Long-Term Health and Economic Effects of Antenatal Testing Laws [℘]

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Abstract

This paper provides the first quasi-experimental analysis of the long-term impacts of mandatory antenatal testing for syphilis. Syphilis is a global public health problem, with an estimated 12 million people infected each year, including 2 million pregnant women. Mother-to-child transmission of syphilis can lead to neonatal death, preterm birth, and congenital deformities. In 1938-1947, different states in the U.S. initiated antenatal testing laws which mandated physicians and other personnel attending to pregnant women to test them for syphilis. Exploiting the variation in the timing of state antenatal testing laws, Fung and Robles (2016) find that the laws decreased neonatal mortality rates and deaths due to preterm births for nonwhites while having no discernible impact on whites. This paper follows up the initial result to study the long-term effects of mandatory antenatal testing on human capital accumulation, labor market, marriage, and crime outcomes. Using 1970 U.S. Census data, we find that treated cohorts who were exposed to antenatal testing laws in early childhood experienced *negative* long-term effects in terms of high school progression, unemployment, and underage marriage compared to control cohorts. Using data from the 1964-1980 March Current Population Surveys and the 1949-1968 Uniform Crime Reports, we find a reduction in unskilled wages and an increase in violent crime rates among treated cohorts. Our findings contribute to a growing literature that documents the link between early-life health conditions and later-life socioeconomic outcomes. We show that the long-term effects of the same health intervention in early childhood can differ significantly by race. Our results also suggest that while early childhood public health interventions are important and can have long lasting impacts, they must be coupled with complementary policies or programs that support the growth and development and eventual success of the treated individuals.

JEL Codes: I1, I12, I18, J13

Keywords: Long-term effects, human capital accumulation, unemployment, crime, antenatal care, sexually transmitted diseases, syphilis

I. Introduction

This paper provides the first quasi-experimental analysis of the long-term impacts of antenatal testing for syphilis. Syphilis is a global public health problem, with an estimated 12 million people infected each year, including 2 million pregnant women (WHO 2007). The infection can be transmitted from the mother to the fetus, leading to congenital syphilis. Left untreated, about 80% of congenital syphilis cases result in neonatal death, preterm birth, or congenital deformities (Fiumara et al. 1952; Ricci et al. 1989; Ray 1995).

In a recent paper (Fung and Robles 2016), we examine antenatal testing laws initiated in the U.S. in 1938-1947 which mandated physicians and other permitted personnel (e.g. midwives) attending to pregnant women to test them for syphilis. We use the variation in the timing of state antenatal testing laws to estimate the laws' effect on neonatal mortality rates and deaths due to preterm birth. Using 1931-1947 Vital Statistics data, we find that these laws decreased neonatal mortality rates of nonwhites by 3.15 per 1,000 live births (an 8.6% reduction) while having no discernible impact on whites. The laws contributed to an 18% narrowing of the white-nonwhite neonatal mortality gap by 1947. Using 1950 U.S. Census data, we find that mandatory antenatal testing led to a 7% increase in the cohort size of nonwhite poor.

Given the significant short-term effects of the antenatal testing laws on neonatal health outcomes, we follow up with this current study to examine the long-term effects of mandatory antenatal testing on adult outcomes, in particular, educational attainment, labor market, marriage, and crime outcomes. Our work contributes to a growing literature that documents the links between health in the fetal period and early childhood and long-term adult outcomes. A recent survey by Currie and Vogl (2013) highlights recent empirical evidence that shows that early-life shocks due to disease, malnutrition, and famine can have long-term consequences for adult health, cognition, and labor market outcomes. In particular, studies have suggested that the introduction of effective treatments for childhood disease can have significant impacts on adult health and economic outcomes. For example, Bhalotra and Venkataramani (2015) use the introduction of sulfa drugs in 1937 to examine the impacts of sulfa-driven declines in pneumonia during infancy and find significant improvements in the probabilities of high school and college completion, disability, employment, and income for men. Other studies that examine the effects of disease eradication (e.g. malaria, hookworm, and intestinal helminths) in early childhood also find significant impacts on schooling, income or other socioeconomic outcomes in adulthood (Bleakley 2010; Cutler et al. 2010; Lucas 2010; Bleakley 2007; Miguel and Kremer 2004; Ozier 2011).

When studying the long-term effects of antenatal testing, it is a priori unclear whether the population average effects would be positive, zero, or negative. Theoretically, the testing and treatment of syphilis in the mother would eliminate or lower the probability of congenital syphilis in the fetus, preventing preterm birth and congenital deformities, which in turn lead to better fetal health and development. A healthier fetus would benefit from better cognitive and physical development when growing up, which may lead to positive improvements in human capital accumulation and labor market outcomes in adulthood. On the other hand, the elimination of mortality selection may lead to *worse* population average outcomes. As neonatal mortality rates decrease as a result of antenatal testing, weaker infants are now able to survive into adulthood, causing a change in the distribution of survivors observed in their adult years. As a result, population average outcomes such as educational attainment, employment, and earnings can actually fall, or at least increase more slowly than they would otherwise have. Moreover, if the increase in cohort size due to a reduction in neonatal mortality rates is large enough to depress wages due to an increase in labor supply, we may also see a negative impact on employment and earnings, though we believe this is theoretically plausible but empirically unlikely. All said, whether the long-term socioeconomic effects of antenatal testing are positive, zero, or negative remains an empirical question. Knowing the answer to this empirical question is important in contemporary public health debates and in the setting of global health priorities, as the WHO has called for the global elimination of syphilis (WHO 2007) and many developing countries have just recently initiated policy guidelines for universal antenatal syphilis screening.

A second contribution of this paper is the testing of race differences in health and socioeconomic outcomes. Previous literature has shown that the long-term effects of the same health condition in early childhood can differ significantly by setting and by gender (Currie and Vogl 2013). We would like to add that they can also vary considerably by race and even by socioeconomic status. Given that significant black-white health gaps still exist in the U.S., it is important to understand the race differences in public health interventions.

A third and final contribution of this paper is the demonstration of unintended consequences of public health intervention programs and the importance of complementary institutions that can support the growth and development of the treated individuals. Even though the antenatal testing laws successfully prevented neonatal deaths, we find that the preserved offspring were more likely to be born into disadvantaged households and hence faced risks common to those households. Using the 1970 U.S. Census data, we find that the treated cohorts experienced *negative* long-term effects in terms of high school progression, unemployment, and underage marriage compared to the control cohorts. Using data from the 1964-1980 March Current Population Surveys and the 1949-1968 Uniform Crime Reports, we find a reduction in unskilled wages and an increase in violent crime rates among the treated cohorts. In the context of our study, it seems that the social problems arising from initial health disparities were not eliminated but simply got pushed back from the infancy period to the adolescent and adult years. Put in another way, syphilis as a public health problem seems to be associated with socioeconomic disadvantage. A remedy for one manifestation (neonatal death) of the root problem (low socioeconomic status) leads to subsequent alternative manifestation (lower educational attainment, higher unemployment, lower wage, higher crime) of the same problem. In light of our findings, we argue that while early childhood public health interventions are important and effective and can have long-lasting impacts, they are not in and of themselves a silver bullet. Early childhood interventions must be coupled with complementary policies or programs that support the growth and development and eventual success of the treated individuals.

The rest of the paper proceeds as follows. Section II discusses the background on syphilis and the antenatal testing laws as well as the effects of the laws on neonatal mortality. Section III discusses the empirical strategy to study the long-term effects of the laws. Section IV describes the data. Section V presents the results and Section VI concludes.

II. Syphilis, Antenatal Testing Laws, and Neonatal Mortality

This section gives a brief background of syphilis and antenatal testing laws that will be helpful for the reader to understand the long-term effects of the laws. More details about syphilis and antenatal testing can be found in Fung and Robles (2016).

Syphilis is a sexually transmitted disease and can be transmitted from the mother to the fetus, causing congenital syphilis. Untreated congenital syphilis can lead to neonatal death, stillbirth, preterm birth, deafness, neurologic impairment, and bone deformities (Harman 1917; Hira et al. 1990; WHO 2002; Watson-Jones et al. 2002).

Syphilis is relatively easy and inexpensive to diagnose and to treat. It is commonly diagnosed using a blood test (WHO 2007) and can be treated effectively with penicillin (Norwitz 2009). It should be noted, however, that for our period under study (around 1940s), syphilis testing and treatment were more expensive, less effective, and carried more health risks compared to the testing and treatment options today (Sartin and Perry 1995).

Syphilis first rose to epidemic proportions in the U.S. in the early 20th century (Peckham 1941). Data on syphilis rates was available beginning in 1941, and it has been shown that syphilis disproportionately affects racial and ethnic minority populations in the U.S. (CDC 2011; Robles 2013). Nonwhites have always had higher syphilis and congenital syphilis rates than whites, and such racial health disparity has not disappeared over time (CDC 1993-2010).[1]

In 1936, then U.S. Surgeon General Thomas Parran kick started a syphilis control campaign to fight the syphilis epidemic. States began adopting antenatal and premarital blood test requirements for syphilis (Shafer 1954). The antenatal testing laws mandated that “a licensed physician or other persons

[1] Before the 1980s, data on syphilis rates was collected in white versus nonwhite categories. In 1993, the black-to-white ratio of congenital syphilis rates was 56.5 (CDC 2011). Even as recently as 2011, blacks had seven times the reported syphilis rates of whites (CDC 2011).

authorized to attend to an expectant mother [be] required to take, or cause to be taken, a sample of blood of such woman, to be submitted to an approved laboratory for a standard test for syphilis within a specified time" (Halse and Liberti 1954). The premarital testing laws mandated couples seeking a marriage license to submit the results of a serological test for syphilis when applying for the license (Shafer 1954; Hedrich and Silverman 1958).

The first state to pass the antenatal testing law was New York State in March 1938. Two years later, 19 more states had passed the laws. To this day, 45 states require antenatal testing for syphilis and/or other sexually transmitted diseases (Robles 2013). The timing of the adoption of the antenatal testing laws is available from Fung and Robles (2016) and has been reproduced in Table 1 and Figure 1.

Using the variation in the timing of state antenatal testing laws, Fung and Robles (2016) estimated the laws' causal effects on neonatal mortality rates and deaths due to preterm birth by race. Using 1931-1947 National Center for Health Statistics (NCHS) Vital Statistics data, we find that the laws decreased neonatal mortality rates of nonwhites by 3.15 per 1,000 live births (an 8.6% reduction) while having no discernible impact on white neonatal mortality. As a result, the white-nonwhite neonatal mortality gap decreased by 18% by 1947. Using a separate dataset from the 1950 U.S. Census, we also find that mandatory antenatal testing led to a 7% increase in the cohort size of nonwhite poor, which is consistent with the nonwhite neonatal mortality estimates. There is no evidence of a change in the cohort size of whites.

Fung and Robles (2016) put forward two main reasons for this racial disparity in mortality outcomes, which will be important when we consider the long-term effects of the laws later on. First, syphilis prevalence has historically been higher among the nonwhite population. Second, nonwhite women may have pursued syphilis testing less frequently than white women before the antenatal testing laws were adopted, either because nonwhite women were less likely to have access to antenatal care in general (Nakashima et al. 1996; Peterman et al. 2005; Robles 2013), or because they were less likely to pay for the testing, given that there are a larger proportion of nonwhites who have lower socioeconomic status compared to whites. Since antenatal testing laws require physicians and permitted persons attending to a pregnant woman to test her for syphilis regardless of the patient's ability to pay, it is likely that those who were unable to pay, specifically nonwhites, benefit more as a result of the antenatal testing laws.

III. Empirical Strategy

We use state-year panel datasets combined with the timing of the adoption of state antenatal testing laws to study the long-term effects on adult outcomes. The baseline regression takes the following form:

$$y_{st} = \beta_0 + \gamma Antenatal\ Testing_{st} + \beta_1 Premarital\ Testing_{st} + \beta_2\ Antenatal\ Testing_{st} * Premarital\ Testing_{st} + \beta_{3s} state_s + \beta_{4t} year_t + \beta_{5st} state_s * time_t + \beta_{6st} state_s * time_t^2 + \varepsilon_{st} \quad (1)$$

where *s* indexes states and *t* indexes years. Individuals observed in their adult years are matched to their fetal exposure to antenatal testing laws based on their state and year of birth. y_{st} is the dependent variable of interest, which includes educational attainment (as measured by progression to high school), labor market outcomes (as measured by unemployment and unskilled wages), and marriage outcomes (as measured by underage marriage). $Antenatal\ Testing_{st}$ is a dummy variable which takes on the value of 1 when antenatal testing is required in state *s* for the entire year *t* and is zero otherwise. $Premarital\ Testing_{st}$ is a dummy variable which takes on the value of 1 when premarital testing is required in state *s* for the entire year *t* and is zero otherwise. The interaction of the dummy variables for antenatal and premarital testing laws accounts for the potential confounding effect of premarital testing.[2] The *state* fixed effects control for time-invariant, unobserved heterogeneity across states which may have affected the dependent variables. The *year* fixed effects control for national events which may have affected the dependent variables in any given year. *Time* is a linear trend so that the interaction terms *state * time* and *state * time*^{2} together control for a quadratic time trend for each state. These variables capture the trend in state-

[2] Specifically, the interaction term accounts for the fact that the premarital testing laws might have a preemptive effect on contagion which precluded fetal syphilitic infection, making the antenatal testing laws obsolete in those cases.

level characteristics that may affect educational attainment, employment, wages, and marriage market outcomes. The coefficient of interest, γ, is the average effect of mandatory antenatal testing. If the antenatal testing laws have a positive long-term impact, γ should be positive, and vice versa.

We estimate equation (1) separately for blacks and whites, and for men and women. Given that the antenatal testing laws had significant impacts on neonatal mortality and preterm birth for nonwhites but not for whites, we expect the long-term effects to be different for blacks and for whites. Separating the estimation by gender is motivated by findings in the literature that suggest that the long-term effects of early childhood health shocks can vary by gender, which may be caused by different biological endowments (e.g. male and female fetuses having different vulnerability to shocks) and/or different compensating investments in later life (e.g. differential parental investments in health and education for boys versus girls) (Field et al. 2009; Currie and Vogl 2013).

We limit the sample to blacks and whites born between 1931 and 1947. The 1931-1947 time frame is chosen to be consistent with our prior examination of the laws' effects on neonatal mortality. 1931 is the earliest year for which mortality data by race is available and it allows for a relatively long pre-reform period (as the first antenatal testing law was passed in 1938) which enables us to account for pre-existing trends. 1947 is chosen to avoid confounding by the introduction of penicillin in 1947 to treat syphilis and other diseases. Regressions are weighted by state-race population using state of birth. Standard errors are clustered at the state level to account for the possibility of serial correlation within a state. As noted by Bertrand et al. (2004), a failure to account for serial correlation when computing standard errors may lead to over-rejection of the null hypothesis.

After examining educational attainment, labor market and marriage outcomes, we turn to study the effects of antenatal testing on teenage crime rates. In a controversial 2001 paper, Donohue and Levitt argue that the legalization of abortion in the 1970s contributed significantly to the reduction in crime rates in the 1990s. Their line of reasoning is that legalization of abortion decreases the number of "unwanted children" being born. Since "unwanted children" are more likely to become criminals, legalizing abortion can help reduce crime. If we apply this reasoning to the current research context, then the adoption of antenatal testing laws may lead to an increase in the number of nonwhite babies born into low socioeconomic status or disadvantaged households. Indeed, teenagers, unmarried women, and the economically disadvantaged are all substantially more likely to be infected with syphilis as well as other sexually transmitted diseases (CDC 1988). Studies have found children born to these women to be at higher risk of committing crime in adolescence (Comanor and Phillips 1999).

We examine the potential effect of antenatal testing laws on crime rates by employing dynamic estimates on a state-year panel of urban crime rates from 1949-1968.[3] We modify equation (1) by adding variables for the years relative to the effective date of the antenatal testing laws:

$$y_{st} = \beta_0 + \Sigma_{k\geq 17}\, \gamma_k\, Antenatal\ Testing\ in\ effect\ for\ k\ periods_{st} \qquad (2)$$
$$+ \Sigma_{k\geq 17}\, \beta_{1k}\, Antenatal\ Testing\ in\ effect\ for\ k\ periods_{st} * Premarital\ Testing_{st}$$
$$+\beta_{2s} state_s + \beta_{3t} year_t + \beta_{4st} state_s * time_t + \beta_{5st} state_s * time_t^2 + \varepsilon_{st}$$

The $Antenatal\ Testing\ in\ effect\ for\ k\ periods_{st}$ dummies take on the value of 1 beginning 17 years after the effective dates listed in Table 1 and are zero otherwise. This means that the first treated cohort will reach 16 years of age when these dummies first take on the value of 1. Most pregnancies treated in the first effective year of the law will lead to a birth in the second effective year due to the gestation period, thus creating a need for a one-year lag in the aforementioned dummies.

The base point of observation at age 16 is chosen given the increasing propensity for individuals to commit violent crimes at that age. Figure 2 illustrates the rates of urban criminal offenses for those aged 15-24 in 1955. The figure demonstrates a pattern of increasing aggravated assault and murder offenses at age 16 and above. The compounding effect of additional birth cohorts entering the 16-24 age range will be visible in the dynamic estimates. For instance, the coefficient estimate for year 18 will show the effect for two cohorts: those first affected by mandatory testing, now reaching age 17, and those born and treated the year after, now reaching age 16. The dynamic estimates should increase with every

[3] The 1949-1968 period is chosen so that the 1931-1947 birth cohorts would be in their adolescent years.

respective year as aging birth cohorts and the propensity for crime by age compound the effect on violent crimes.

IV. Data

Information on the timing of antenatal and premarital testing laws is obtained from an editorial by the American Social Hygiene Association in the *Journal of Social Hygiene* (1948). We refer readers to Fung and Robles (2016) for more details.

We use three separate datasets for the dependent variables. The first dataset is the 1% Form 1 State sample of the 1970 U.S. Census. The 1931-1947 birth cohorts will be of ages 23-39 in the data. The data is based on a 1-in-100 national random sample of the U.S. population in 1970. The smallest geographic unit in this sample is the state. State of birth is used in our analysis but state of residence is also available. While there are two distinct forms administered in the 1970 Census, we present our results based on the sample that received Form 1. However, our preliminary results are consistent across both samples using Form 1 and Form 2. From the 1970 Census, we obtain information on progression to high school (defined as having attended the ninth grade or higher), unemployment (defined as being unemployed at the time of survey), and underage marriage (defined as having married prior to age 18). We compile the data by state and year of birth and by race and gender.

The second dataset is the 1964-1980 March Current Population Surveys (CPS), from which we obtain data on unskilled wages for individuals aged 30-34. We calculate average unskilled wages by restricting observations to those individuals with fewer than 16 years of education. We focus on unskilled wages because of the relatively low likelihood that children born to disadvantaged households will obtain four-year college degrees.[4] We choose the age range of 30-34 because we would like to study the effect on unskilled wages when the first treated cohort reached working age. The survey data was first available

[4] Although theory suggests that improved fetal and infant health may increase educational attainment, we suspect that financial limitations will prevent a number of the treated cohorts from pursuing post-secondary education.

in 1964, the year in which the first treated cohort reached age 26. We start with age 30 to allow for a four year pre-reform period of observed wages.

The third dataset is a state-year panel of crime rates obtained from the 1949-1968 Uniform Crime Reports (UCRs) produced by the U.S. Department of Justice. The 1949-1968 period is chosen so that the 1931-1947 birth cohorts would be in their adolescent years. While crime data is available in detail by criminal offense, offender race and geographic location after 1960, the data prior to 1960 is only available in summary reports. Unfortunately for this study, the data prior to 1960 is not available by race and is available only in limited geographic detail. As a result we are limited to a state-year panel of urban crime rates from 1949-1959. We extend the state-year panel through 1968 using micro-data from 1960 onwards.

Our crime data consists of two measures of violent crime: aggravated assault and murder/non-negligent manslaughter. The 1949-1957 UCRs report data on eight criminal acts: (1) murder and non-negligent manslaughter; (2) manslaughter by negligence; (3) rape; (4) robbery; (5) aggravated assault; (6) burglary/breaking or entering; (7) larceny; and (8) auto theft. The 1958-1959 UCRs do not report data on (2) and use different rules of measure for (3), (6), and (7), which prohibit a complete state-year panel on all four of the aforementioned offenses. Although (8) is reported consistently across the period we find that nonwhites were the minority of auto theft offenders. Figure 3 shows that approximately 80% of auto theft offenses were committed by whites whereas the majority cases of aggravated assault and murder/non-negligent manslaughter were committed by nonwhites during the 1953-1957 period. In light of this, we cogitate that the rate of auto theft is unlikely to be noticeably impacted by the nonwhite population increase from mandatory antenatal testing.

The pattern of robbery incidence by age hinders our ability to analyze the effect of an aging treated population using a state-year panel of aggregate robbery rates. We exploit the flat trend in the 1949-1957 urban crime rates (see Figure 4) to estimate the increased propensity for post-reform cohorts to commit crime. As illustrated in Figure 2 the incidence of robbery increases from age 15 to 17 but declines moderately thereafter. The effect from the aging of separate birth cohorts will be ambiguous as a result of this: the marginal effect from treated populations reaching ages 15-17 will pull the aggregate rate upward while the marginal effect from treated populations reaching ages 18 and beyond will pull the aggregate rate downward. This lack of a monotonic effect from an aging population hinders our ability to measure the effect of antenatal testing laws on robbery rates. On the other hand, the incidences of aggravated assault and murder/non-negligent manslaughter do increase monotonically for ages 16-24, which allow us to examine the impact on said crime rates by way of the aging treated populations.

V. Results

Tables 2 to 4 present the long-term effects of antenatal testing on propensity for progression to high school, unemployment, and underage marriage. The data comes from the 1970 Census, in which the 1931-1947 birth cohorts are aged 23-39. For these three tables, Panel A presents results for blacks and Panel B shows results for whites. From Table 2 Panel A, we see that antenatal testing laws led to a lower propensity for progression to high school (about -2% reduction) for both black men and women. There is no statistically significant impact on progression to high school for whites (Table 2 Panel B), which is unsurprising.

Table 3 Panel A shows that antenatal testing laws led to an increase in the likelihood of unemployment by 1.6-2.2% for black men. Our preferred empirical specification is Column (2), with both the state linear and quadratic trends. The 2.2% increase in unemployment likelihood is statistically significant at the 1% level and is economically large compared to the average unemployment rate of 4% for black men in the sample. There is no statistically significant impact on unemployment for black women or for whites. It is interesting to note that premarital testing laws, on the other hand, led to a decrease in unemployment likelihood of about 2-3% for blacks.

Results on underage marriage are shown in Table 4. This time, there is a statistically significant effect of antenatal testing laws on the likelihood of getting married before age 18 for black women (a 2.3-3.9% increase) but not for black men. Generally speaking, underage marriage is a more common phenomenon among women than men (22% of black women versus 6% of black men in the sample). Our preferred empirical specification is Column (4), which shows that antenatal testing led to an increase in underage marriage of 2.3% for black women, a result that is statistically significant at the 5% level and is nontrivial in terms of magnitude. We note that the coefficients for the interaction term between antenatal testing and premarital testing laws are negative and statistically significant for both black men and women. Taking Column (4) as example, it means that with premarital testing laws in place, the antenatal testing laws had a net negative effect on underage marriage for black women. Table 4 Panel B shows that there is no statistically significant effect of antenatal testing laws on underage marriage for whites, with the exception of Column (3). Antenatal testing led to an increase of 1.3% of underage marriage for white women, though this increase is only weakly significant at the 10% level.

We then turn to the unskilled wage results from the 1964-1980 March CPS. As described earlier, the state-year panel of unskilled wages is calculated as the average reported wage for individuals aged 30-34 with 15 years of education or less.[5] Table 5 Panel A shows that antenatal testing laws led to a reduction of unskilled wages by 9.3-11.1%, an effect that is both statistically and economically significant. Panel B presents a robustness check, where a four-year pre-reform trend is added to the empirical specification. The years -3 & -4 dummy takes on a value of 1 for the three and four years prior to the adoption of the antenatal testing laws, while the years -1 & -2 dummy takes on a value of 1 for the one and two years preceding law adoption. We should expect a discontinuity in the dynamic estimates for the years preceding the laws and the years that follow the law. Indeed, we see that the coefficient estimates for the pre-reform dummies are positive whereas the coefficient estimates for the antenatal testing law dummy are negative. This indicates that the initial estimates in Panel A are not simply picking up a downward trend in unskilled wages that preceded the antenatal testing laws.

We also run the same regression for skilled wages, by calculating skilled wages as the average reported wage for individuals aged 30-34 with more than 15 years of education. We find that antenatal testing laws had a negative but insignificant effect on skilled wages (results available from authors). We

[5] Only the following states are included in the analysis due to data limitations: AL; CA; CT; IA; IN; IL; FL; GA; MA; MI; MN; MO; MS; NC: NJ; NY; PA; OH; OR; SC; TX; WI.

plan to further examine the robustness of these results by examining the impact on wages at older age ranges and also breaking down the analysis by race and by gender.

Lastly we present the dynamic estimates on urban crime when the treated population reaches 12 years of age. Although we are primarily interested in the effects on crime when the treated population reaches 16 years of age because of the increasing propensity to commit crime at that age, we also want to observe the crime trend four years prior. This will allow us to separate the pre-existing trends from the dynamic response of the policy shock when the first treated population reaches 16 years of age.

Since we are examining the impact on crime rates 16 years after birth there may be a concern with identification.[6] Interstate systematic migration may bias the estimated impact on crime to the extent that individuals with lower (higher) crime propensities relocate to non-reform (reform) states 16 years after the adoption of the antenatal testing laws. We use the 1960 U.S. Census (the year 1960 is chosen as it is in the middle of our period of observation 1949-1968) to examine the propensity for treated individuals to reside in their birth state at ages 13-21. Figure 5 shows that over 85% of blacks born in reform states reside in their birth state through age 18. This proportion is even higher among disadvantaged individuals (defined as those reporting a poverty level below 100 in the 1960 Census) born in reform states. We therefore conclude that interstate migration is unlikely to affect our results.

We present the dynamic estimates for two separate crime rates in Table 6: aggravated assault and murder & non-negligent manslaughter. In each case we present the results using separate specifications. The first includes state and year fixed effects as well as state linear trends. The second allows the state trends to vary quadratically.

In the first specification (Column (1)) the dynamic estimates on aggravated assault start positive in years 12 & 13 but approach zero prior to increasing sizably and consistently when the first treated population reaches 16 years of age. In the second specification (Column (2)) the dynamic estimates start lower in years 12 & 13 and increase slightly in years 14 & 15 before increasing significantly and

[6] This discussion of the potential concern about migration bias also applies to the earlier analyses of progression to high school, unemployment, underage marriage, and unskilled wages.

consistently from year 16 onwards. Both sets of results indicate that aggravated assault rates are increasing relative to the period before the first treated population reaches ages 12 & 13. However, the distinct shift in the coefficient estimates that occurs at year 16 suggests that there is an additional force which increases the rate of growth in aggravated assault. We plot the pattern of dynamic estimates in Figure 6. The sizable upward shift in the dynamic estimates that occurs after years 14 & 15 is clearly visible while the trend prior to year 16 appears relatively flat by comparison.

The dynamic estimates on murder and non-negligent manslaughter start positive in years 12 & 13 but dip below zero in years 14 to 16 for both specifications (Columns (3) and (4)). Only in year 17 do the dynamic estimates turn positive and then consistently increase. The demographic effect from mandatory antenatal testing increases murder and non-negligent manslaughter rates a year later than it does in the case of aggravated assault. The one-year lag in the pattern of dynamic estimates reflects the fact that aggravated assault is often a precursor for murder and non-negligent manslaughter. We graph this relationship in Figure 7 by taking the data used to create Figure 2 and overlaying the aggravated assault rates on the murder and non-negligent manslaughter rates one-year forward.[7] The line graphs reveal a nearly identical pattern of crime prevalence by age with a one-year lag. The shift in the dynamic estimates occurring at year 17, and not 16, in Figure 8 similarly reflects this relationship. In sum, we see that treated populations who have been exposed to the antenatal testing laws are more likely to be involved in violent crimes, and in particular, aggravated assault as they reach their late adolescent years.

VI. Conclusion

This paper provides the first quasi-experimental analysis of the long-term impacts of mandatory antenatal testing for syphilis. Using three separate datasets, we show that treated cohorts who were exposed to antenatal testing laws in early childhood experienced *negative* long-term effects in terms of educational attainment, labor market, marriage, and crime outcomes compared to control cohorts. Using

[7] That is, murder and non-negligent manslaughter rate at age 17 is overlaid on the aggravated assault rate at age 16 and so on.

1970 Census data, we find that antenatal testing laws led to a lower propensity for progression to high school (about -2% reduction) for both black men and women; an increase in the likelihood of unemployment by 2.2% for black men; and an increase in the likelihood of underage marriage by 2.3% for black women. There are no corresponding effects for whites. Using data from the 1964-1980 March Current Population Surveys and the 1949-1968 Uniform Crime Reports, we find a reduction in unskilled wages and an increase in violent crime rates among treated cohorts.

Our findings contribute to a growing literature that documents the link between early-life health conditions and later-life socioeconomic outcomes. We show that the long-term effects of the same health intervention in early childhood can differ significantly by race (and possibly by socioeconomic status). Our results suggest that while early childhood public health interventions are important and can have long lasting impacts, they must be coupled with complementary policies or programs that support the growth and development and eventual success of the treated individuals.

In future work, we would like to study additional outcome variables, namely, high school and college completion rates as measures of educational attainment; education of spouse as a measure of marriage market outcome; and probability of being in poverty as a measure of socioeconomic outcome. We would also like to explore the mechanisms governing the link between improvements in early-life health and deterioration in later-life outcomes.

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Table 1: Timeline of Antenatal and Premarital Testing Laws for Syphilis 1936-1948

State	Antenatal Testing	Premarital Testing	State	Antenatal Testing	Premarital Testing
Alabama		1948	Nebraska	1944	1944
Arizona	1946		Nevada	1942	
Arkansas	1948		New Hampshire	1948	1939
California	1940	1940	New Jersey	1939	1939
Colorado	1940	1940	New York	1939	1939
Connecticut	1942	1936	North Carolina	1940	1940
Delaware	1940	1948	North Dakota		1940
Florida	1946	1946	Ohio	1946	1942
Georgia	1944		Oklahoma	1946	1946
Idaho	1944	1944	Oregon	1942	1939
Illinois	1940	1938	Pennsylvania	1941	1941
Indiana	1940	1941	Rhode Island	1939	1939
Iowa	1940	1942	South Carolina	1947	
Kansas	1944	1948	South Dakota	1940	1940
Kentucky	1941	1941	Tennessee		1942
Louisiana	1941		Utah	1942	1942
Maine	1940	1942	Vermont	1942	1942
Massachusetts	1940	1944	Virginia		1941
Michigan	1940	1938	Washington	1940	
Missouri	1942	1944	West Virginia	1946	1940
Montana	1946	1948	Wisconsin		1938
			Wyoming	1942	1944

Note: Above dates pertain to first full year of effective legislation. In most cases the true effective year is midway through the prior year except for states in which the effective date occured on or around January 1st of the above-stated year (IN; NJ; NC; WA) or the stated effective date was prior to the approval date (SC) in which case the latter was taken to be the true effective date. Source: *American Social Hygiene Association (1948); Fung and Robles (2016).*

Table 2: Effects of Antenatal Testing Laws on Propensity for Progression to High School

Panel A: Blacks	Progression to High School			
	Men		Women	
	(1)	(2)	(3)	(4)
Antenatal Testing	-1.85% **	-1.69% **	-2.30% **	-1.10%
	(0.006)	(0.006)	(0.006)	(0.012)
Premarital Testing	1.85%	1.70%	0.77%	1.44%
	(0.011)	(0.012)	(0.011)	(0.012)
Antenatal Testing	1.57%	1.80%	1.50%	0.55%
x Premarital Testing	(0.013)	(0.015)	(0.012)	(0.016)
State Linear Trends	Yes	Yes	Yes	Yes
State Quadratic Trends	No	Yes	No	Yes
Average	0.93		0.96	
Observations	17,813	17,813	22,215	22,215

Panel B: Whites	Progression to High School			
	Men		Women	
	(1)	(2)	(3)	(4)
Antenatal Testing	0.03%	0.30%	-0.20%	-0.03%
	(0.002)	(0.002)	(0.002)	(0.001)
Premarital Testing	-0.06%	0.60%	-0.17%	-0.10%
	(0.002)	(0.002)	(0.001)	(0.001)
Antenatal Testing	-0.08% +	-0.30%	0.32% +	0.08%
x Premarital Testing	(0.003)	(0.003)	(0.002)	(0.002)
State Linear Trends	Yes	Yes	Yes	Yes
State Quadratic Trends	No	Yes	No	Yes
Average	0.98		0.99	
Observations	162,884	162,884	167,270	167,270

Note: Data comes from the 1970 U.S. Census. Sample is limited to blacks and whites born between and 1947 within the U.S. Progression to high school is defined as having attended the 9th grade or h Regressions are weighted by state-race population using state of birth. Regressions include birth-sta and birth-year fixed effects as well as state linear trends. Standard errors are clustered by state and reported in parentheses. Statistical significance is indicated by + 10%; * 5%; ** 1%.

Table 3: Effects of Antenatal Testing Laws on Unemployment

Panel A: Blacks	Unemployment							
	Men				Women			
	(1)		(2)		(3)		(4)	
Antenatal Testing	1.60%	*	2.20%	**	0.37%		0.20%	
	(0.007)		(0.005)		(0.010)		(0.006)	
Premarital Testing	-2.30%	+	-3.20%	**	-2.14%	**	-1.90%	
	(0.013)		(0.010)		(0.006)		(0.006)	
Antenatal Testing	1.56%		2.00%		0.86%		1.10%	
x Premarital Testing	(0.016)		(0.015)		(0.010)		(0.015)	
State Linear Trends	Yes		Yes		Yes		Yes	
State Quadratic Trends	No		Yes		No		Yes	
Average	0.04				0.05			
Observations	17,813		17,813		22,215		22,215	

Panel B: Whites	Unemployment							
	Men				Women			
	(1)		(2)		(3)		(4)	
Antenatal Testing	-0.06%		0.09%		0.31%		0.40%	
	(0.002)		(0.003)		(0.003)		(0.004)	
Premarital Testing	0.14%		0.20%		-0.04%		-0.10%	
	(0.002)		(0.002)		(0.002)		(0.002)	
Antenatal Testing	-0.45%		-0.50%	+	-0.06%	+	-0.50%	
x Premarital Testing	(0.003)		(0.003)		(0.003)		0.004	
State Linear Trends	Yes		Yes		Yes		Yes	
State Quadratic Trends	No		Yes		No		Yes	
Average	0.03				0.02			
Observations	162,884		162,884		167,270		167,270	

Note: Data comes from the 1970 U.S. Census. Sample is limited to blacks and whites born between 1931 and 1947 within the U.S. Unemployment is defined as being unemployed at th time of survey. Regressions are weighted by state-race population using state of birth. Regressions include birth-state and birth-year fixed effects as well as state linear trends. Stan errors are clustered by state and reported in parentheses. Statistical significance is indicated 10%; * 5%; ** 1%.

Table 4: Effects of Antenatal Testing Laws on Underage Marriage

Panel A: Blacks	Underage Marriage			
	Men		Women	
	(1)	(2)	(3)	(4)
Antenatal Testing	1.40%	0.36%	3.93% **	2.32% *
	(0.009)	(0.010)	(0.014)	(0.010)
Premarital Testing	1.30%	0.70%	2.30%	1.90%
	(0.012)	(0.013)	(0.016)	(0.019)
Antenatal Testing	-3.71% *	-3.00% +	-7.64% **	-7.30% **
x Premarital Testing	(0.015)	(0.017)	(0.023)	(0.023)
State Linear Trends	Yes	Yes	Yes	Yes
State Quadratic Trends	No	Yes	No	Yes
Average	0.06		0.22	
Observations	17,813	17,813	22,215	22,215

Panel B: Whites	Underage Marriage			
	Men		Women	
	(1)	(2)	(3)	(4)
Antenatal Testing	-0.19%	-0.46%	1.29% +	1.19%
	(0.003)	(0.004)	(0.007)	(0.009)
Premarital Testing	-0.38%	-0.47%	0.08%	0.13%
	(0.003)	(0.003)	(0.004)	(0.004)
Antenatal Testing	0.74% +	0.96% *	-1.55% +	-1.20%
x Premarital Testing	(0.004)	(0.005)	(0.008)	(0.009)
State Linear Trends	Yes	Yes	Yes	Yes
State Quadratic Trends	No	Yes	No	Yes
Average	0.04		0.18	
Observations	162,884	162,884	167,270	167,270

Note: Data comes from the 1970 U.S. Census. Sample is limited to blacks and whites born between 1931 and 1947 within the U.S. Underage marriage is defined as having married prior to age 18. Regressions are weighted by state-race population using state of birth. Regressions include birth-state and birth-year fixed effects as well as state linear trends. Standard errors are clustered by state and reported in parentheses. Statistical significance is indicated by + 10%; * 5%; ** 1%.

Table 5: Effects of Antenatal Testing Laws on Unskilled Ln(Wages)

	(1)	(2)	(3)
Panel A: Average effects			
Antenatal Testing	-11.1% **	-9.3% **	-10.1% *
	(0.03)	(0.02)	(0.04)
Premarital Testing	-6.0% **	-6.4% *	-5.6%
	(0.02)	(0.03)	(0.04)
Antenatal Testing	9.2% *	14.4% **	14.4% +
x Premarital Testing	(0.04)	(0.04)	(0.07)
Adjusted R^2	0.96	0.97	0.96
Observations		255	
Years		1964-1980	
Panel B: Dynamic estimates			
Years -3 & -4	3.1% **	3.2% +	4.4%
	(0.01)	(0.02)	(0.03)
Years -1 & -2	1.0%	1.6%	3.4%
	(0.03)	(0.04)	(0.05)
Antenatal Testing	-10.5% **	-8.4% **	-7.4%
	(0.03)	(0.02)	(0.05)
Premarital Testing	-5.7% *	-6.2% *	-4.8%
	(0.02)	(0.02)	(0.03)
Antenatal Testing	9.8% *	15.4% **	15.6% *
x Premarital Testing	(0.05)	(0.05)	(0.07)
Adjusted R^2	0.97	0.97	0.97
Observations		255	
Years		1964-1980	
Year Fixed Effects	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes
State Linear Trends	No	Yes	Yes
State Quadratic Trends	No	No	Yes

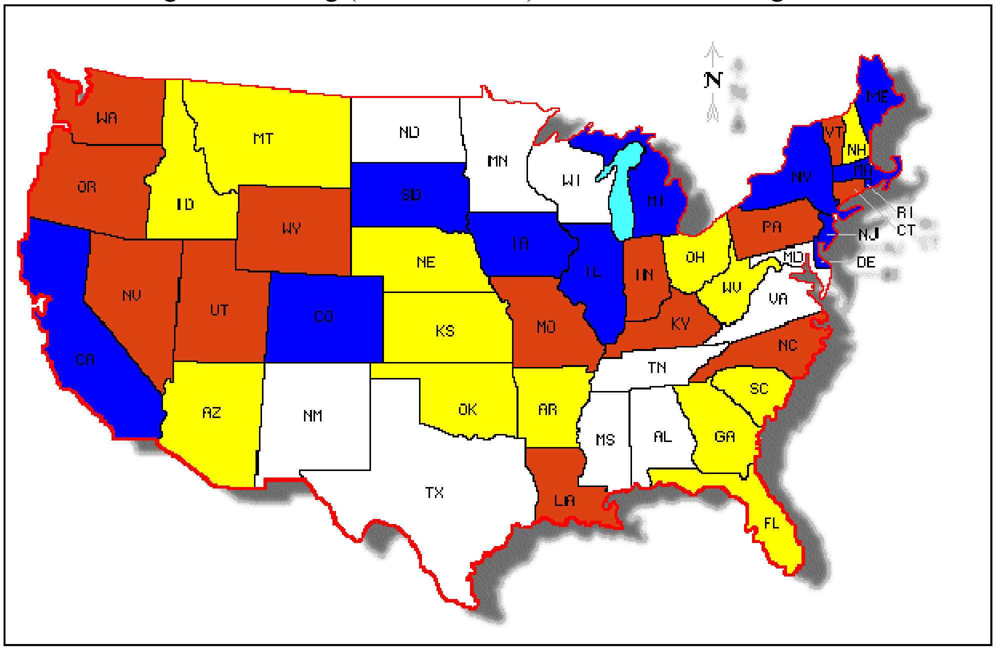
Note: Data comes from the 1964-1980 March Current Population Surveys. The state-year panel of unskilled wages is calculated as the average reported wage for individuals aged 30-34 with 15 years of education or less. Only the following states are included in the analysis due to data limitations: AL; CA; CT; IA; IN; IL; FL; GA; MA; MI; MN; MO; MS; NC: NJ; NY; PA; OH; OR; SC; TX; WI. Regressions are unweighted. Standard errors are clustered by state and reported in parentheses. Statistical significance is indicated by + 10%; * 5%; **1%.

Table 6: Effects of Antenatal Testing Laws on Crime

	Aggravated Assault		Murder & Non-negligent Manslaughter	
	(1)	(2)	(3)	(4)
Year 12 & Year 13	3.40	2.03	0.24 +	0.19
	(2.95)	(3.02)	(0.11)	(0.10)
Year 14 & Year 15	0.30	3.03	-0.03	-0.15
	(3.70)	(2.74)	(0.14)	(0.12)
Year 16	7.01	9.30 *	-0.01	-0.35 +
	(7.40)	(4.63)	(0.30)	(0.20)
Year 17	10.80	17.52 *	0.40	0.15
	(10.35)	(8.08)	(0.53)	(0.44)
Year 18	12.09	20.70 +	0.52	0.21
	(13.13)	(11.38)	(0.53)	(0.40)
Year 19+	24.32 *	33.22 *	1.34	0.46
	(12.19)	(14.46)	(1.11)	(0.61)
Year 16 x Premarital	-5.75	-0.74	-0.16	0.15
Testing	(12.42)	(11.03)	(0.29)	(0.23)
Year 17 x Premarital	-2.55	-3.07	-0.32	-0.35
Testing	(11.09)	(7.12)	(0.45)	(0.33)
Year 18 x Premarital	-2.90	-1.46	-0.61	-0.52
Testing	(11.27)	(7.82)	(0.41)	(0.32)
Year 19+ x Premarital	-14.45	-4.51	-1.38	-0.46
Testing	(8.93)	(8.94)	(0.89)	(0.37)
Adjusted R^2	0.90	0.87	0.92	0.94
Mean	72.16 per 100,000		4.56 per 100,000	
Observations	833		901	
Years	1949-1968		1949-1967	
Year Fixed Effects	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes
State Linear Trends	Yes	Yes	Yes	Yes
State Quadratic Trends	No	Yes	No	Yes

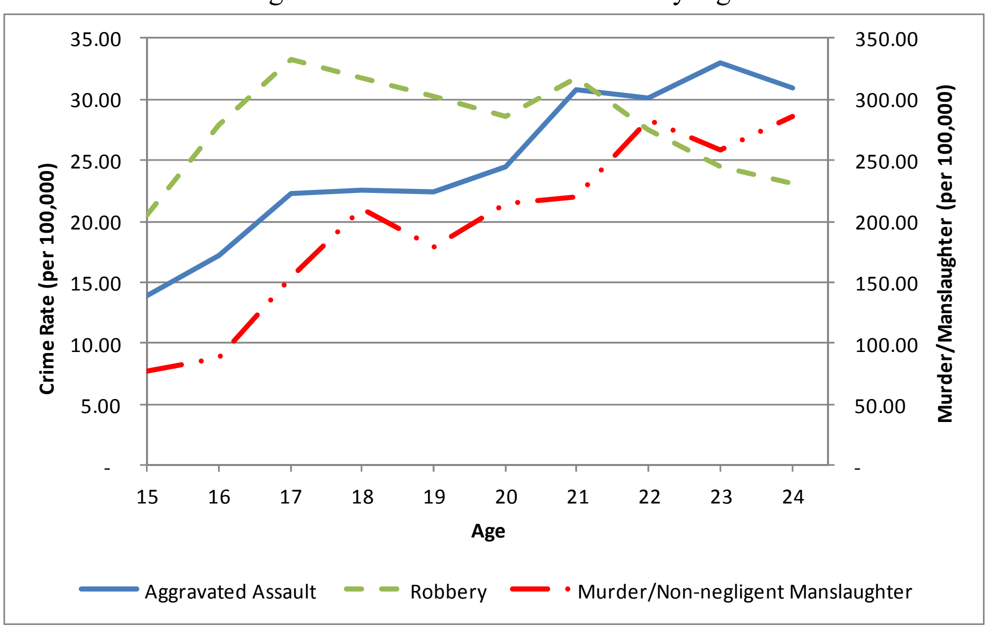
Note: Data comes from 1949-1968 Uniform Crime Reports. Standard errors are clustered by state and reported in parentheses. Statistical significance is indicated by + 10%; * 5%; **1%.

Figure 1: Timing (Effective Date) of Antenatal Testing Laws



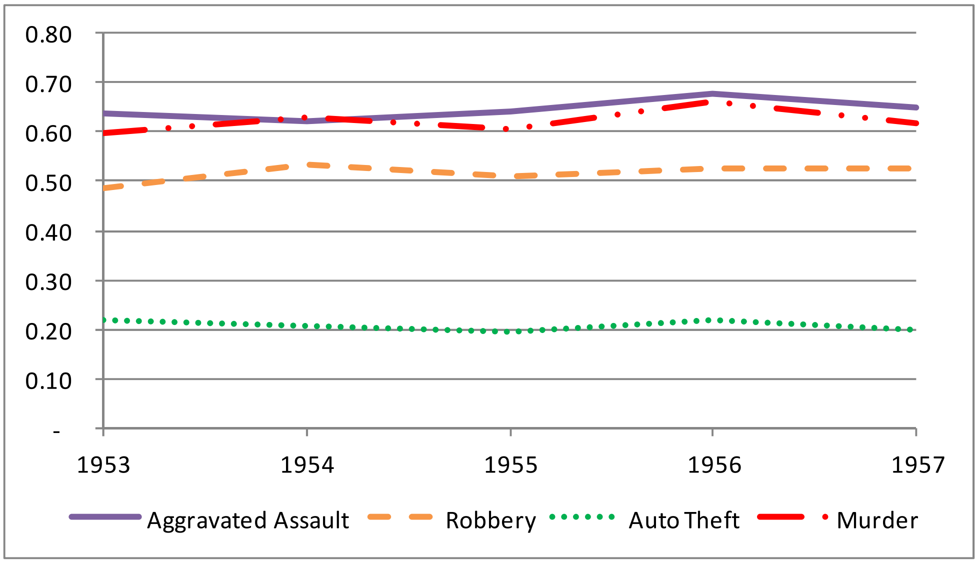
Group I [Blue]: 1938-1939; CA, CO, DE, IA, IL, MA, ME, MI, NJ, NY, OK, RI, SD
Group II [Orange]: 1940-1941; CT, IN, KY, LA, MO, NC, NV, OR, PA, UT, VT, WA, WY
Group III [Yellow]: 1943-1947; AZ, AR, FL, GA, ID, KS, MT, NE, NH, OH, OK, SC, WV
Group IV [White]: Post-1947 Unknown Date; AL, MD, MN, MS, ND, NM, WI, VA, TN, TX
Source: Table 1 for more detailed information.

Figure 2: 1955 Urban Crime Rates by Age



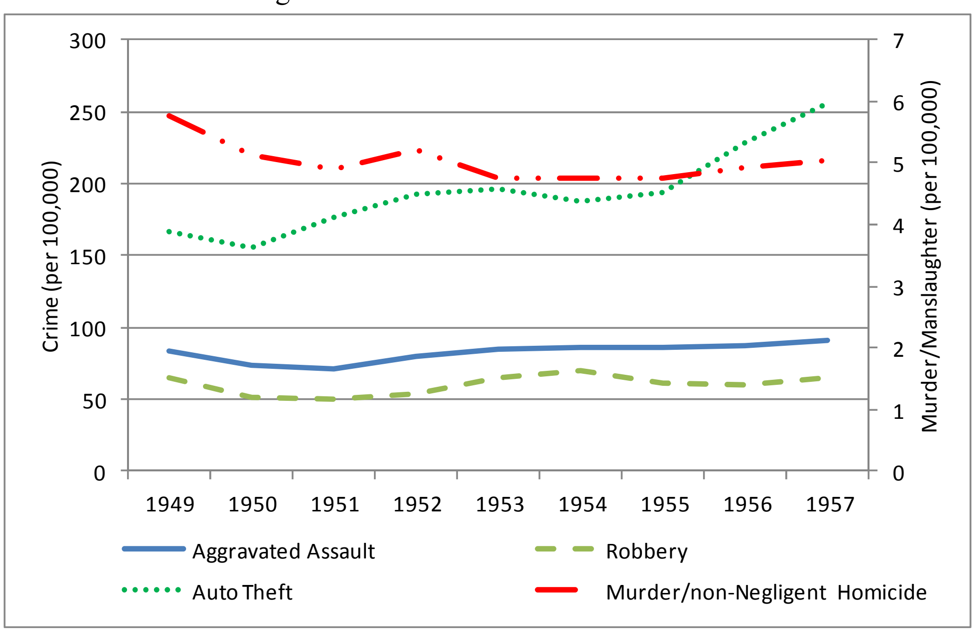
Source: Uniform Crime Reports 1955.

Figure 3: 1953-1957 Fraction of Urban Crime Rates by Nonwhite Offenders



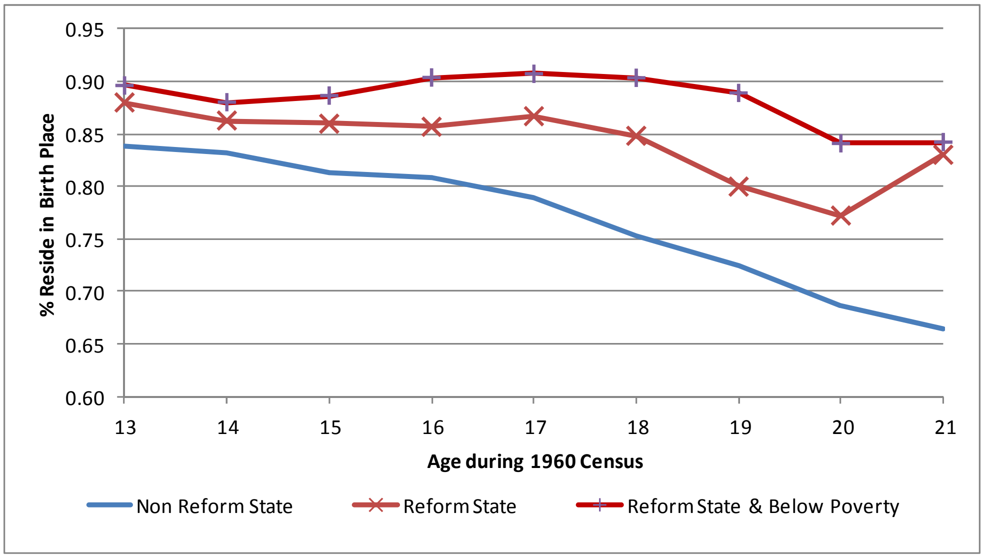
Source: Uniform Crime Reports 1953-1957.

Figure 4: Urban Crime Rates 1949-1957



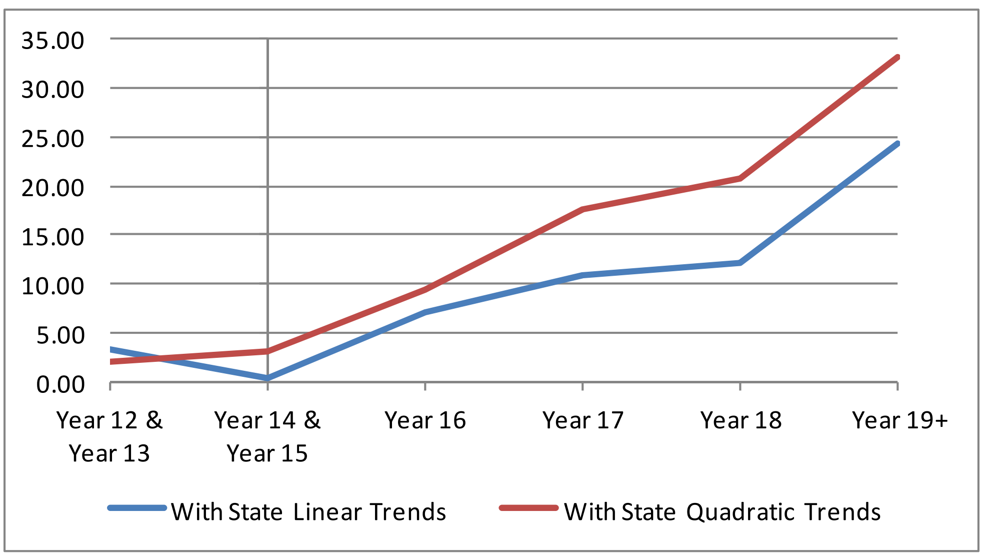
Source: Uniform Crime Reports 1949-1957.

Figure 5: Percent of Blacks Who Reside in Birth State



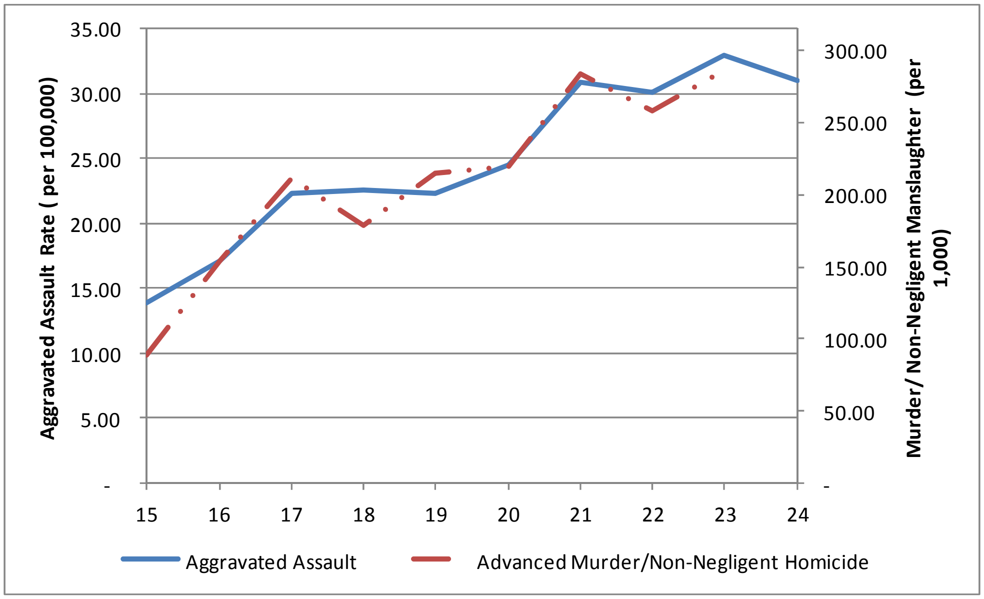
Source: 1960 U.S. Census.

Figure 6: Dynamic Estimates for Aggravated Assault Rates by Age of First Treated Population



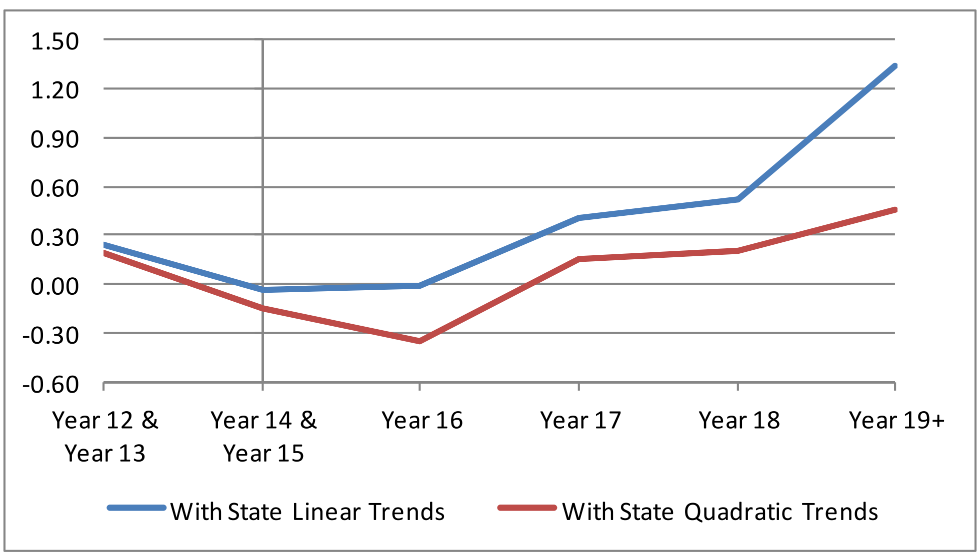
Source: Table 6.

Figure 7: Aggravated Assault and Advanced Murder/Non-Negligent Manslaughter Rates by Age



Source: Uniform Crime Report 1947.

Figure 8: Dynamic Estimates for Murder & Non-Negligent Manslaughter Rates by Age of First Treated Population



Source: Table 6.