adjust this figure to reflect underreporting and cumulative use in two additional steps. First, we inflate the estimate to reflect the high degree of retrospective underreporting of clinic use. Bound, Brown and Mathiowetz’s (2001) survey of measurement error in the reporting of public program use shows that survey responses agree with administrative records as little as 50 percent of the time; survey responses agree with provider records of “clinic visits” agrees as little as 39 percent of the time (p. 3813), which implies that the SHSUE one-year utilization rate may have been as high as 14 percent ( $5,432/.39 = 13,928$  per 100,000). Second, we use nine surveys conducted by the OEO in CHC catchment areas between 1968 and 1972 to adjust the estimates for cumulative use over five years (see appendix A for details on data). For respondents 50 and older, 75 percent of physician visits that occurred in the previous five years took place in the year prior to the survey, which inflates our estimates of 5-year use of CHCs to 18.5 percent—an estimate that is similar to the PSU-level increase in “clinic use” reported in section I.A. Dividing the *AMR* reduction of 60 deaths per 100,000 by the approximately 18.5 percent of the county population using these centers yields an estimated ATET of 323 deaths per 100,000. Keeping in mind that those most in need of medical services were the most likely CHC patients, this is a large but plausible treatment effect.

These two approaches imply ATETs ranging from a reduction of roughly 240 to 320 deaths per 100,000. Of course, the ATETs could be even larger if some CHC users would have obtained services otherwise (though historical accounts suggest this did not occur frequently). Consistent with CHCs providing primary care for urgent (but deferrable) conditions, the larger ATET is roughly two thirds the size of Chay et al.’s (2011) estimates of Medicare on one-year mortality rates. The range of ATETs is also plausible given the higher mortality rates among the poor. Using the 1966-1968 Mortality Followback Survey (MFS) to construct mortality rates by poverty status, these ATETs suggest that CHCs reduced the annual *AMR* among the poor ages 50 and

older by 6 to 8 percent within a decade.<sup>22</sup> These ATETs are also equivalent to 18 to 24 percent of the mortality gap between the poor and the non-poor for this age group, which lends credence to claims that CHCs reduced health disparities. The next section describes these mortality patterns by grant and community characteristics, age group, and cause of death to understand how CHCs achieved these effects.

### C. *Heterogeneity in Community Health Centers' Effects*

Table 3 investigates heterogeneity in CHCs' mortality effects by first (start-up) grant characteristics and baseline community characteristics, which also allows us to examine whether certain exceptional CHCs drive our results. For ease of interpretation, we implement these heterogeneity tests by replacing  $\sum_{g=-2}^{-1} \tilde{\pi}_g D_j D_j^g + \sum_{g=0}^3 \tilde{\tau}_g D_j D_j^g$  in equation 3 with  $\Sigma_k \left( \sum_{g=-2}^{-1} \tilde{\pi}_g^k D_j^k D_j^g + \sum_{g=0}^3 \tilde{\tau}_g^k D_j^k D_j^g \right)$ , where  $D_j^k$  is equal to 1 if the county received a CHC between 1965 and 1974 *and* belongs to group  $k$  (defined below). One hypothesis we test is whether larger start-up grants made CHCs more effective. Columns 1 and 2 present estimates for the group of CHCs receiving first grants below and above \$793,000, which was the median first CHC grant in 1970 dollars. Although CHCs' effects are statistically significant in both groups, their effects are absolutely (and statistically) larger in counties receiving above-median establishment grants. But the effects of above-median *per-capita* grants are very similar in magnitude and statistically indistinguishable from those of below-median *per-capita* grants (columns (3) and (4)). This is consistent with the idea that CHC programs in larger counties were more effective (see columns (5) and (6)) and historical accounts of larger grant sizes compensating for larger infrastructure needs. The comparability of effects across per-capita grant sizes also supports our empirical approach, which uses a binary indicator for treatment (rather than grant amounts).

Another hypothesis is that OEO-initiated CHCs were more effective, because they adhered more to the ideal of comprehensive medicine, did not implement a means test, and placed more emphasis on non-medical services (Davis and Schoen 1973: 163, Sardell 1983) than did DHEW-funded CHCs. Consistent with this hypothesis, the absolute effects of OEO-initiated CHCs are significantly larger at the 10-percent level than those of DHEW-initiated CHCs in every 5-year

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<sup>22</sup> Vital Statistics mortality data do not contain information on income or education. The 1966-1968 National Mortality Followback Survey (US DHHS and ICPSR 1986) sampled death certificates from 1966 to 1968 and surveyed death record informants about the decedent's household income in 1965. Together with the 1965 CPS population estimates, these data allow us to compute mortality rates by income (King et al. 2010). This survey shows that the *AMR* for those 50 and older were 50 percent higher for decedents living in a household with a total income of less than the 1965 poverty line for a family of four (<\$3,000) than for those above this threshold (4,127 versus 2,769).

period after they began. The “OEO effect,” however, largely reflects the types of communities these centers served, because OEO programs began earlier and were more likely to be in urban areas (table 1). Indeed, columns (7) and (8) of table 3 show that almost all of CHCs’ effects appeared in the more urban counties. This urban concentration of effects points to the importance of proximity of CHCs to their clientele. Whereas urban CHCs successfully served patients in their neighborhoods, rural CHCs had trouble reaching their highly dispersed target populations.

The next set of estimates (columns 9 to 16) investigates whether CHCs had larger effects in locations with above-median *AMRs* in 1960, above-median numbers of physicians per capita, or in different census regions. Overall, CHCs’ effects are similar in areas with different numbers of physicians and across regions, but the effects are significantly stronger—both absolutely and relatively—in treated locations with higher 1960 *AMRs*. In counties with above-median 1960 *AMRs*, the point estimates are two to seven times larger and imply a reduction of 1.5 percent and 3 percent in years 0 to 4 and 5 to 9, respectively. Although CHCs appear to have been equally successful in all census regions of the U.S., their effects are concentrated in urban areas and locations with relatively high pre-period *AMRs*.

Next, we examine heterogeneity in CHCs’ mortality effects by age group. Figures 4B, 4C, and 4D plot weighted event-study estimates for models 1 to 3 of equation 2 using the all-cause *AMR* as the dependent variable for 50 to 64 year olds (B), 65 to 79 year olds (C), and 80+ year olds (D). Echoing the aggregated results, the pre-period estimates for each age group are close to zero and statistically insignificant. This means that trends in mortality rates before CHCs began were not different in counties that eventually received programs. As with the aggregate results, the estimates exhibit a noticeable trend break following CHC program establishment. For 50 to 64 year olds, the trend break in mortality is subtle and less immediate, which reflects both the lower, shorter-term mortality risk and potentially delayed effects of primary care for this age group. In contrast, the *AMR* for 65 to 79 year olds fell noticeably following the establishment of CHCs. The trend break in mortality for those 80+ is also evident.

Difference-in-differences summary estimates (table 4) show the robustness of these age-group-specific effects. Across the four models, the pre-CHC effect is small and statistically insignificant, and the post-treatment estimates are not statistically different from those in model 2. For 50 to 64 year olds (panel A, column (2)), mortality rates were 1 percent lower 0 to 4 years after CHCs began and 2.1 percent lower in years 5 to 9. Interestingly, the estimates for the 65-and-over population are relatively larger than those for 50 to 64 year-olds. In years 5 to 9, the

*AMRs* were roughly 2.3 percent lower for 65 to 79 year olds (panel B, column (2)) and 1.7 percent for those 80 and older (panel C, column (2)).

Finally, table 5 shows that CHCs differentially affected deaths by different causes (these cause-specific estimates should be interpreted with caution, because the cause reporting varies over time and often with the type of care available to decedents). Importantly, CHCs' effects were concentrated among deaths related to hypertension for all age groups, but they had effects on deaths on a broader set of cause categories for 50 to 64 year olds. For these near elderly, table 5A provides evidence that CHCs reduced the *AMR* for all causes except accidents. Causes related to hypertension show the largest response to CHCs: heart-disease-related mortality fell by 1.3 percent (7.4 deaths per 100,000) and other CVD-related mortality fell by 2.8 percent (3.3 deaths per 100,000) within the first five years CHCs operated. In years 5 to 9, deaths in the latter category had dropped by 5.2 percent (6.1 deaths per 100,000). However, deaths from cancer, infectious disease, and diabetes also declined significantly – by 1.6 percent, 4.2 percent, and almost 7 percent, respectively, by years 5 to 9. Despite being more common than deaths from infectious disease and diabetes, deaths due to accidents, our falsification test, is the only cause that does not show an economically and statistically significant decline.

These patterns are also evident for those 65 and older (table 5, panels B and C). Heart-disease-related deaths fell by 1.7 percent in both age groups in years 5 to 9, and other CVD-related deaths fell by a large and statistically significant 3.7 percent for the 65 to 79 year olds and 4 percent for the 80+ year olds. For both age groups, deaths due to cancer, infectious disease, and diabetes show no trend break when CHCs began.<sup>23</sup> It is also reassuring that CHCs have no effects on accident-related mortality.

These age-group-specific estimates translate into large impacts of CHCs on major CVD mortality for the 50-and-older population.<sup>24</sup> Although the difference in treated counties for both CVD categories was not (individually or jointly) statistically different from zero in the pre-period, age-adjusted deaths by those causes fell more sharply after CHCs began operating. Model 2 implies that 5 to 9 years following program establishment, CHCs reduced the *AMRs* due to heart disease by 1.6 percent (25 deaths per 100,000) and other CVD-related deaths by 4 percent (17 deaths per 100,000) relative to the pre-program means. These results are consistent

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<sup>23</sup> Although the estimate for cancer for those 65 to 79 is statistically significant, so is the pre-period estimate. The corresponding event-study figure shows no trend break associated with the establishment of a CHC.

<sup>24</sup> These estimates are reported in appendix table B4.

with the RAND Health Experiment (Newhouse et al. 1993) and Chay et al.'s (2011) study of Medicare, which find that both reductions in the cost of care translated into large reductions in hypertension and major CVD deaths, respectively. By contrast, we find no association of CHCs with reductions in mortality from cancer, infectious disease, diabetes, or accidents. For accidents, the estimates are close to zero, comparable in each 5-year period, and not statistically significant.

In summary, these findings underscore the importance of CHCs' primary care model and shed light on potential mechanisms for their effects. First, CHCs' effects on the Medicare ineligible (50 to 64 year olds) show that the results reflect more than Medicare eligibility. CHC-related reductions in deaths due to cancer, infectious diseases, and diabetes for the 50 to 64 age group are consistent with prevention, earlier diagnosis, and treatment reducing mortality for a broad set of causes. These effects highlight the program's broad potential for improving longer-term health outcomes. Second, CHCs' effects on the Medicare eligible (65+ year olds) suggest their potential for improving health outcomes among those with generous health insurance. Within a year of Medicare's nationwide implementation in 1966, almost all of those eligible for coverage were enrolled (Chay et al. 2011). Evidence that the relative effects of CHCs are larger for the Medicare-eligible and that CHCs reduced deaths related to CVD makes it likely that many of CHCs' benefits were realized through screening for hypertension and making reduced cost drugs available, and especially growing awareness of new anti-hypertensive medications. These improvements may also reflect counseling about the warning signs of a heart attack or stroke or simple advice to improve diet or quit smoking. Thus, CHCs' primary care services complemented those available through Medicare and hospitals.

#### **IV. AN ANALYSIS OF POTENTIAL THREATS TO INTERNAL VALIDITY**

Important threats to the internal validity of the study are unobserved shocks that both reduce mortality and occur concurrently with or just after CHC program establishment. Candidates include targeted federal spending (e.g., other OEO programs which were packaged with CHC funding), correlated changes in other local medical resources, and larger effects of Medicaid in counties with CHCs.

Although oral histories provide no indication that OEO administrators intentionally bundled funded programs, the coincidence of CHC program establishment with other federal grants may have happened inadvertently or because certain (more affluent and urban, table 1) communities were more effective at writing proposals. This is an important concern, because studies show that

other OEO programs also impacted mortality.<sup>25</sup> Although the literature does not consider the effects of OEO programs on older individuals, the programs may have benefited this group by increasing the resources available to households supporting them.

Newly compiled data on grants for other federal programs uniquely allow us to test this concern. Figure 5 presents event-study estimates for six separate binary dependent variables equal to one if a county received a grant for the indicated program. Panel A is shown for the CHC program: The likelihood of receiving a CHC grant before establishment is zero, and all counties with a CHC program established received their first CHC grant in event-year zero. Subsequent reductions in CHC coverage, however, do not indicate that these programs were discontinued. CHCs often received multi-year federal grants and were supported by other state and local sources. For instance, 92 percent of locations treated before 1975 also received a federal grant between 1978 and 1980, and only a handful of CHCs closed.

In order for other federal funding to bias our estimates of CHCs' effects, the remaining panels of figure 5 would need to show an abrupt shift or trend-break in funding around year 0, when the CHC program was established. In contrast, panels B through F show almost no change. There is no evidence of a coincident or subsequent shock to other health funding (panel B), Community Action Program administration (panel C, this includes local development projects), programs serving the elderly (panel D), Head Start funding (panel E), or legal services (panel F). Although Head Start and legal services may not directly affect the health of those 50 and older, a coincidence in the timing of CHC establishment with these program grants would indicate a packaging of War on Poverty spending that would threaten the validity of our empirical approach. The absence of correlated spending increases, however, suggests that the OEO itself did not bundle programs and that communities receiving CHCs did not simultaneously receive other grants because of especially talented grant writers.

Another alternative explanation for CHCs' mortality effects is that their establishment coincided with local changes in other health resources with similar effects on mortality. Correlated changes in hospital funding or capacity, for instance, could generate the same pattern of estimates. Using data from 1948 to 1990 from the American Hospital Association's (AHA) Annual Survey, figure 6 provides little evidence that this was the case. Event-study estimates using the number of hospitals and the number of hospital beds (both measured per 1,000

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<sup>25</sup> Ludwig and Miller (2007) document the relationship between Head Start and child mortality and Almond et al. (2008) document the relationship between Food Stamps and infant mortality.

residents) as dependent variables show that both evolved smoothly before and after CHC programs were established. The number of hospitals per capita rose faster in locations that eventually received CHCs, but the expansions in beds were roughly keeping pace with the growth in residents—both before and after the CHC program began. In short, figure 6 provides no evidence of correlated changes in hospital capacity.

Yet another alternative explanation for CHCs' mortality effects is that counties with CHCs may have benefited disproportionately from Medicaid. Although our primary models (2, 3, and 4) account for Medicaid's state-level roll-out using state-by-year fixed effects *and* although the timing of Medicaid implementation is uncorrelated with CHC establishment dates, Medicaid's effects may have been larger in poorer urban areas also served by CHCs (for reasons unrelated to CHCs).<sup>26</sup> We evaluate this directly by including in our model a binary variable for event-years to Medicaid implementation dates interacted with county-level characteristics such as high 1960 poverty rates, high numbers of active physicians, and the presence of a medical school. Appendix figure B6 shows that the estimated effects of CHCs from models with each of these sets of controls are similar in magnitude and statistically indistinguishable from our baseline estimates.

In summary, we find no evidence that correlated local shocks in federal spending, medical resources, or state-level adoption of Medicaid legislation compromise the internal validity of our research design or explain the mortality effects of CHCs.

## V. MECHANISMS FOR COMMUNITY HEALTH CENTERS' MORTALITY EFFECTS

Thus far, we document a large, negative relationship between CHCs and older adult mortality and build an empirical case that CHCs caused these declines. A final part of our causal story relates to *how* CHCs achieved these mortality improvements. This section assesses the contribution of three potential mechanisms linking CHCs to older-adult mortality: increases in the use and quality of primary care, the provision of anti-hypertensive drugs, and increases in the utilization of Medicare-covered services.

### A. *Use and Quality of Primary Care*

Our investigation of the primary care mechanism relies upon the 1963 and 1970 waves of the SHSUE as described in I.A, which contain comparable questions on whether respondents had

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<sup>26</sup> Regressing the year a CHC was established on the year Medicaid was passed results in a correlation of 0.071 (s.e. 0.081). Excluding Arizona, which is an outlier as a late Medicaid adopter, the correlation is 0.216 (s.e. 0.150). State-wide effects of Medicaid are captured in the specifications with state-by-year fixed effects.

a regular source of care,<sup>27</sup> whether they spent any money on prescription medication, the number of doctor and hospital visits, and whether they saw a physician in the previous year. For ease of interpretation, we estimate separate models of equation 1 for three household income groups (below 100 percent, 101 to 300 percent, 301 percent and above the poverty line). Aside from income groupings or their interactions (which will not be identified in most cases because we stratify by income group), our covariates are identical to those described in equation 1.

The results presented in table 6 provide evidence that CHCs increased poor adults' use of medical care and the frequency of their use of care. Column (1) shows that CHCs differentially changed the likelihood that the poorest adults report a regular source of care. Panel A shows that the share of poorest residents who have a regular source of care grew significantly by 24 percent (18 percentage points, s.e. 0.09, over a base of 0.77) in areas with CHCs by 1970, but this effect is much smaller and never statistically significant for higher income groups (panels B and C). Column (2) shows that the share of poor residents with any expenditures on prescription drugs was 38 (panel A) and 21 percent (panel B) lower in areas with CHCs by 1970 (relative to the 1963 mean in treated PSUs), but small and not statistically different from zero in these PSUs for residents at 300 or more percent of the poverty line (panel C). Finally, column (3) provides suggestive (though imprecise) evidence that poor respondents in areas with CHCs (panel A)—but not higher-income respondents—increased their medical visits from 1963 to 1970 (scheduled doctor visits and hospital admissions) by 48 percent (3.49 visits, s.e. 4.29, relative to a baseline of 7.35 visits). There is no evidence, however, that CHCs increase the probability of seeing a physician (in the previous year) for any income group (column (4)). In summary, the SHSUE suggest that CHCs increased the use of primary care among the poor along the intensive (but not extensive) margin and improved the quality and integration of that care by transferring a regular source. CHCs also reduced the likelihood that the poor paid for prescription drugs, which should have increased compliance with recommended treatments.

#### *B. The Provision of Lower-Cost Hypertension Drugs*

The most important advance in the outpatient treatment of CVD in the 1960's and 1970's was the development of anti-hypertensive medication, and CHCs' low-cost provision of these drugs through their in-house pharmacies may be an important mechanism for their effects on mortality and CVD mortality. Because the available data do not allow a direct test of the impact

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<sup>27</sup> Having a regular source of care is stronger predictor of positive health outcomes than having health insurance (Sox et al. 1998). Shea et al. (1992) show that having a regular source of care is predictive of compliance with anti-hypertension treatment.

of anti-hypertensive medications, we use the Hypertension Detection and Follow-up Program (HDFP)—a large, community-based, randomized trial of the mortality effects of hypertension drug treatment over five years—to generate a back-of-the-envelope calculation of the potential importance of this CHC service.

The HDFP trial obtained a sample of over 10,000 participants with hypertension (diastolic blood pressure over 90 mm Hg) in 1973. A randomly selected treatment group saw HDFP staff on a flexible schedule, received frequent follow-up and, importantly, was prescribed anti-hypertensive medication. The control group learned about their hypertension and was referred to their usual source of care (and may have taken anti-hypertensive drugs). Although HDFP did not assess the role of screening and diagnosis (all participants were screened and informed of their hypertension), the HDFP treatment group received anti-hypertensive drugs and auxiliary services, similar to the services provided by CHCs.

The results of the trial show a relative reduction in the five-year, all-cause mortality risk for treated participants ages 50 to 70 of -2,160 deaths per 100,000 (HDFP 1979; table 9). To translate this result into the potential effect of CHCs, we multiply the HDFP treatment effect by the share of people with hypertension who used CHCs, which we assume to be the 18.5 percent utilization rate in treated communities (section III.B), and also multiply by the 26.2 percent of the population who are hypertensives (National Health Examination Survey, USDHHS and NCHS 1989a and 1989b). If CHCs provided anti-hypertensive medication *but no screening* (as in the HDFP study), the trial shows that the aggregate, age-adjusted five-year mortality rate would have fallen by -105 deaths  $(-2,160 \cdot .262 \cdot .185)$  per 100,000. Of course, this estimate understates CHCs' effects on hypertension-related mortality through screening, education, and other non-drug channels.

To compare this back-of-the-envelope calculation to our estimates of one-year mortality reductions, we translate our ITT estimates for years 5 to 9 (-60, average over columns (1)-(4), table 2) into a five-year mortality reduction using the mean age-adjusted mortality rate in year -1 (3,280 deaths per 100,000, table 2) as the counterfactual mortality rate. Our estimates imply a difference of -263  $[= (1 - .0328)^5 - (1 - (.0328 - .0006))^5]$  per 100,000 in the five-year mortality risk. Extrapolating from the HDFP study, CHCs' effects through intensive treatment for hypertension could account for approximately 40 percent (105/263) of our ITT effects. The remaining 60 percent of the CHCs' effect may also incorporate the broader benefits of primary care on mortality as well as improvements in outcomes not related to hypertension. It is also

worth noting that the magnitude of the residual is consistent with the RAND Health Insurance Experiment, which found that nearly half of the reduction in blood pressure difference between those receiving free care and those with co-payment plans is attributable to the one-time initial blood pressure screening (Newhouse et al. 1993: 229, 243).

### C. *Information about and Use of Medicare*

As a final mechanism, CHCs may have influenced health simply by increasing knowledge about and use (e.g., via van transport to hospitals) of Medicare, which would be consistent with the suggestive SHSUE finding that CHCs increased medical visits. To examine this channel, we estimate the event-study model using real, county-level per-capita military and Medicare expenditures (available from 1959 to 1988) and Medicare expenditures (available for 1969 forward) as dependent variables.<sup>28</sup> If the effects of CHCs on elderly mortality arose primarily through greater use of Medicare in treated areas through treatments such as bypass surgery, then we should see per-capita Medicare spending increase following the establishment of CHCs. Figure 7, however, provides no evidence of this CHC-Medicare interaction. Total per-capita medical spending evolved smoothly before and after CHC program establishment, and per-capita Medicare expenditures did not increase after CHCs began operating. This does not mean that CHCs did not promote awareness of Medicare. Increases in primary care at CHCs could reduce the need for Medicare-covered services, while also *increasing* Medicare awareness among eligible patients requiring them. If this greater awareness led some patients to use Medicare-covered services while diverting others from hospitals, CHCs could have no effect on Medicare expenditure per capita. Thus, the data are consistent with (1) CHCs not promoting awareness and use of Medicare *or* (2) CHCs diverting some patients from using Medicare-covered services but increasing use among others. However, both scenarios imply that CHC services themselves drove the estimated reductions in mortality.

The bottom line of this analysis is that CHCs' mortality effects likely reflect the interactive, longer-term benefits of primary care and prescription drugs—rather than a single one of these mechanisms.

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<sup>28</sup> Because information on all per-capita medical expenditures is available from 1959 to 1988, figure 7 presents estimates from –6 forward for this outcome. In the specifications using Medicare expenditures as the dependent variable, we omit the pre-treatment coefficients from the figure because they are not based upon a balanced set of counties. We exclude counties with CHCs established before 1969 for the same reason.

## VI. THE LONGER-TERM RETURNS TO PRIMARY

Since 1965, the CHC experiment has been an important but understudied part of the U.S. health care safety net—not least because the CHC program costs so much less than Medicare and Medicaid. Even recent political support for the CHC program relates, in part, to its role as an *alternative* to expanding public health insurance (Mickey 2011). An important lesson from our analysis is that less expensive investments in the delivery of primary care may play an important role in improving the health of the poor.

The CHC program’s rollout from 1965 to 1974 presents a rare opportunity to quantify the effects of changes in access to primary care among the underserved. Within an event-study framework, our empirical strategy exploits the disorganized grant-making process during the War on Poverty’s early years. Consistent with accounts of “great administrative confusion” at the Office of Economic Opportunity (OEO) (Levine 1970), our data show no association of CHC program establishment with a variety of pre-treatment county characteristics, with 1965 mortality rates, with changes in mortality rates from 1960 to 1965, with funding for other OEO programs, or with local expansions in hospital capacity. When CHCs began, however, is an important predictor of sharp reductions in older-adult mortality.

Our results imply that CHC induced increases in primary care led to sustained health improvements over 15 years. One decade after CHCs were established, age-adjusted all-cause mortality rates remained almost 2 percent lower than pre-program rates, owing mostly to large reductions in cardiovascular-related deaths among 65 to 79 year olds. The implied treatment effects on the treated are a 6- to 8-percent reduction in age-adjusted mortality rates among the 50-and-older poor, which amounts to an 18 to 24 percent reduction in the 1966 poor-nonpoor mortality gap for the same age group. These estimates likely understate the broader effects of increasing access to primary care, because mortality fails to capture changes in either morbidity or disability, as well as other gains to well-being. Our focus on those 50 and older also excludes benefits to other age groups.<sup>29</sup>

Although some of CHCs’ longer-term benefits accrued to individuals ineligible for Medicare, the program achieved its largest reductions in mortality among the Medicare eligible

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<sup>29</sup> Multiplying model 2 estimates in figure 4 by the relevant population in treated counties gives 107,359 life-years gained in CHCs’ first ten years. We obtain the total cost of the CHC program over its first ten years by estimating the average cost of CHCs by years from the date of CHC establishment and multiplying by the 112 CHCs in our estimation sample, which yields \$4.2 billion in 2010 dollars. Naively dividing the number of life-years gained by the total cost of the CHC program gives a cost-per-life-year of approximately \$39,000 (in 2010 dollars).

without an accompanying increase in Medicare spending. Important reasons for this are that CHCs brought physicians and health care facilities to underserved areas and provided free or substantially discounted medications, none of which were provided by public health insurance. These findings highlight the potential returns to interventions that improve the delivery of primary care. Whether the benefits of CHCs for the 50-and-older population remain this large today and whether the program also benefited younger populations remain important areas for future research.

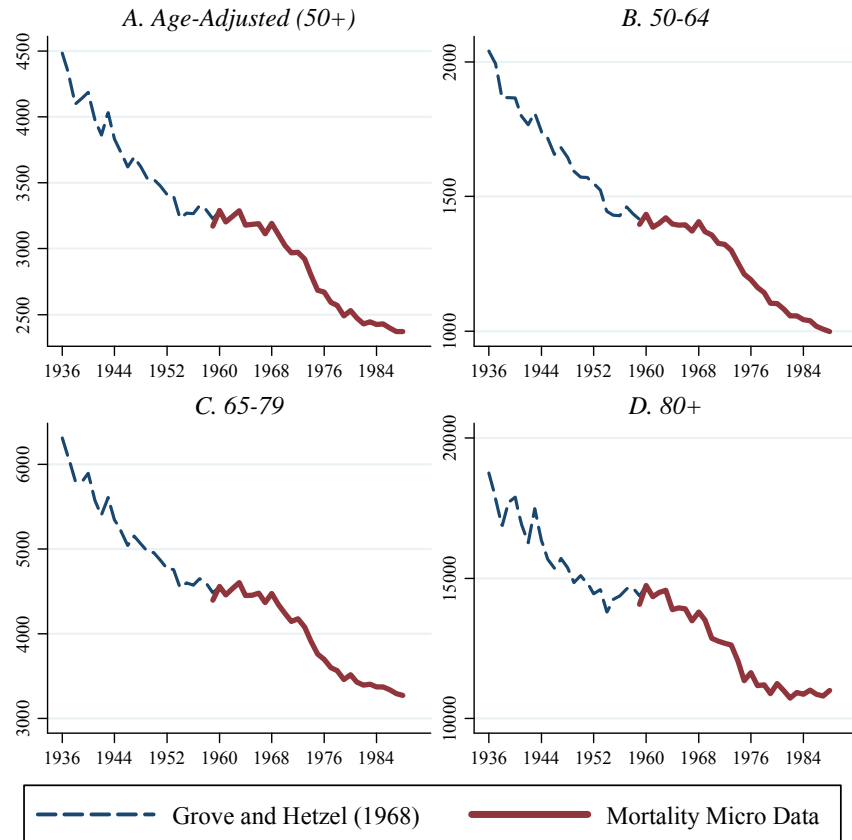
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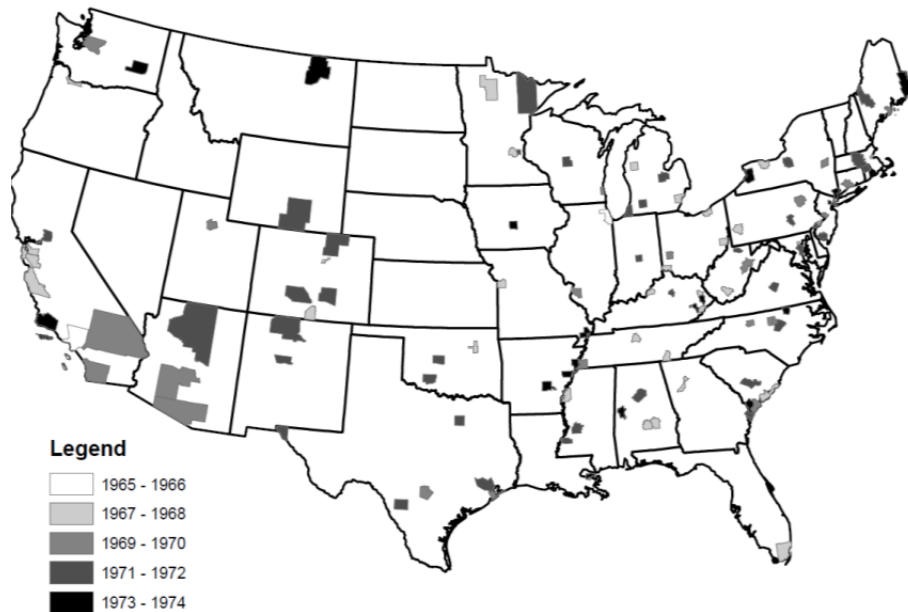
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**Figure 1. All-Cause Mortality Rates by Age Group, 1959 to 1988**



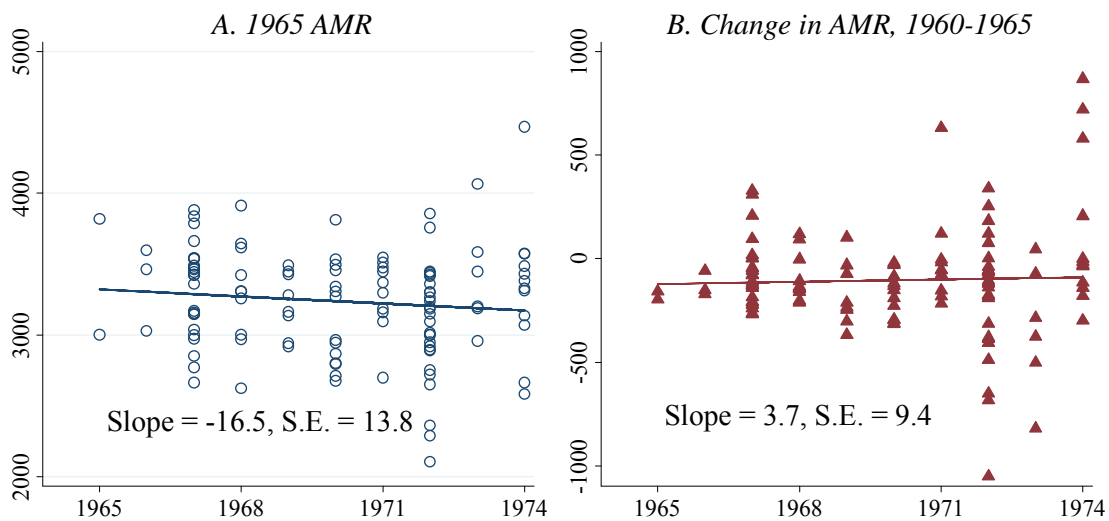
Sources: 1936-1958: Grove and Hetzel (1968), 1959 to 1988 Vital Statistics Multiple-Cause of Death Files (US DHHS 2007), 1950 and 1960 population estimates (Haines and ICPSR 2005), 1969 to 1988 population statistics (SEER 2009).

**Figure 2. The Establishment of Community Health Centers by County of Service Delivery, 1965 to 1974**



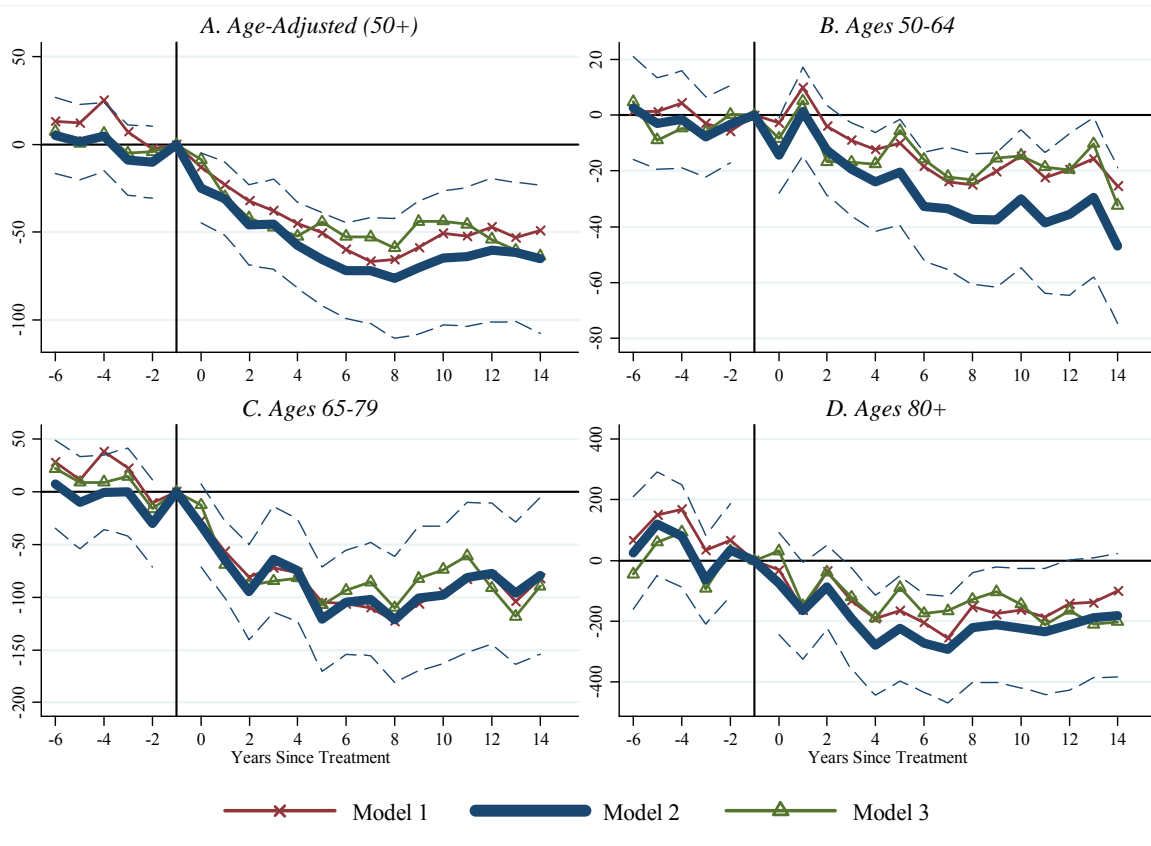
Note: Dates are the first year that a CHC was established in the county. Source: NACAP and Public Health Service Reports.

**Figure 3. Mortality Rates before the Community Health Center Program Began**



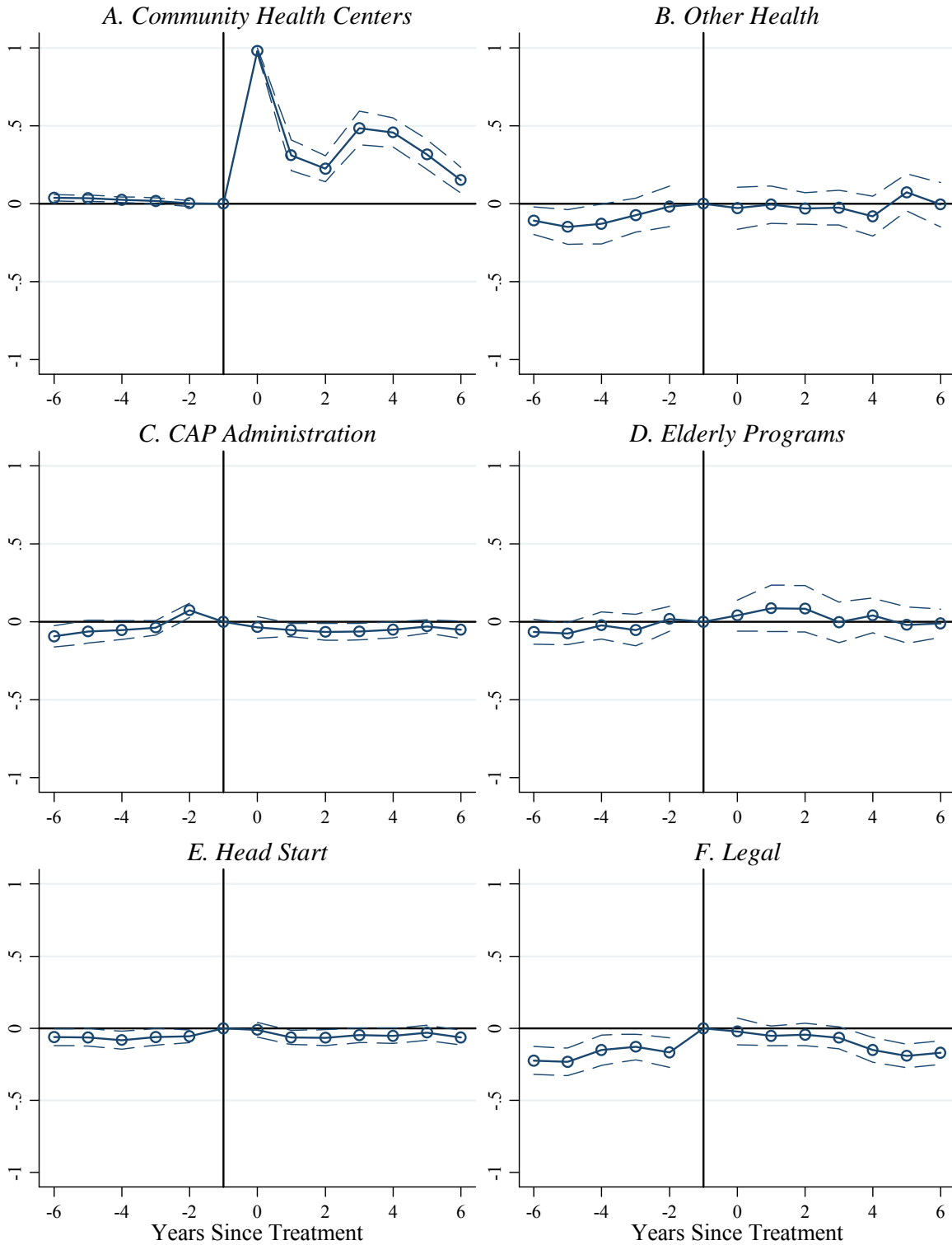
Notes: The dependent variable is indicated in the panel heading. Regression coefficients and predicted values are from univariate regressions of the dependent variable on mortality on the year CHCs were established for the 112 treated counties in the estimation sample. Source: See figures 1 and 2.

**Figure 4. Changes in All-Cause Mortality Rates with the Establishment of a Community Health Center**



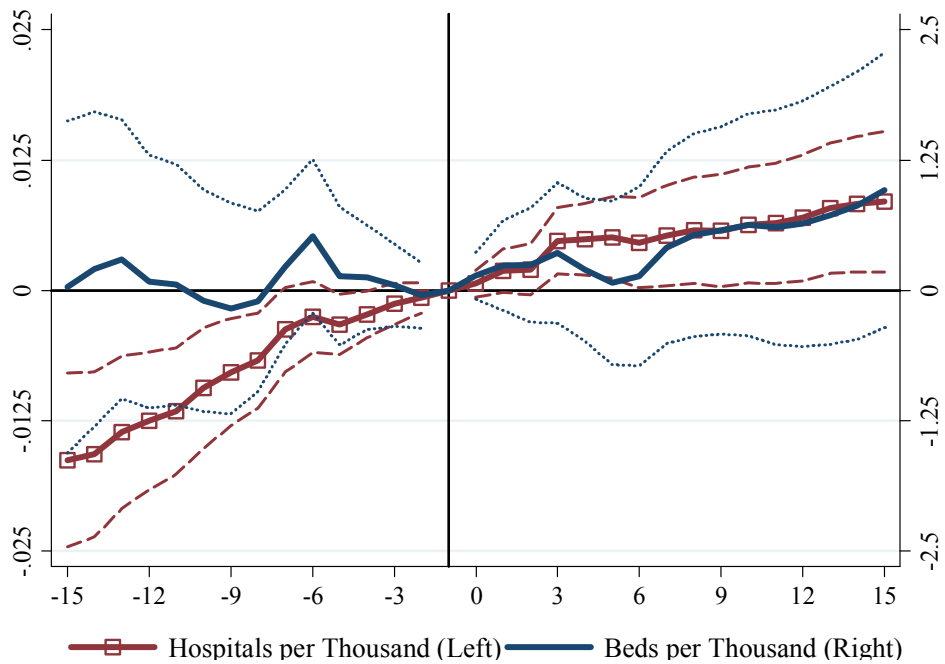
Notes: The dependent variable is the all-cause mortality rate for the indicated group. The coefficients are weighted, least-squares estimates of  $\alpha$  and  $\tau$  from different specifications of equation 2. Dashed lines are 95-percent confidence intervals using heteroskedasticity-robust standard errors clustered at the county level corresponding to model 2. Weights are the appropriate county populations in 1960. See text for model details. The year prior to a health center's establishment is omitted, so CHCs were funded for part of year 0 but for the entirety of years 1 to 14. Sample: 2,635 U.S. counties that had at least 100 residents over age 79 in every year between 1959 and 1988 (79,050 county-year observations). Source: Mortality rates constructed from the 1959 to 1988 Vital Statistics Multiple-Cause of Death Files (US DHHS 2007), 1950 and 1960 population estimates (Haines and ICPSR 2005), 1969 to 1988 population statistics (SEER 2009). Information on CHCs drawn from NACAP and Public Health Services Reports.

**Figure 5. Changes in Program Funding with the Establishment of a Community Health Center**



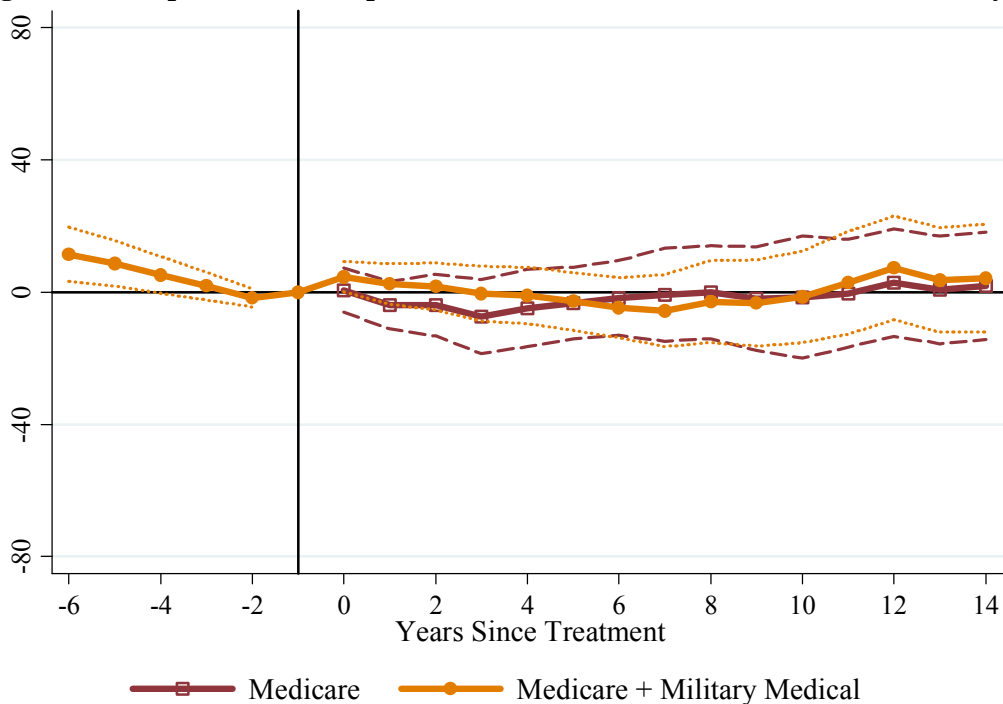
Notes: Each panel plots weighted least-squares estimates of  $\pi$  and  $\tau$  from equation 2 for model 2 excluding covariates. The dependent variable is equal to 1 if the county received *any* federal grant for the indicated program in a given year. Heteroskedasticity-robust standard errors clustered by county are used to construct point-wise, 95-percent confidence intervals, which are presented in dashed lines. See figure 4 notes for details on the specification and sample. Sources: NACAP and NAFO. Information on CHCs described in figure 2.

**Figure 6. Changes in Hospital Services with the Establishment of a Community Health Center**



Notes: Dependent variables are hospitals per thousand residents (left vertical axis) and beds per thousand residents (right vertical axis). Definitions of hospitals and hospital beds exclude mental institutions, tuberculosis sanatoriums, military hospitals and correctional hospitals. Because of inconsistencies in geographic coding, Virginia is excluded from the estimation sample. Data for 1954, 1977, and 1979 are missing and are linearly interpolated. The sample means are 0.24 for hospitals per capita and 24.3 for beds per capita. See figure 4 notes for more details. Source: 1948 to 1990 AHA Surveys. Information on CHCs drawn from NACAP and Public Health Services Reports as described in text.

**Figure 7. Changes in Per-Capita Medical Expenditures with the Establishment of a Community Health Center**



Notes: Dependent variables are per-capita Medicare + military medical spending and per-capita Medicare spending. The sample means (standard deviations) are 203.1 (163.6) for Medicare and 217.3 for Medicare + Military Medical (165.1). See figure 4 for more details on specifications and sample. Source: 1959 to 1988 REIS data. Information on CHCs drawn from NACAP and Public Health Services Reports as described in text.

**Table 1. 1960 Characteristics of Counties Receiving Community Health Centers, 1965 to 1974**

	CHC Established in			Estimation Sample			Reweighted Sample	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	1965- 1967 (N=26)	1968- 1970 (N=31)	1971- 1974 (N=55)	Counties with CHCs (N=112)	Counties without CHCs (N=2523)	P-value on t-test of difference (4)-(5)	Counties without CHCs (N=2523)	P-value on t-test of difference (4)-(7)
Mean 1960 Population of 50 and older	164,951	100,391	56,277	93,715	10,175	<0.01	42,913	<0.01
Mean Total 1960 Population	675,466	445,877	253,906	404,903	44,271	<0.01	195,050	<0.01
Percent of Total 1960 Population:								
in urban area	74.6	77.2	58.9	91.0	56.0	<0.01	74.3	0.18
in rural area	4.5	3.5	9.9	1.1	11.9	<0.01	5.0	0.16
in Northeast	15.4	16.1	20.0	29.9	23.6	<0.01	8.5	0.03
in Midwest	23.1	19.4	16.4	26.5	32.2	<0.01	27.8	0.31
in South	38.5	41.9	45.5	24.8	34.7	<0.01	44.3	0.89
in West	23.1	22.6	18.2	18.8	9.5	<0.01	19.4	0.85
under 5 years of age	11.0	11.7	12.3	11.2	11.2	0.23	11.7	0.62
65 or older	9.7	8.5	8.4	9.2	10.0	<0.01	8.7	0.99
Nonwhite	14.8	18.2	18.6	15.0	8.6	<0.01	19.6	0.57
with ≥12 years of education	39.6	42.1	38.2	43.1	40.7	0.25	42.4	0.17
with ≤4 years of education	9.9	9.7	13.2	7.5	8.7	<0.01	10.7	0.53
in households with income <\$3k	24.2	22.6	29.3	16.5	25.5	<0.01	24.5	0.40
in households with income >\$10k	14.8	14.2	12.2	17.8	12.2	<0.01	13.4	0.98
Medical Resources:								
Total Active MDs (per 1k)	6.7	8.2	4.59	6.1	2.73	<0.01	7.2	0.33
Any Medical Students, 1969	57.7	51.6	20	37.5	0.71	<0.01	46.0	0.42
Age-Adjusted Mortality in 1965	3,325	3,233	3,215	3,245	3,049	<0.01	3,275	0.66

Notes: County characteristics are not weighted by 1960 county populations, so that they can be interpreted as the shares for the average county in the relevant category. Column (7) applies propensity-score weights as described in the text and in appendix figure B4. Column (8) p-values are based upon bootstrapped standard errors with 1000 replications. The table sample (our estimation sample) contains 2,635 counties that had at least 100 residents over age 79 in every year between 1959 and 1988. Sources: 1960 County and City Databooks (Haines 2005) and 1960 Area Resource Files (US DHHS and ICPSR 1994). Information on CHCs drawn from NACAP and Public Health Service Reports.

**Table 2. Changes in All-Cause Mortality Rates with the Establishment of a Community Health Center**

	(1)	(2)	(3)	(4)
DV:		Age-Adjusted Mortality Rate (AMR)		
<i>Mean at t=-1:</i>	<i>Mean DV in treated counties in t=-1: 3281.6</i>			
Years -6 to -2	10.8 [10.4]	-2.0 [8.0]	-3.0 [8.1]	0.5 [10.2]
Years 0 to 4	-29.5 [13.7]	-40.2 [9.6]	-37.9 [8.9]	-34.4 [11.7]
Years 5 to 9	-58.8 [17.3]	-69.6 [14.7]	-61.4 [11.6]	-47.4 [16.9]
Years 10 to 14	-49.3 [21.1]	-61.8 [19.2]	-46.5 [15.4]	-50.4 [20.8]
Covariates	C, U-Y	C, U-Y, S-Y, R, D·Year	C, U-Y, S-Y, R, C·Year	C, U-Y, S-Y, R, P-weights
Model in figure 4	1	2		3
R <sup>2</sup>	0.79	0.82	0.95	0.86

Notes: Models presented are weighted least-squares estimates for the DiD specification in equation 3. Weights are the 50+ county populations in 1960. C: county fixed effects. U-Y: urban by year fixed effects; S-Y: state-by-year fixed effects; R: annual, county-level covariates; D·Year: 1960 characteristics interacted with linear time trends; C·Year: county-specific linear time trends; P-weights: uses an estimate of the propensity of receiving a CHC to reweight untreated counties. See text for more details. The year prior to CHC establishment (-1) is omitted, so CHCs had been funded for only part of year 0 but for the entirety of years 1 through 14. The sample includes 2,634 counties in each specification. See figure 5 notes for details on sample and sources.

**Table 3. Heterogeneity in the Effects of Community Health Centers on Age-Adjusted Mortality Rates**

Characteristic defining stratification	(1)	(2)	(3) Per-Capita First Grant		(5) Agency Initiating	(6)	(7) Urban Share		(8)
	Real First Grant <i>Below Median</i>	First Grant <i>Above Median</i>	<i>Below Median</i>	<i>Above Median</i>	<i>DHEW</i>	<i>OEO</i>	<i>Below Median</i>	<i>Above Median</i>	
Years -6 to -2	-9.2 [12.4]	9.1 [10.0]	-4 [8.8]	8.1 [17.3]	4.5 [11.3]	-2.3 [10.2]	19.3 [18.5]	-4.4 [9.3]	
Years 0 to 4	-31.7 [15.1]	-50.3 [11.5]	-41.7 [10.9]	-32.2 [14.9]	-24.1 [11.7]	-53.1 [12.6]	0.3 [22.6]	-49.9 [10.2]	
Years 5 to 9	-47.2 [18.0]	-92.4 [18.9]	-69.8 [16.9]	-68.3 [22.4]	-41.8 [15.9]	-93 [18.9]	-16.8 [28.2]	-84.4 [16.8]	
Years 10 to 14	-32.6 [23.5]	-90.4 [23.5]	-63.3 [21.9]	-53.9 [25.5]	-34.4 [22.9]	-87.2 [23.6]	-10.8 [36.2]	-78.2 [22.4]	
R <sup>2</sup>	0.82		0.82		0.82		0.82		
Characteristic defining stratification	(9)	(10)	(11) 1960 PC MDs		(13) Dropping One Region at a Time				
	1960 AMR <i>Below Median</i>	1960 AMR <i>Above Median</i>	<i>Below Median</i>	<i>Above Median</i>	<i>NE</i>	<i>MW</i>	<i>S</i>	<i>W</i>	
Years -6 to -2	-1.9 [12.5]	1.4 [9.6]	-19.1 [18.2]	5.2 [8.9]	10 [9.0]	-4.1 [8.9]	-12.8 [9.6]	-1.3 [9.1]	
Years 0 to 4	-24.7 [11.3]	-54.9 [12.7]	-37.8 [23.4]	-43 [10.1]	-43.7 [9.8]	-39.2 [11.9]	-34.7 [11.5]	-40.6 [10.3]	
Years 5 to 9	-29.4 [16.4]	-107.8 [18.9]	-53 [28.4]	-79.2 [16.9]	-76.8 [16.8]	-60.2 [18.3]	-54.4 [16.1]	-79 [15.4]	
Years 10 to 14	-16.2 [23.5]	-108.3 [24.1]	-37.3 [36.3]	-76.6 [22.4]	-73.6 [21.4]	-48.9 [24.0]	-39.2 [21.0]	-74.2 [19.8]	
R <sup>2</sup>	0.82		0.82		0.77	0.83	0.87	0.82	

The dependent variable is the AMR. This table reports model 2 estimates of the effects of  $\tilde{\pi}_y^k$  and  $\tilde{\tau}_y^k$  obtained by replacing equation 3's component,  $\sum_{g=-2}^{-1} \tilde{\pi}_g D_j D_j^g + \sum_{g=0}^3 \tilde{\tau}_g D_j D_j^g$ , with  $\sum_k (\sum_{g=-2}^{-1} \tilde{\pi}_g^k D_j^k D_j^g + \sum_{g=0}^3 \tilde{\tau}_g^k D_j^k D_j^g)$ , where  $D_j^k$  is equal to 1 if the county received a CHC between 1965 and 1974 and belongs to group  $k$ .  $k$  is defined as the group of treated counties with real first grants below/above the median in columns (1) and (2); treated counties with real per capita first grants below/above the median in columns (3) and (4); treated counties with CHCs initiated by the DHEW/OEO in columns 5 and (6); treated counties with below/above median (in treated counties) share urban in columns (7) and (8); treated counties with below/above median (in treated counties) 1960 AMR in columns (9) and (10); and treated counties with below/above median (in treated counties) per capita MDs in columns (11) and (12). Estimates in each of these column pairs are from a single regression using 2,634 counties. Columns (13)-(16) are from separate regressions, each dropping one region from the analysis at a time as indicated in the column header, and are for 2,423, 1,691, 1,418, and 2,367 counties.

**Table 4. Changes in Age-Specific Mortality Rates with the Establishment of a Community Health Center**

	(1)	(2)	(3)	(4)
<i>A. DV: Age-Adjusted Mortality Rates, Ages 50-64</i>				
<i>Mean DV at t=-1:</i>	1482.0			
Years -6 to -2	-0.4 [7.0]	-2.9 [6.4]	-6.5 [7.1]	-3.2 [7.0]
Years 0 to 4	-3.3 [8.1]	-13.7 [6.5]	-8.3 [6.0]	-10.6 [7.5]
Years 5 to 9	-18.8 [10.0]	-31.5 [9.8]	-15.2 [7.7]	-15.2 [10.1]
Years 10 to 14	-18.6 [12.9]	-34.7 [12.9]	-8.6 [9.4]	-17.3 [11.7]
R <sup>2</sup>	0.56	0.60	0.65	0.89
<i>B. DV: Age-Adjusted Mortality Rates, Ages 65-79</i>				
<i>Mean DV at t=-1:</i>	4627.3			
Years -6 to -2	17.2 [21.0]	-7.4 [17.0]	-4.5 [18.8]	5.9 [19.9]
Years 0 to 4	-61.7 [25.2]	-65.4 [18.6]	-67.0 [17.5]	-65.4 [20.2]
Years 5 to 9	-108.3 [31.0]	-108.8 [26.2]	-110.9 [21.3]	-92.3 [31.3]
Years 10 to 14	-87.5 [38.1]	-86.5 [32.9]	-88.5 [26.8]	-84.4 [36.5]
R <sup>2</sup>	0.68	0.72	0.75	0.93
<i>C. DV: Age-Adjusted Mortality Rates, Ages 80+</i>				
<i>Mean DV at t=-1:</i>	13700.0			
Years -6 to -2	95.7 [70.7]	39.7 [64.0]	43.6 [65.0]	13.5 [79.7]
Years 0 to 4	-107.6 [78.0]	-153.0 [65.4]	-158.7 [68.9]	-82.3 [71.8]
Years 5 to 9	-185.2 [98.5]	-234.1 [81.6]	-246.2 [88.7]	-114.7 [84.0]
Years 10 to 14	-144.1 [116.4]	-200.5 [95.3]	-191.1 [116.5]	-170.0 [96.1]
R <sup>2</sup>	0.57	0.61	0.66	0.89
Covariates	C, U-Y	C, U-Y, S-Y, R, D·Year	C, U-Y, S-Y, R, C·Year	C, U-Y, S-Y, R, P-weights
Model in figure 4	1	2	3	

Notes: The dependent variable is indicated above each panel, and the mean dependent variable in treated counties in the year before CHCs were established is presented below the panel title. Models are weighted least-squares estimates in equation 3; weights are the relevant county populations in 1960. See table 3 and text for more model details. The year prior to a CHC's establishment is omitted, so CHCs had been funded for part of year 0 but for the entirety of years 1 through 14. See figure 5 notes for details on sample and sources.

**Table 5. Changes in Cause-Specific Mortality Rates with the Establishment of a Community Health Center, by Age Group**

DV Cause:	(1) Diseases of the Heart	(2) Other CVD	(3) Cancer	(4) Infectious Disease	(5) Diabetes	(6) Accident
<i>A. Age-Adjusted Mortality Rates, Ages 50-64</i>						
<i>Mean at t= -1</i>	597.6	116.3	374.9	51.8	30.4	55.1
Years -6 to -2	-1.3 [5.2]	1.5 [1.6]	-0.4 [2.8]	0.5 [1.1]	-0.8 [.8]	-1.2 [1.4]
Years 0 to 4	-7.4 [3.8]	-3.3 [1.6]	-1.5 [3.0]	-1.8 [1.0]	-1.2 [.8]	0.3 [1.4]
Years 5 to 9	-7.7 [5.4]	-6.2 [2.1]	-6.1 [3.7]	-2.2 [1.1]	-2.1 [.9]	0.4 [1.4]
Years 10 to 14	-2.1 [6.3]	-5.7 [2.2]	-8.4 [4.4]	-1.8 [1.7]	-2.4 [1.0]	-0.2 [1.4]
R <sup>2</sup>	0.60	0.51	0.10	0.24	0.09	0.16
<i>B. Age-Adjusted Mortality Rates, Ages 65-79</i>						
<i>Mean at t= -1</i>	2204.2	593.1	901.2	171.5	117.0	105.1
Years -6 to -2	7.1 [9.7]	5.6 [5.2]	-17.0 [6.3]	4.0 [3.5]	-0.3 [2.1]	-3.8 [2.2]
Years 0 to 4	-21.2 [10.8]	-12.5 [5.4]	-17.5 [6.7]	-2.2 [3.0]	-1.3 [2.1]	-3.5 [1.9]
Years 5 to 9	-37.1 [15.0]	-21.8 [6.8]	-22.7 [8.7]	-1.3 [3.8]	-1.3 [2.1]	-3.0 [2.3]
Years 10 to 14	-23.1 [19.1]	-13.8 [7.3]	-18.5 [9.8]	0.5 [4.7]	-4.5 [2.8]	-2.4 [2.5]
R <sup>2</sup>	0.73	0.69	0.22	0.23	0.19	0.19
<i>C. Age-Adjusted Mortality Rates, Ages 80+</i>						
<i>Mean at t= -1</i>	7472.5	2415.8	1422.8	699.9	214.1	367.6
Years -6 to -2	35.7 [48.1]	-1.9 [26.0]	-5.9 [15.6]	26.2 [14.9]	-3.6 [6.9]	4.2 [9.7]
Years 0 to 4	-55.4 [43.5]	-65.4 [28.9]	-17.7 [15.8]	-0.8 [13.5]	2.2 [6.1]	3.9 [8.3]
Years 5 to 9	-130.8 [56.5]	-98.3 [35.4]	-3.9 [17.9]	22.9 [18.5]	2.8 [6.7]	3.6 [12.2]
Years 10 to 14	-160.3 [72.0]	-74.5 [38.7]	-9.5 [19.5]	43.6 [21.4]	-0.4 [7.2]	12.2 [13.6]
R <sup>2</sup>	0.59	0.56	0.09	0.14	0.06	0.24

Notes: The dependent variable is the age-adjusted, age-group specific mortality rate by cause for the model 2 specification of equation 3. See table 3 notes for details on the specification, sample and sources.

**Table 6. Changes in Primary Care Use with the Establishment of a Community Health Center by Household Income**

	(1)	(2)	(3)	(4)
	1= Has Regular Source of Care	1=Any Prescription Drug Expenditures	Scheduled Visits + Hosp Admits	1=Saw Physician Last Year
<i>A. Household Income Less than 100 Percent of the Poverty Line</i>				
1970	-0.04	-0.04	-3.91	0.06
	[0.04]	[0.06]	[02.21]	[0.05]
CHC*1970	0.18	-0.22	3.50	0.03
	[0.08]	[0.11]	[4.3]	[0.1]
Observations	956	956	956	956
R <sup>2</sup>	0.16	0.22	0.19	0.12
Mean Dependent Variable in 1963 in Treated PSUs	0.77	0.58	7.35	0.67
<i>B. Household Income between 100 and 299 Percent of the Poverty Line</i>				
1970	0.00	0.00	0.55	0.06
	[0.03]	[0.03]	[0.92]	[0.03]
CHC*1970	-0.03	-0.11	-1.49	0.00
	[0.05]	[0.06]	[1.42]	[0.06]
Observations	2,076	2,076	2,076	2,076
R <sup>2</sup>	0.07	0.09	0.07	0.07
Mean Dependent Variable in 1963 in Treated PSUs	0.86	0.52	8.73	0.69
<i>C. Household Income over 300 Percent of the Poverty Line</i>				
1970	0.04	-0.04	0.60	0.06
	[0.03]	[0.05]	[02.03]	[0.05]
CHC*1970	-0.04	0.01	-0.50	0.02
	[0.04]	[0.06]	[2.36]	[0.06]
Observations	1,220	1,220	1,220	1,220
R <sup>2</sup>	0.09	0.12	0.10	0.13
Mean Dependent Variable in 1963 in Treated PSUs	0.89	0.56	7.56	0.71

Notes: See text for more details on the dependent variables. For information on specification, see equation 1. “1970” corresponds to the coefficient on  $D_t$ , and “CHC\*1970” corresponds to the coefficient on  $D_t D_j^*$ , the interaction of the dummy variable for 1970 and the dummy variable for receiving a CHC before 1970. Source: 1963 and 1970 SHSUE.

## APPENDIX A: DATA APPENDIX

### Community Health Center Data

Data on CHC grants are taken from the NACAP files and PHS reports and are validated using primary source materials (OEO 1966, OEO 1967, OEO 1968, DHEW 1972a, DHEW 1972b, Zwick 1972, GAO 1973, Health Services Administration 1974, Rudd et al. 1976). We first use the published information on CHCs in the primary source documents to identify grants in the NACAP and PHS data that fund CHCs. Second, we drop grant observations which are listed as “planning grants” either in the datasets or in the primary source materials. The remaining grants are used to construct the year in which a county *first* received a CHC program.

### Mortality Data

We construct mortality rates using Multiple Cause of Death (MCD) files for all years except 1981 and 1982, because the MCD files contain a 50% sample of deaths for some states in these years. For 1981 and 1982, we instead use the Mortality Detail files. The 1972 MCD file (and Mortality Detail file) contains a 50% sample of deaths for all states, so we multiply death counts by two in this year. All mortality rates are based on county of *residence* of the decedent. We do not include information on decedents who live outside the continental United States, and the publicly available mortality files exclude foreign military deaths.

The causes of death used in table 5 and figure B1 are based on the 33/34 cause recodes generated by NCHS. This recode as well as 3-digit International Cause of Death (ICD) codes used to define the causes examined in this paper are shown in table A1. There are two ICD revisions between 1959 and 1988, and they are incorporated into the mortality data in 1968 (7<sup>th</sup> Revision to 8<sup>th</sup> Revision) and 1979 (8<sup>th</sup> Revision to 9<sup>th</sup> Revision). Note that the causes of death we consider are not comprehensive.

### 1963 and 1970 Survey of Health Services Utilization and Expenditures (SHSUE)

The public version of these data does not contain geographic identifiers, but we obtained information on individual primary sampling units (PSUs) from NORC with the help of Cheryl Sutherland and Jeffrey Hackett. Key for this analysis is that the 1970 SHSUE re-interviewed households in the same PSUs as the 1963 survey. The 1970 SHSUE also added new PSUs, which we omit from our analysis for lack of a 1963 comparison. This restriction yields a total of 4,252 respondents over 50 for the pooled sample: 1,694 in 1963 and 2,558 in 1970. Unfortunately, PSUs do not often fall within one county, which precludes conducting this analysis at the county level. We use geographic information to identify common PSUs between 1963 and 1970 as well as link information on CHCs to PSUs. Locations are coded as having a CHC if a CHC was established in the PSU before 1970. Ideal for our analysis is that the 1963 SHSUE interviewed households before the CHC program began and the 1970 SHSUE interviewed households in the same PSUs after CHCs were operating in 17 out of the SHSUE’s 73 PSUs.

### Office of Economic Opportunity Surveys of CHC Catchment Areas

The OEO CHC surveys were conducted in Atlanta; Marshfield, Wisconsin; Washington, D.C.; Philadelphia; 5 rural counties in Montana; Charleston; East Palo Alto; San Francisco; Boston and two locations in Brooklyn. For section III.B, we construct measure of physician visit timing using questions that ask: “About how long has it been since you last talked to a medical doctor about your own health—for any kind of condition, even for a few minutes (including for a visit or a check-up)?” Responses to this question are not comparable in Atlanta and one of the Brooklyn locations, so we exclude them from our calculation. Similar to Okada and Wan, five of these surveys are also used by Freeman et al. (1982) to examine use of CHCs by different age groups. Their table 5 (p. 253), for instance, shows that 15.5 percent of those ages 46 to 64 and 14.3 percent of those 65 and older report CHCs as their usual source of care in CHC catchment areas, which is similar to our estimates using different surveys.

**Table A1. ICD Code Groups**

34 Cause Recode	1959-1967 (ICD 7)	1968-1978 (ICD 8)	1979-1988 (ICD 9)	Recode
10	1-19	10-19	10-18	Infectious Disease
20	20-29	90-97	90-97	Infectious Disease
30	30-138	Remainder of 0-136	1-9, 20-88, 98-139	Infectious Disease
50	150-159	150-159	150-159	Cancer
60	160-164	160-163	160-165	Cancer
70	170	174	174-175	Cancer
80	171-179	180-187	179-187	Cancer
90	180-181	188-189	188-189	Cancer
100	204	204-207	204-208	Cancer
110	140-148 190-203 165 205	140-149, 170-173, 190-203, 208, 209	140-149, 170-173, 190-203	Cancer
120	260	250	250	Diabetes
150	400-402 410-416	390-398	390-398	Diseases of the Heart
160	440-443	402, 404	402-404	Diseases of the Heart
170	420	410-413	410-414	Diseases of the Heart
180	421-434	420-429	415-429	Diseases of the Heart
190	444-447	400, 401, 403	401, 403	Diseases of the Heart
200	330-334	430-438	430-438	Other CVD
210	450	440	440	Diseases of the Heart
220	451-468	441-448	441-448	Other CVD
230	480-493	470-474, 480-486	480-487	Infectious Disease
330	810-835	810-825	810-825	Accidents
340	800-802 840-962	800-807, 825-949	800-807, 826-949	Accidents
370	990-999 965	980-999	980-999	Accidents

### County Codes

We re-combine all counties that split or merge after 1959. Using Forstall (1995), we make the changes noted below (not all county changes are assigned a year, and these instances contain a “-“ below).

**Table A2. Non-Virginia County Code Changes**

stfips	new_cofips	old_cofips	year	note
4	12	27	1983	La Paz County, AZ split off from Yuma county in 1983.
13	510	215	1971	The city of Columbus, GA became a consolidated city-county in 1971. Previously part of Muscogee (stfips==215).
29	186	193	-	Ste. Genevieve county, MO changed codes. Always changed to 186.
32	510	25	1969	Ormsby County (25) became Carson City (510) in 1969.
35	6	61	1981	Cibola County, NM split off from Valencia County in 1981.
46	71	131	1979	Washabaugh County was annexed to Jackson County in 1979.
55	78	83, 115	1961	Menominee split off from Shawano and Oconto Counties.

**Table A3. Virginia County Code Changes**

stfips	new_cofips	old_cofips	year	note
51	83	780	1995	South Boston City rejoins Halifax County.
51	510	13	-	Alexandria City//Arlington County
51	515	19	1968	Bedford City splits from Bedford County.
51	520	191	-	Bristol City//Washington County
51	530	163	-	Buena Vista City//Rockbridge County
51	540	3	-	Charlottesville City//Albemarle County.
51	550	129	1963	Norfolk County merges (w/ South Norfolk City) to form Chesapeake City.
51	550	785	1963	South Norfolk City merges (w/ Norfolk County) to form Chesapeake City.
51	560	75	-	Clifton Forge City//Alleghany County.
51	590	143	-	Danville City//Pittsylvania County.
51	595	81	1967	Emporia City splits from Greenville County.
51	600	59	1961	Fairfax City splits from Fairfax County.
51	620	175	1961	Franklin City splits from Southampton County.
51	630	177	-	Fredericksburg City//Spotsylvania County.
51	660	165	-	Harrisonburg City//Rockingham County.
51	670	149	-	Hopewell City//Prince George County.
51	678	163	1966	Lexington City splits from Rockbridge County.
51	680	31	-	Lynchburg City//Campbell County.
51	683	153	1975	Manassas City splits from Prince William County.
51	685	153	1975	Manassas Park City splits from Prince William County.
51	690	89	-	Martinsville City//Henry County.
51	710		-	Norfolk City came from Norfolk County, which was ultimately

				combined into Chesapeake City. Census notes that Norfolk, Portsmouth, and Chesapeake cities (and including Norfolk and South Norfolk Counties before 1963) are often combined into one group.
51	730	53	-	Petersburg City//Dinwiddie County.
51	735	199	1975	Poquoson City splits from York County.
51	740		-	Portsmouth City came from Norfolk County before it was Chesapeake City.
51	750	121	-	Radford City//Montgomery County.
51	770	161	-	Roanoke City//Roanoke County.
51	775	161	1968	Salem City splits from Roanoke County.
51	780	83	1960	South Boston City splits from Halifax County.
51	790	15	-	Staunton City//Augusta County.
51	800	123	1974	Nansemond County merges into Suffolk City.
51	810	151	1963	The rest of Princess Anne County merges into Virginia Beach City.
51	840	69	-	Winchester City//Frederick County.

We further make county changes necessary to use the SEER population data. These changes can be found here: <http://seer.cancer.gov/popdata/methods.html>.

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**APPENDIX B: ADDITIONAL TABLES AND FIGURES**

**Appendix Table B1. Services Provided by Neighborhood Health Centers as of September 1973**

	Services per Person per Year Delivered by NHC				
	Medical Care	Prescriptions	Laboratory Tests	Dental Care	X-Rays
All	2.6	2.5	1.8	0.59	0.3
<i>Predominant ethnic group<sup>1</sup> served</i>					
White	3.2	1.9	1.5	0.63	0.26
Black	2.7	2.8	1.9	0.64	0.3
Ratio, white to black	1.19	0.68	0.79	0.98	0.87
<i>Location</i>					
Urban	2.6	2.5	1.9	0.59	0.32
Rural	2.4	2.2	1.5	0.57	0.24
Ratio, urban to rural	1.08	1.14	1.27	1.04	1.33
<i>Region</i>					
Northeast	3.1	1.8	1.7	0.68	0.25
Midwest (North Central)	2.3	2.4	1.9	0.44	0.28
South	2.8	3.3	2	0.7	0.32
West	2.2	2.4	1.7	0.51	0.36

Source: Davis and Schoen (1978), table 6-2. <sup>1</sup>According to Davis and Schoen, this designates the ethnic group of the “majority of registrants.” Centers with no dominant group are excluded from calculations by race.

**Appendix Table B2. The Determinants of When Community Health Centers Were Established**

	(1)	(2)
	Dependent Variable: <i>Year NHC Grant Awarded</i>	
<hr/>		
Proportion of Residents (1960)		
in urban area	-0.06**	-0.02
	[.027]	[.014]
in rural or farm areas	-0.04	0.04
	[.075]	[.037]
under 5 years of age	0.30	0.06
	[.326]	[.221]
over 64 years of age	0.13	-0.15
	[.241]	[.138]
nonwhite	0.01	0.00
	[.032]	[.02]
with 12 years of education	0.07	0.01
	[.063]	[.043]
with less than 4 years of education	0.15	0.09
	[.121]	[.075]
in households with income <\$3,000	-0.07	-0.08
	[.106]	[.064]
in households with income >\$10,000	0.03	-0.03
	[.141]	[.089]
County Medical Resources		
Total active MDs (per 1,000 residents)	-1.04**	-0.57**
	[.474]	[.251]
Age-Adjusted Mortality Rate		
1965 level	0.001	0.00
	[.001]	[.001]
1960-1965 change	0.001	0.001
	[.002]	[.001]
<hr/>		
Weighted?	Y	N
Observations	112	112
R <sup>2</sup>	0.22	0.22
<hr/>		
<i>p</i> -value from <i>F</i> -test:		
H <sub>0</sub> : Demographics (w/o urban)=0	0.42	0.31
H <sub>0</sub> : Demographics (w/o urban) and Mortality=0	0.40	0.41

Notes: Each column reports estimates from a separate linear regression. Robust standard errors are presented in brackets. Sample: 112 counties receiving a CHC between 1965 and 1974. Any medical students is omitted, because it is highly collinear with total active MDs rendering both variables statistically insignificant. Including it does not alter our conclusions. Sources: See table 1.

**Appendix Table B3. Difference-in-Difference Estimates using a Propensity Score Trimmed Sample**

	(1)	(2)	(3)	(4)
DV:	Age-Adjusted Mortality Rate (AMR)			
<i>Mean at t=-1:</i>	<i>Mean DV in treated counties in t=-1: 3281.6</i>			
Years -6 to -2	8.0 [9.5]	1.1 [10.2]	-1.0 [18.3]	-3.0 [8.1]
Years 0 to 4	-30.6 [13.5]	-29.4 [15.1]	-59.7 [19.7]	-37.9 [8.9]
Years 5 to 9	-57.8 [17.2]	-58.2 [19.9]	-94.3 [26.5]	-61.4 [11.6]
Years 10 to 14	-50.3 [20.3]	-54.1 [20.5]	-97.5 [29.7]	-46.5 [15.4]
Covariates	C, U-Y, R-Y	C, U-Y, R-Y P-weights	C, U-Y, R-Y P-weights	C, U-Y, S-Y, R, P-weights
P-Score Trimmed Sample	No	No	Yes	No
R <sup>2</sup>	0.81	0.93	0.92	0.86
Counties	2634	2634	151	2634

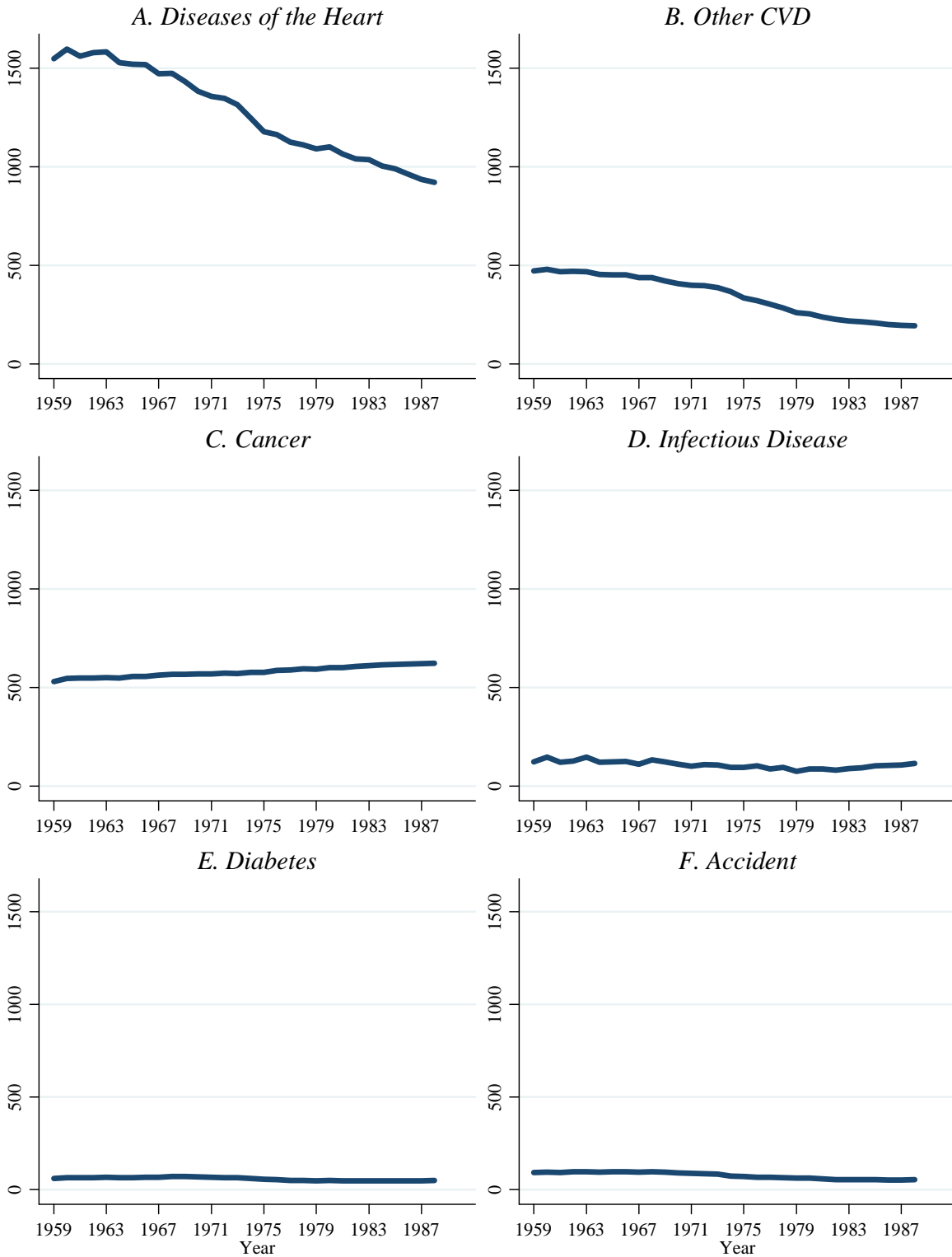
Notes: This table presents difference-in-difference estimates using a sample trimmed to use only counties with propensity-scores from 0.10 to 0.90 as suggested by Crump et al. (2009). See appendix figure B3 for the distribution of propensity score groups. Because the trimmed sample contains only 151 counties, the state-by-year effects in model 2 are not well estimated. We estimate, instead, a less demanding specification that includes census-region-by-year fixed effects rather than state-by-year fixed effects. Table B2 presents these estimates for our full sample without weighting by propensity score (column 1), our full sample using propensity-score weights (column 2), and the propensity-score trimmed sample using propensity-score weights (column 3). Column (4) reprints column (4) of table 2 presented in the paper, which shows the estimates including state-by-year fixed effects and other covariates for full sample with propensity-score weights. Trimming the sample using propensity scores almost doubles the magnitudes of the estimates.

**Appendix Table B4. Changes in Cause-Specific, Age-Adjusted Mortality Rates with the Establishment of a Community Health Center**

	(1)	(2)	(3)	(4)	(5)	(6)
DV Cause:	Diseases of the Heart	Other CVD	Cancer	Infectious Disease	Diabetes	Accidents
<i>Mean at t=-1</i>	<i>1558.5</i>	<i>413.0</i>	<i>616.0</i>	<i>131.8</i>	<i>71.0</i>	<i>90.7</i>
Years -6 to -2	3.515 [6.358]	2.532 [3.001]	-6.144 [2.877]	3.194 [2.180]	-0.819 [1.030]	-1.646 [1.595]
Years 0 to 4	-15.36 [5.862]	-10.16 [3.256]	-7.629 [3.248]	-1.873 [1.723]	-1.097 [0.982]	-0.719 [1.281]
Years 5 to 9	-25.3 [9.100]	-16.72 [4.435]	-10.93 [4.690]	-0.342 [2.416]	-1.568 [1.113]	-0.455 [1.732]
Years 10 to 14	-18.69 [11.85]	-12.05 [4.702]	-11.15 [5.377]	1.742 [3.172]	-3.044 [1.485]	-0.267 [1.994]
R <sup>2</sup>	0.818	0.788	0.283	0.341	0.226	0.362

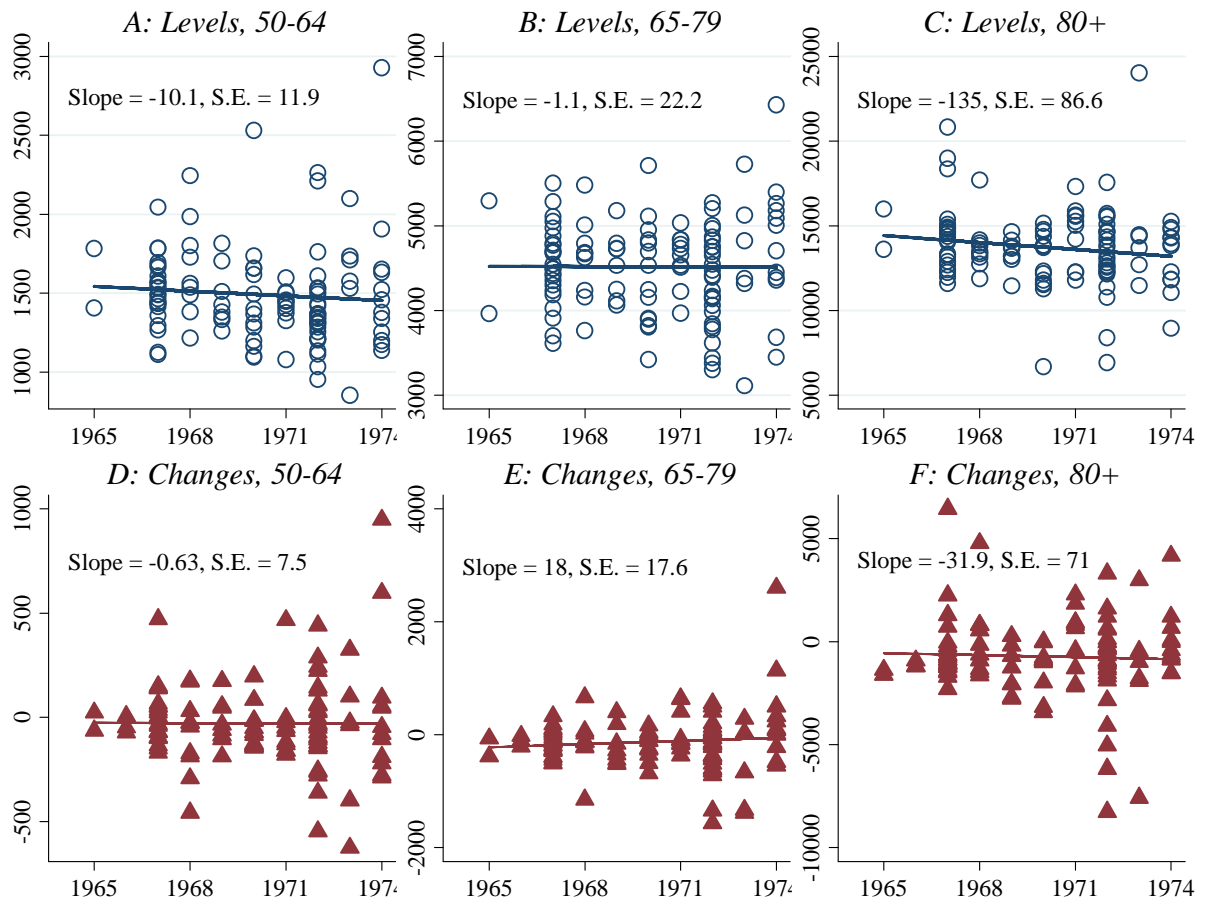
Notes: The dependent variable is the age-adjusted mortality rate for those 50 and older by cause for the DiD specification of model 2 (see equation 2). See table 2 notes for details on the specification, sample and sources.

**Appendix Figure B1. Age-Adjusted Mortality Rates by Cause of Death, 1959 to 1988**



Sources: Vital Statistics Multiple-Cause of Death Files (US DHHS 2007), 1950 and 1960 population estimates (Haines and ICPSR 2005), 1969 to 1988 population statistics (SEER 2009).

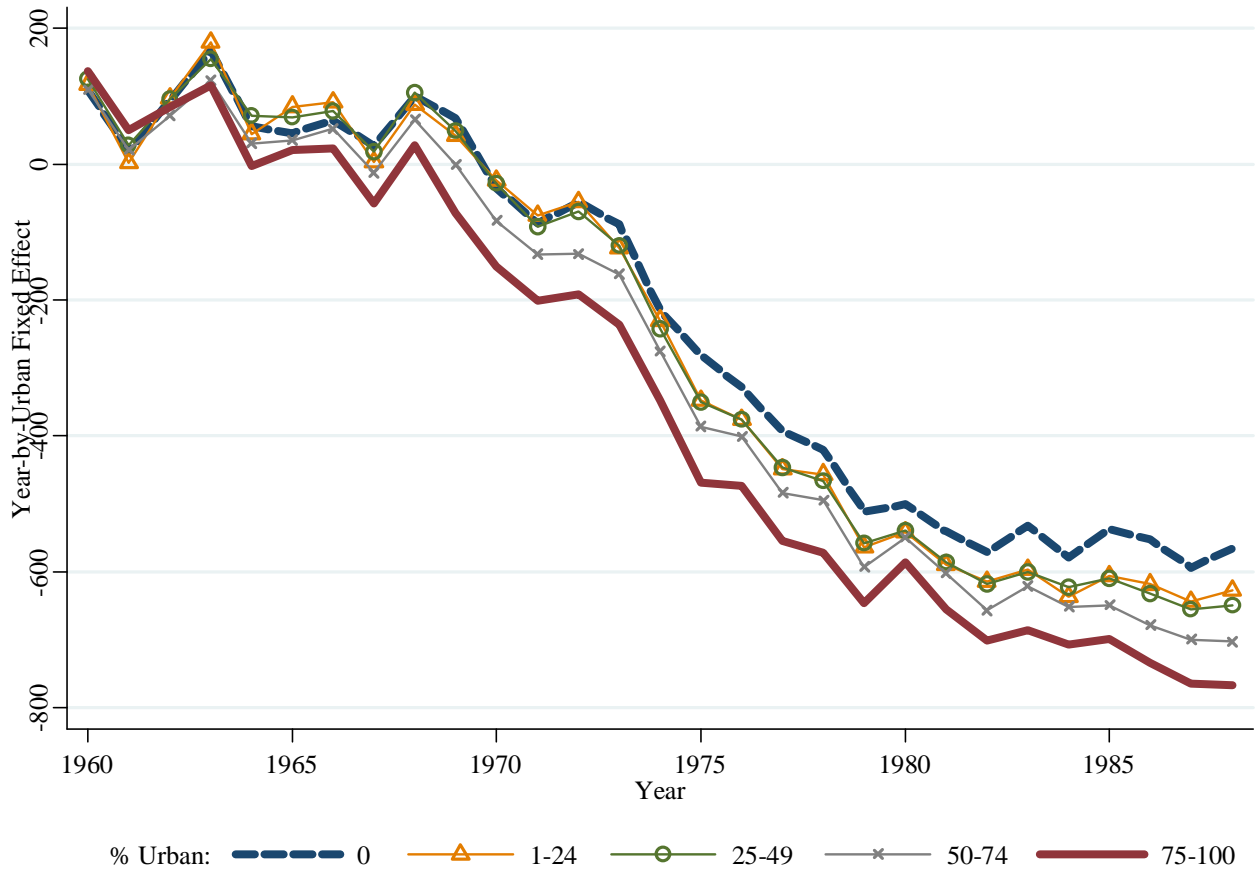
**Appendix Figure B2. Mortality Rates by Age Group, before the Community Health Center Program Began**



Notes: The dependent variable is indicated in the panel heading. Regression coefficients and predicted values are from univariate regressions of the dependent variable on mortality on the year CHCs were established for the 112 treated counties in the estimation sample. Source: See figures 1 and 2.

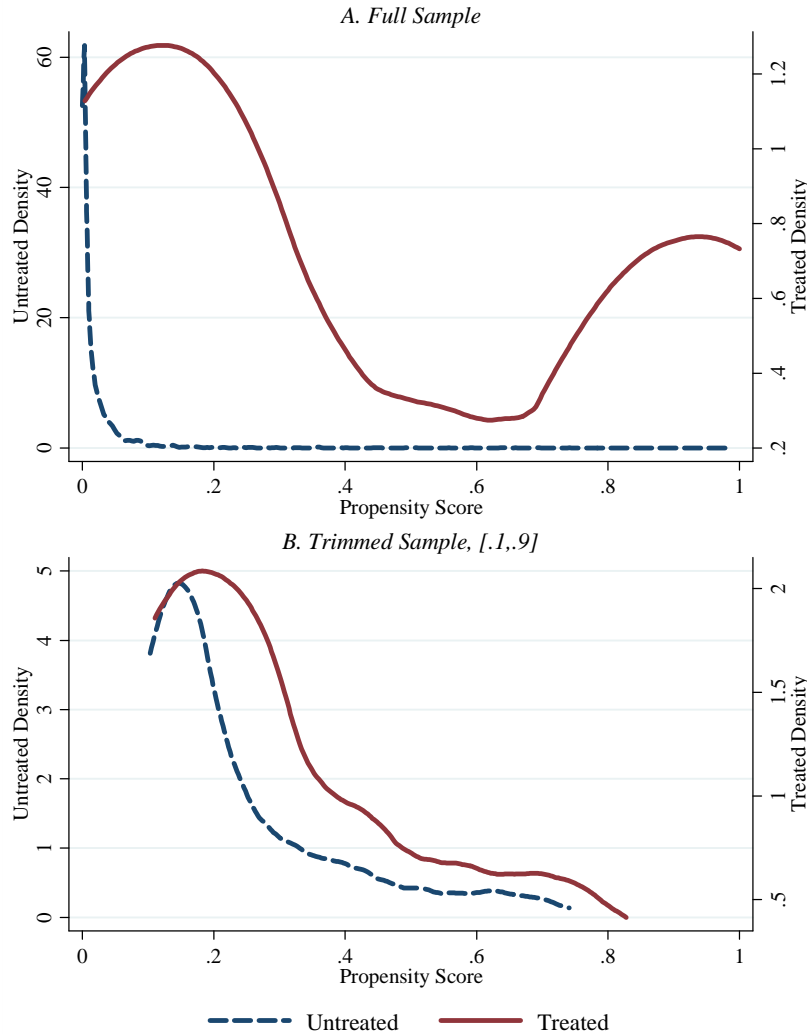
### Appendix Figure B3. Plots of Year-by-Urban Group Fixed Effects for Model 1

This figure plots the year-by-urban-group fixed effects from model 1, which correspond to the estimates in figure 4A. They show that mortality changed differentially in counties with different shares of their population in urban areas. Importantly, age-adjusted mortality rates in the most and the least urban counties diverged during our sample period.



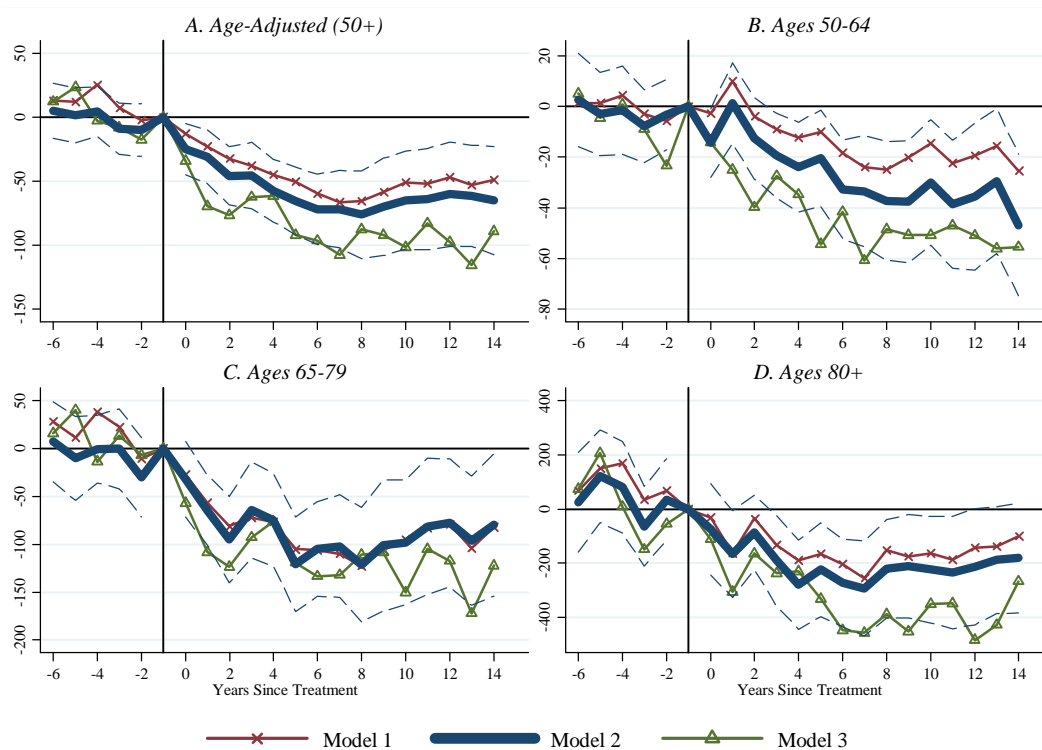
Notes: Urban groups are defined using the 1962 County Data Book, and include 0% urban, 1-24% urban, 25-49% urban, 50-74% urban and 75-100% urban. 1959 is omitted.

## Appendix Figure B4. Propensity Score Distributions



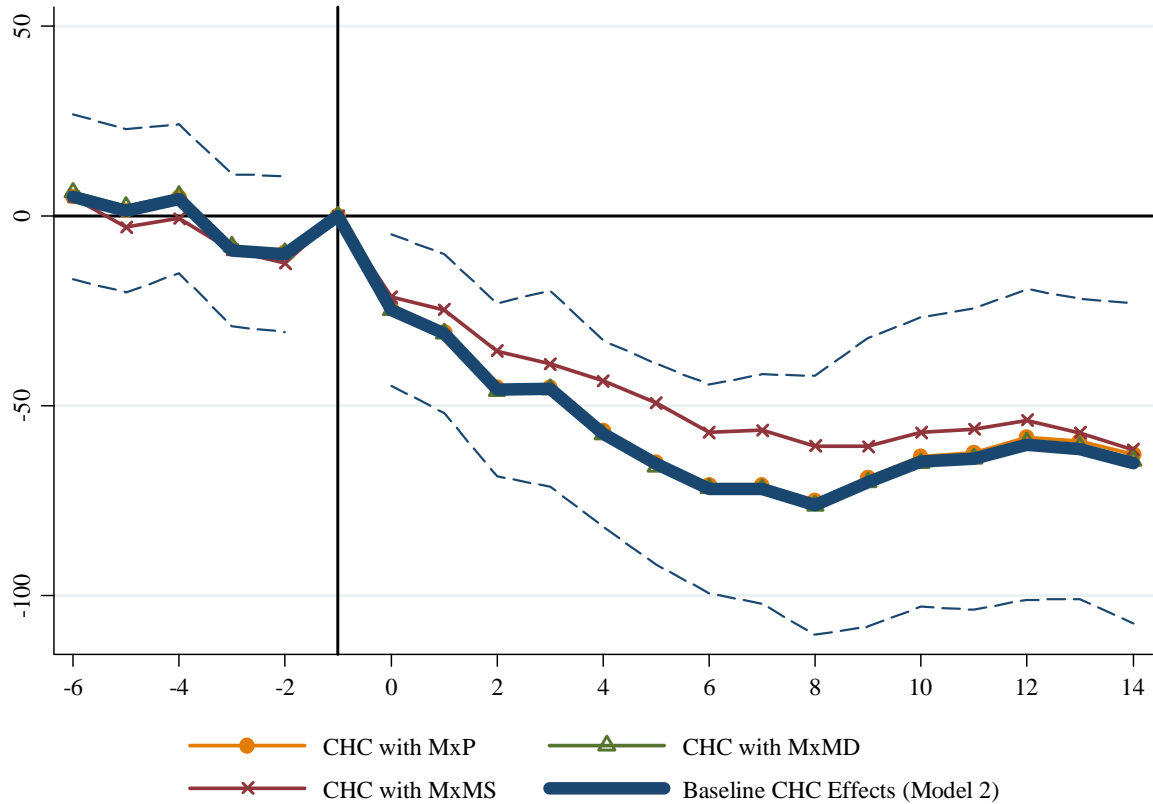
Notes: Figures show kernel density estimates using the Epanechnikov kernel for the full estimation sample (2,634 counties) and for a sample trimmed to include only propensity scores between 0.10 to 0.90 as suggested by Crump et al. (2009). The bandwidths for the untreated sample are .0026 and .0398 in the full and untrimmed samples, respectively, and for the treated sample are .1388 and .0923 in the full and trimmed samples, respectively. We construct propensity scores by estimating a probit with the binary dependent variable equal to 1 if a county received a CHC from 1965 to 1974 using the following covariates: (1) Variables measured in 1960: population density and population density squared, 1950 to 1960 population growth, percent urban, percent rural, percent nonwhite, percent of population younger than 5, percent of population older than 21, percent of population older than 65, local government expenditures per 1000 population, total housing units per 1000 population, civilian labor-force participation, fraction of housing units rented, median number of rooms per housing unit, percent of housing units with plumbing, share of housing units with a TV, share of housing units with a telephone, share of housing units with a car, the unemployment rate, share of the labor force that is male, fraction of the population 25 and older with less than 4 years of schooling, fraction of the population 25 and older with more than 12 or more years of schooling, number of MDs per 1,000 population. (2) Variables measured in 1959: median family income, fraction with family income below \$3,000, fraction with family income above \$10,000. (3) Other variables: dummy variables for the presence of a hospital in 1968 and for whether the county had a medical school in 1969. This yields estimates of the propensity of treatment,  $p_i = P(D_i=1|X_i)$ . We then reweight untreated counties using the ratio,  $p_i(1-q)/(1-p_i)q$ , where  $q$  is the fraction of individuals over 50 in locations receiving CHCs. In our regressions, we then multiply this weight by the relevant population weight.

**Appendix Figure B5. Event-Study Estimates of Changes in All-Cause Mortality with Establishment of a Community Health Center, Trimmed Sample**



Notes: Table notes and estimates from models 1 and 2 are identical to figure 4. Model 3 results only use the 151 counties with propensity score estimates between .1 and .9 (Crump, et al. 2009). In the trimmed sample, model 3 is estimated using region-by-year fixed effects instead of state-by-year fixed effects. See also appendix table B2.

**Appendix Figure B6. Changes in All-Cause, Age-Adjusted Mortality with Establishment of a Community Health Center with Controls for Medicaid Timing**



Notes: Here we present event-study estimates from model 2 of the effects of CHCs on AMR, which additionally control for local characteristics interacted with a binary variable for Medicaid start dates that vary across states. The idea behind this specification is that Medicaid may have had larger effects in places with different baseline characteristics (were poorer, had more physicians, or had a medical school). This specification controls for these potential effects of Medicaid by interacting dummy variables for years before and after Medicaid-implementation with county characteristics that may be correlated with stronger Medicaid effects. The estimates labeled *MxP* are from a specification that includes interactions of Medicaid event dummies with an indicator for 1960 poverty rates greater than 45%. The estimates labeled *MxMD* are from a specification that interacts Medicaid event dummies with an indicator for whether a county had more than the median number of active MDs in 1960. The estimates labeled *MxMS* are from a specification that interacts Medicaid event dummies with an indicator for whether or not a county contained a medical school in 1969. The baseline CHC effects for model 2 (figure 4A) are presented for comparison. The estimated effects of CHCs are similar and statistically indistinguishable in all models.