Incidence and Cumulative Incidence of Incarceration in the United States

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1. Introduction

Mass incarceration is acknowledged as a social problem by both the political left and right (Dagan & Teles, 2016). This unusual consensus stems from skepticism about whether, at the margin, the social benefits from imprisonment eclipse the social and personal costs of incarceration. Although the costs of incarceration are difficult to compute, commonly used social indicators are the proportion of Americans who enter prison and whether those proportions vary across social groupings. For instance, there is a concern that too many Americans enter prison and that the incidence of prison is especially disadvantageous for blacks.[1] Consequently, the level and trends in prison admissions, by social groupings, is a pressing policy concern. As discussed subsequently, currently available estimates come from surveys that are expensive to administer, that suffer from large sampling and non-sampling errors, and that perform poorly when tracking trends.

This paper introduces a new methodology for estimating the incidence and cumulative incidence of imprisonment in the United States. Intended as a complement to the survey-based approach, the new methodology uses administrative data that the Bureau of Justice Statistics (BJS) routinely assembles for the National Corrections Reporting Program (NCRP), and hence the marginal cost of annually performing and reporting the estimates are negligible. The new methodology is especially useful for identifying trends.

[1] Throughout this paper, we use the term "black" instead of African-American. The reason is that the racial category will encompass a population category that is of African heritage but whose members are not necessarily American citizens.

Incidence is the rate of first-time admissions to prison for members of an *age cohort*; thus, it is a function of the year when members of that cohort were born and the age when they first entered prison. The observation that "2 percent of the 1975 age cohort entered prison for the first time at age 18" is an example of incidence. Cumulative incidence is the sum over time of first-time admissions; thus, it reflects the percentage of an age cohort who ever entered prison as of a given age. The observation that "6 percent of the 1975 age cohort entered prison at least once by age 30" is an example of cumulative incidence. The definition of age cohort is subtle because it accounts for movement across states and immigration and emigration across U.S. borders. Prison is a state or federal prison and excludes jails. It also excludes confinement in juvenile facilities because juvenile confinement is not reported to the NCRP.

The NCRP is a BJS program that assembles adult prison term records (when an offender entered prison and when he or she exited prison) over an observation window. The observation window begins in 2000 or earlier in many states and later in other states. All offenders who served any time in state prison during the observation window are captured by the NCRP. The NCRP is updated yearly. While all states report to the NCRP, the NCRP project team cannot construct reliable term records for all states. Imputations (discussed in this paper) are increasingly important as reporting windows shorten and, of course, for states that fail to provide reliable term records. This paper explains estimation for states that report for long windows, for states that report for shorter windows, and for states that do not provide reliable term records. Our goal is to produce national estimates.

This paper first elaborates on the definitions that are already introduced. The elaboration is necessary because the definition of age cohort is subtle. The paper then contrasts this new method with the complementary survey-based method. To build the argument, the paper begins

with estimates for a single state (Georgia), adapts the estimates to include eighteen states with lengthy observation windows, and finally extends the estimates to include all states. The paper ends with a discussion of bias and recommendations for improvements.

2. Definitions

Much of the language and concepts of criminal justice contacts comes from the discipline of epidemiology. Using epidemiological terminology, we distinguish among *incidence, point prevalence* and *cumulative incidence*. This last measure is sometimes called period prevalence *(*Jekel, Katz, Elmore and Wild, 2007*)*. Different epidemiological sources use somewhat different terminology, so we will be specific about the definitions used in this paper.

Interpretation of these statistics depends on the pool of people at risk of incarceration. Most survey-based estimates characterize the at-risk pool using the concept of a *birth cohort*. For example, the 1982 United States birth cohort comprises everyone born in the United States during 1982. However, the NCRP data do not identify individuals' places of birth. NCRP-based estimates therefore define the risk pool using the concept of an *age cohort*. The 1982 age cohort for a specific state comprises everyone born during 1982 and currently living in that state. Seemingly clear, the definition of age cohort is actually fuzzy. In a nation of foreign immigrants, an age cohort is not the same as a birth cohort. For the United States, age cohorts may grow over time until death rates ultimately dominate net immigration rates, so prison burden applies to an ever-changing population.

We will discuss accommodations for this fuzziness in the methodology section, but for now, note that survey-based estimates pertain to U.S. birth cohorts while NCRP-based estimates pertain to age cohorts. Differences in the populations being studied will require subtle differences in interpretation.

When there is no need to distinguish between an age cohort and a birth cohort, we will reference a *cohort* by date of birth.

For our purposes, *incidence* is the number of members of a cohort admitted to prison for the first time at a specified age. Thus, members of the 1982 cohort could only begin to enter prison in 2000, when the first cohort members began to turn 18, and in theory first-time admissions could continue to occur until all members of the cohort have died. *Cumulative incidence* is the sum of incidence over years. For example, presuming for simplicity that the size of the cohort is constant over time, if 100 members of the 1982 cohort entered prison for the first time in 2000, and if 200 more entered prison for the first time in 2001, and if 300 more entered prison for the first-time in 2002, then as of 2002 the cumulative incidence for the 1982 age cohort would be 600. Later in this paper, we will show calculations of cumulative incidence over different periods including ages 18 to 33 and 18 to 70.

Point prevalence is the number of people associated with a condition at a specific point in time. When epidemiologists use this measure to study disease, a person having had the disease who had been cured would not be counted among the people having the disease at a later point in time. For our purposes, a counterpart might be to study the proportion of a cohort who are incarcerated at a point in time, but that topic is not studied here, because it requires a straightforward tabulation of NCRP data.

Returning to the distinction between survey-based estimates and NCRP-based estimates, in both cases, converting from raw rates to proportions requires dividing a numerator (first-time prison admissions) by a denominator (people at risk). In this regard, the survey-based method and the new NCRP-based method differ regarding what data are included in the numerator and denominator. Consider the survey-based method. The numerator is the number of first-time admissions for a birth

cohort. Some admissions are excluded from the numerator because the offenders were born outside the United States so they are not part of the calculations. Likewise, the denominator is restricted to members of the birth cohort alive at a given year and not previously admitted to prison. This calculation produces what statisticians call a *hazard*. The hazard can be converted into what statisticians call a survival function, and ultimately into a failure function, which is one minus the survival function. The failure function is analogous to the cumulative incidence, not for an age cohort, but rather for a birth cohort.

Now consider the NCRP-based estimates. The numerator is measured as the number of admissions to prison for an age cohort, some of whose members were not born in the United States. The denominator is the size of this age cohort. Deaths cause the size of this age cohort to change over time, but until cohort members age considerably, the size of the cohort is more sensitive to immigration and (to a lesser extent) emigration. The calculation is not a hazard and the cumulative incidence is not a properly constructed survival function, although as we will discuss subsequently, the cumulative incidence approximates a survival function.

The survey-based approach and the NCRP-based approach are estimating different things. The survey-based approach seems conceptually clearer, but the NCRP-based approach is more comprehensive, because the NCRP-based approach includes prison admissions for everyone, not just U.S. citizens. Despite these differences, and discounting measurement errors, we will argue that the two approaches produce similar estimates of incidence rates provided immigrants and emigrants share the same risk of incarceration as do native-born Americans. To the extent that they come from different risk pools, the difference in the estimate will vary with the size of the immigration pool relative to the native-born pool.

Simple refinements of these definitions are straightforward and useful. Cohorts can be stratified by race or gender, and the relative prison burden placed on blacks and whites, or on men and women, can be compared. Another refinement is to report statistics as raw numbers (i.e., 10,000 men have experienced prison as of 2015) or as rates (i.e., 2% of all people living in the United States have experienced prison as of 2015). This paper reports rates.

3. Prior Studies of Incidence and Prevalence

Despite the importance of estimating incidence or cumulative incidence of incarceration, no one source allows investigators to simply count either form of incidence. This is not surprising given the fragmentation of the U.S. criminal justice system. Ignoring jail incarceration, there are 51 systems of imprisonment in the United States (50 state systems, and a federal system including the District of Columbia). To support computations of lifetime cumulative incidence, an administrative record of imprisonment must combine incidents of imprisonment over time beginning with very old age cohorts.

Given this limitation, there have been several attempts to estimate criminal justice contacts including incarceration by using survey samples and available administrative records. A recent paper by Shannon, Uggen, Thompson, Schnittker, and Massoglia (2016) estimates the total ex-felon population (e.g. convicted of a felony although not necessarily sentenced to prison) as of 2010 at about 19.8 million people, or 8.6% of the adult population. More useful for this paper they also estimate that there were 7.7 million current or ex-prisoners, which is 3.4% of the adult population. A prior report by Uggen, Manza, and Thompson (2006) provides methodological detail.

> Our estimates of the number of ex-prisoners in the United States are based on the number exiting prisons each year (including those conditionally released to parole) since 1948. In addition to these former prisoners, our estimates of ex-felons include those felons leaving probation supervision and jails each year. Using demographic life tables, we compute the number of these former felons lost to recidivism and mortality annually. Based on national studies of probationer and parolee recidivism, we assume that most ex-prisoners will ultimately return to prison and that a smaller percentage of ex-probationers and jail inmates will cycle back through the justice system. We further assume a much higher mortality rate among felons relative to the non-felon population. Both groups are removed from the ex-felon pool—the recidivists because they would already be counted among the "current" felon population, and the deaths because they are permanently removed from the population. Each existing release cohort is thus successively reduced each year and joined by a new cohort of releasees, allowing us to compute the number of ex-felons no longer under supervision in each year. (Uggen et al., 2006, p. 284)

Uggen et al. (2006) and Shannon et al. (2016) use mostly BJS sources of prisoner, jail and probation releases. They incorporate recidivism statistics so that they do not over-count people who have already served a term of prison, jail, or probation. Shannon et al. (2016) indicate they used one, two-and three-year estimates from the 1983 BJS recidivism study (Beck and Shipley, 1989). However, recidivism does not peak until about 7 years post-release. It is unclear to us exactly how these recidivism statistics were applied. If they did use only up to three-year return rates, they may have inadvertently made an error. Release cohorts overestimate the number of

individual offenders who return to prison. Rhodes et al. (2014) show that most offenders do not return to prison so that using the release cohort percentages will reduce the cumulative incidence too much. This return estimation procedure may not produce appreciable error, but it is hard to know. In addition to these BJS sources, Shannon et al. (2016) use demographic life tables for the period 1948-2010 to estimate the number of released felons lost to mortality, assuming substantially higher mortality rates for ex-felons. It is unclear how these researchers estimate felon death rates over time since we know of no source for those data. Nevertheless, we appreciate the difficulty in cobbling together these separate sources to make a valuable estimate of the extant current and ex-offender felony population.

Pettit, Western and Sykes (2009) estimated cumulative incidence for the Pew Public Safety and Mobility Project. Western and Pettit (2010) cite these data to show that risk of imprisonment for individuals up to ages 30 to 34 was influenced by race and education. Their data were constructed to examine cumulative incidence for the following cohorts: the 1979 cohort born 1945-1949; the 1989 cohort born 1955-1959; the 1999 cohort born 1965-1969; the 2009 cohort born 1975-1979. The methods and data used by Pettit, Western, and Sykes (2009) are similar to the survey-based approach of Bonczar (2003a). The report by Pettit, Western, and Sykes (2009) showed cumulative incidence by birth cohort, race/ethnicity, gender, and education. For example, data on men ignoring the impact of education (Table 38) show that the cumulative risks of imprisonment by age 30 to 34 for the 1979 cohort were 1.35% for Non-Hispanic whites, 2.83% for Hispanics, and 10.35% for Non-Hispanic blacks. For the 2009 cohort the cumulative incidence to ages 30-34 were 5.35% for Non-Hispanic whites, 12.16% for Hispanics, and 26.84% for Non-Hispanic blacks, a marked increase in cumulative incidence over time.

Unfortunately, they did not publish aggregate cumulative risk estimates so we cannot compare their estimates to Bonczar's estimates and ours.

Analysts at the Bureau of Justice Statistics have published cumulative incidence estimates (Bonczar and Beck, 1997; Bonczar, 2003a) of imprisonment. We focus on the more recent report since both reports appear to use the same methodology. Bonczar (2003a) estimated that as of 2001, nearly 17 percent of black male Americans then alive had been incarcerated at some time during their lives. Moreover, between 1974 and 2001, the estimated percentage had nearly doubled, and Bonczar (2003a, p. 1) projected that "if rates of incarceration remain unchanged, 6.6% of all persons born in the United States in 2001 will go to State or Federal prison during their lifetimes, up from 5.2% in 1991, and 1.9% in 1974." We highlight this increasing trend, also reported by Pettit, Western and Sykes (2009), because our analysis of younger age cohorts shows a reverse trend.

The methodology used for the two BJS publications is explained in the publications' appendices and a stand-alone publication available from BJS (Bonczar, 2003b). The stand-alone publication provides the most explanation we have found for survey-based estimation procedures, and we credit Bonczar for his transparency. It allowed us to make a more systematic comparison among survey-based methods and the one we propose. We greatly simplify our review of this methodology, dropping important details from our synopsis, and encourage reading the original papers. This summary builds on what was already reported about the survey-based approach.

The estimates of first-time prison admission come from the inmate surveys of state and federal inmates conducted by BJS. The surveys have a complex survey design that samples prisons and then, within prisons, samples inmates who are asked to answer a series of questions. One

question is whether the inmate was admitted to prison for the first time during the current year. Inmates are also asked if they are native born. Because the survey is a cross-section of prisoners on a specific date, anyone who had been admitted and released for the first time within the year prior to the day of the survey would not be surveyed. The methodology paper by Bonczar (2003b) describes using the NCRP to derive an adjustment factor which is composed of the ratio between new court commitments in the 12 months preceding the survey date and new court commitments minus the first releases during that same period. One problem with this measure is that it is sometimes difficult with the NCRP to determine whether a commitment is the first prison commitment. So, it seems that the adjustment factor, which varied between 1.19 and 1.25 from 1987 to 1997, would overweight first-time offenders.

Combined with general Census data, the survey weights, and the adjusted factor, the researchers provide estimates of the proportion of members of a birth cohort who were admitted to prison as of a specific age. An illustration may be helpful.

Suppose that 100 offenders born in the United States during 1992 were incarcerated during a survey administered in 2012 and this number has already been adjusted with the NCRP-based weights described immediately above. Suppose that 10 of those 100 offenders said that they were incarcerated for the first time during the year before the survey. This implies that the incidence for the 1992 birth cohort is 10, and dividing 10 by the size of the 1992 birth cohort still living in 2012 gives incidence at age 20 for the 1992 birth cohort. In fact, Bonczar's calculations were more complicated than this simple description but this simplification suffices for our purpose.

This methodology provides estimates of incidence for a specific birth cohort/age combination. For example, it provides incidence at age 20 for the 1992 birth cohort, incidence at age 21 for the 1991 birth cohort, incidence at age 22 for the 1990 birth cohort, and so forth. It appears Bonczar used the five inmate surveys conducted in 1974, 1979, 1986, 1991, 1997, and 2001 to estimate age-specific first prison admissions. Bonczar used those results to fill-in a sparse number of cells in a matrix of birth cohorts by year eligible to be incarcerated as an adult. To work, the methodology presumes a steady-state of incidence over birth cohorts prior to the first survey, and interpolation between the dates of the subsequent surveys. So, it depends on a presumed model.

Having summarized Bonczar's survey-based approach and the new NCRP-based approach, we conclude with a more detailed contrast here:

- For cohorts born in 1982 and later, and where data are available, the NCRP-based methodology uses tabulations for first-time admissions through 2015 and it uses imputations thereafter. The survey-based methodology is model-based.
- The survey-based methodology relies heavily on steady-state assumptions. The NCRP-based methodology needs no steady-state assumptions for incidence estimates during years when incidence is highest, weak steady-state assumptions for incidence during years when incidence is moderate, and (like the survey-based approach) heavy reliance on steady-state assumptions during years when incidence is smallest.
- The survey-based approach has unknown sampling and non-sampling error.
    - Some facilities refuse to participate in the survey. Within facilities that agree to participate, some offenders refuse to participate. We cannot be sure about selection bias.

- The accuracy of survey-based self-reports of when an offender first entered prison is unknown.
- One might expect substantial sampling variance although Bonczar does not provide any estimates, so it is difficult to judge the accuracy of his estimates.[2]
- Steady-state assumptions add non-sampling errors to projections.

- The survey-based approach uses life tables to adjust estimates based on birth and death rates. The age specific mortality estimates are estimated for prisoners based mortality rates in the general population after conditioning on education levels and using an adjustment that reflects the presumably higher mortality rates of people with lower education levels. The weighting adjustment was 1.20, a 20% increase for prisoners over the general population. It is difficult to know the accuracy of this adjustment. In contrast, death rates are unimportant for the NCRP estimate.
- Because surveys are expensive, the survey-based estimates are expensive. NCRP-based estimates are easily and inexpensively updated on an annual basis.
- The first four bullets signify advantages for the NCRP-based estimates, but the survey-based estimates are advantageous in other regards.

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[2] Essentially Bonczar is estimating a hazard under steady-state assumptions. First note that each age-specific hazard is estimated with considerable error. This is because inmates with especially long terms accumulate in prisons, so inmates admitted during the last year are a small proportion of the offender pool. A different sampling scheme could improve the estimates. Second, note that the survival function (prevalence is one minus the survival function) is a function of the integrated hazard, so standard errors grow larger as cumulative incidence is estimated at older ages. Bonczar's prevalence estimates are likely to have a great deal of unreported sampling error. For a discussion of sampling errors, see standard sources, such as (Hosmer, Lemeshow, & May, 2008). Additionally, while we use the terms hazard and survival functions in this paper, from a statistical viewpoint Bonczar is estimating a competing risk model, for which other descriptive terms may be more appropriate (Pintilie, 2006).

- First, the survey-based estimates are based on a well-defined population of U.S.-born citizens.[3] The NCRP-based methodology mixes U.S.-born citizens with non-U.S.-born citizens and non-citizens.
- Second, by definition, immigration and emigration are not a problem for the survey-based method but they create ambiguity for the NCRP-based methodology.
- Third, the survey-based approach leads to a prevalence estimate of the proportion of U.S. born citizens alive during year X who have ever served time in prison. This useful metric is not available from the NCRP-based method.

Although the bullets provide a contrast of two methods, we do not conclude that one approach is superior to the other, and in fairness, the following sections will identify additional weaknesses of the NCRP-based approach. The purpose of this review is to show that a new NCRP-based method would be a welcome adjunct to the traditional survey-based method.

4. Methodology

Although the concepts of incidence and cumulative incidence previously introduced may seem simple, measurement is sometimes complicated, and therefore it is necessary to make informed approximations. As indicated, the NCRP is a BJS program that reports all prison terms that were active during an observation window in each state (see table 1). The first column identifies the state. The second and third columns identify the first and last reporting years, respectively, as of the time of this paper. The fourth column reports the observation window length in years and the

[3] This appears to be true. However, the methodological discussions are vague about details.

fifth column reports the total number of admissions to prison during the observation window. These calculations do not distinguish between first and repeat admissions. The table is ordered from longest to shortest observation window. States that lack reliable term-based records do not appear in this table. We will return to this table repeatedly.

The length of the observation window varies across the states, and seven states (comprising roughly 7 percent of the U.S. population) do not provide data that we can process into prison terms.[4] Otherwise, state-level data can be combined into national estimates with imputations for missing states. We initially ignore incarceration in federal prisons.

We begin the discussion with a single age cohort, born in 1982, living in a single state, Georgia. We then consider estimates for younger Georgia cohorts and then for older Georgia cohorts. The NCRP has an observation window which runs from 2000 through 2015 for many states. Other states report for earlier years (including Georgia) and some start reporting after 2000, but we will use 2000 through 2015 as an illustration and then consider how different observation windows affect the calculations.

The 1982 age cohort is special for our purposes because its members turn 18 in 2000 and 2001, at the start of our NCRP observation window. This means that we can count the number of 1982 age cohort members who enter prison for the first time at the age of 18, at the age of 19, and so on. So clearly, we can compute rather than estimate incidence and cumulative incidence for this age cohort through the age of 32. Only about half this cohort's members turn 33 in 2015, the last year of currently available NCRP data, but if we double the observed incidence from this group for this last year, we

[4] To be useful, state-level data must identify offenders across multiple prison terms. Otherwise we cannot identify first-time admission.

also have an estimate of incidence as of age 33.[5] Thus it is straightforward to derive incidence and cumulative incidence using the raw number of first-time admissions for the 1982 age cohort, but only through age 33. Despite this minor adjustment, we treat doubling of the last year of data as an observation rather than an imputation, because we intend to use the term imputation in a different context and seek to avoid confusion.

For younger cohorts born after 1982, the counting exercise is no more difficult, but we cannot track incidence through the age of 33. For the 1983 age cohort, the best we can do is track incidence through the age of 32, for the 1984 age cohort the best we can do is track incidence through the age of 31, and so on. A basic but understandable problem is that we cannot track cohorts into the future. Nevertheless, we can project (impute) future incidence for these relatively young cohorts, and such projections are part of the methodology.

Computing incidence for older cohorts is more complicated. Consider the 1981 age cohort. Its members began to turn 18 in 1999, but the NCRP observation window begins in 2000. This means that we cannot use the NCRP data to definitively identify an offender's first-time admission. The first-time admission may have occurred in 1999, and therefore it would be invisible to us. This may seem like a minor problem for the 1981 age cohort, because only a small number would have been incarcerated for the first time in 1999 (when half of the 1981 age cohort turns 18), but the problem is progressively worse for older age cohorts born before 1981. Thus, this problem cannot be ignored.

[5] Doubling is only an approximation. If incidence is trending, then doubling cannot be correct, and if birth dates are not evenly distributed during the first and second half of a year, doubling cannot be correct. A refined methodology might deal with this problem but we ignore the problem for this paper.

Because of this indeterminacy, we compute incidence through 2015 only for age cohorts born in 1982 and later. However, we use data for older cohorts to project or impute incidence for younger cohorts beyond the age of 33, attempting thereby to answer the question: what is the likely lifetime cumulative incidence for age cohorts born in 1982 and later?

To explain the basis for the projections, consider that we cannot tabulate the 2016 incidence for the age cohort born in 1982. However, we might assume that the incidence (measured as a population proportion rather than a raw number) for the 1982 age cohort would be about the same in 2016 as was the incidence for the 1981 age cohort in 2015, that the incidence for the 1982 age cohort would be about the same in 2017 as was the incidence for the 1980 age cohort in 2015, and so on. In this illustration, then, we are imputing incidence for the 1982 age cohort using the closest age cohort for which we have information about incidence. Similarly, if we calculate cumulative incidence for younger age cohorts such as those from 1983 to 1987 we can directly count incidence through 2015 then impute beyond this observation window using the nearest age cohorts older than the one we are imputing, which we term the "nearest neighbor" cohort.

We will show subsequently that there is considerable randomness when computing incidence for any age cohort. This suggests that using the nearest neighbor for imputation might impart unwarranted variation to the projections. Therefore, we modify the above nearest neighbor approach slightly: the nearest neighbor is actually the average across the nearest three neighbors. Notice also that the post-1981 age cohorts have the nearest neighbors in common, so tabulations for the post-1981 cohorts differ, but projections beyond the data support for those tabulations, the projected incidence beyond 2015, are necessarily the same.

This imputation method has a problem: we cannot positively identify that a prison admission in 2015 for the 1981 age cohort was in fact a first-time admission (we would need 1999 data). However, we can fairly accurately identify that an admission in 2015 was a first-time admission for any cohort member born before 1982 by adopting what we call the *7-year rule*. That is, if a person was released from prison and has not returned to prison in a seven-year period, then it is very likely that the offender will not return to prison (Rhodes et al., 2014). A corollary is that if a member of the 1981 age cohort entered prison in 2015 and had not been released from prison within the previous seven years, then the 2015 admission was almost certainly a first-time admission. Note that for states that began reporting in 2000, this is actually a fifteen-year rule, because if we observed an admission between 2000 and 2014 inclusive, we would know that the 2015 admission was not a first-time admission. However, some NCRP states report admission starting on a date post-2000 and for them a seven-year rule may be binding.

A limitation to this methodology is that it only provides incidence measures for age cohorts born in 1982 and later. Although this is a limitation, projections for these young cohorts are especially useful because the prison experiences of these young cohorts are relatively recent, ongoing and subject to public policy intervention. The projections for these cohorts are most credible because incidence for early ages is observable, estimated incidence for middle ages is imputed from closely matched older cohorts, and estimated incidence for older ages – while based on distantly matched older cohorts – is relatively low and has little ultimate impact on cumulative incidence. Readers might think of our approach as answering the question: what would be the projected incidence and cumulative incidence of prison burden if we assume that the future burden of prison will mimic the immediate past burden of prison? We will refer to such projections as *synthetic estimates*.

To clarify and then introduce new complications, when forming state-specific imputations, we always divide the number of first-time prison admissions for members of an age cohort within a state by the number of the members of the age cohort who were residents in the state during the year of calculation. That is, if 100 adults age 20 were admitted to prison for the first time during 2002, and if the size of the 1982 age cohort living in the state equaled 100,000 in 2002, then estimated incidence at age 20 for the 1982 age cohort would be 0.001=100/100,000. As the size of age cohorts varies over time (e.g. the 1982 age cohort is larger than the 1952 age cohort at all ages), the proportion of first admissions remains on the same scale.[6]

There are two problems. Understanding the first requires that we re-emphasize the difference between an age cohort and a birth cohort. Understanding the second requires that we make some accommodation for the way that race is reported by the NCRP and the Census Bureau.

Regarding the age cohort/birth cohort distinction, if we follow the size of the 1982 age cohort (or any other recent age cohort) over time, we find that the 1982 age cohort gets larger in Georgia, and by contrast, it gets smaller in Michigan. Obviously, what is happening is that in Georgia, immigration into the state has exceeded the combination of deaths and emigration. In Michigan, the combination of deaths and emigration has exceeded immigration.

These observations raise the question of what a population-adjusted computation means. Specifically, for every year that we compute incidence, we are basing that computation on a different population base. Cumulative incidence calculated from annual incidence in this way has no clear

[6] This ratio is not a hazard. Computing a hazard would require removing past first-time admissions from the base, and that step is impractical given that the population base is constantly changing.

interpretation except the mechanical one of summing incidence. The survey-based approach does not suffer from this ambiguity.

One way to resolve this ambiguity is to assume that immigration and emigration do not change the nature of the risk set. That is, the age-specific risk of first-time admission to prison does not depend on cohort members emigrating and immigrating – those who stay in the state, those who emigrate and those who immigrate are all from the same risk pool, e.g. the immigrants, emigrants and native born Georgians have the same incidence. Given the available data, which do not distinguish populations by emigration and immigration status, there seems to be no alternative to assuming the constancy of the risk set, but in so doing, we remain cognizant that we are approximating the truth.

Moreover, there is some solace in moving from state-level estimates to national estimates, because if emigrants are at a higher or lower risk than are immigrants within a state, the bias will approximately balance when state-level statistics are aggregated. By combining data across states, we reduce the swings in age cohort sizes. If we combine Georgia and Michigan, for example, we internalize the migration and immigration between these two states. This is not altogether true; even when we combine all states, we are faced with immigration from outside the United States, but surely the problem is reduced.

Unfortunately, two new problems emerge. One is that the 7-year rule no longer works reliably because we cannot track offenders across states. If an offender was released from a Michigan prison in 2001, emigrated to Georgia in 2005, and was admitted to a Georgia prison in 2007, he would appear to us as a first-time offender in both states. We have no immediate solution to this problem. Based on the best information at our disposal that shows that the same offender rarely appears in prison across two states (see the section on bias), we think that the problem is small although not

inconsequential. Regardless, the NCRP is developing a procedure for linking offenders across states, so the salience of this problem will diminish as the NCRP matures.

A different sort of problem is that states use prisons differently and report prison administrative records differently. For example, some states have integrated jail/prison systems, so the definition of a "prison term" is expansive.[7] Some states have restrictive criteria for entering prison; for example, to enter a Massachusetts state prison, an offender must have been sentenced to a term of two and one-half years or more. Thus, movement across states will change incidence measured at a national level even if incidence remains unchanged at every state level. Both the survey-based approach and the NCRP-based approach are susceptible to this problem.

A final difficulty comes from the way that race is reported in the NCRP and the decennial census. In a nation attaching so much importance to racial disparity, it may seem surprising that race is difficult to define and measure. For the NCRP, some states allow offenders to self-define their race, and other states allow prison officials to attribute race categories to offenders. Both self-definition and attribution are fraught with error. The Census Bureau allows respondents to report many race categories including mixed categories, which are defined in our Census data as *mixed*. This difficulty alerts us to the possibility, if not the probability, that race-based incidence may trend for reasons that have nothing to do with prison burden and will have much to do with changing patterns in the self-reporting of race.

[7] For NCRP, six states (Alaska, Connecticut, Delaware, Hawaii, Rhode Island, and Vermont) have "unified" systems, meaning that the state prisons and jails are administered by a single agency and as two components (or functions) of one system. The distinct functions of jails and prisons remain, and they may occur in separate types of facilities, but they are jointly administered and have a single information system for managing offender and detainee data and records. Because their systems will contain both sentenced and unsentenced populations, NCRP data providers in unified states are reminded to ensure that they exclude those awaiting trials and other unsentenced populations before submitting NCRP records to BJS.

Using Georgia as an illustration, table 2 illustrates this problem. The table reports six races as classified by the U.S. Census. The table reports a large growth in the Georgia population between 2000 and 2010, but that growth is not our immediate focus. What we note especially is that the percentage of the Georgia population self-described as mixed has more than doubled. (These are self-reports because they come from Census responses.) Mixed race percentages may change because of actual changes in race or because of changes in individuals' self-identification. In this time frame, respondents to the 2000 census who previously classified themselves as black or white may have reclassified themselves as mixed. Is there more of a change from black to mixed than white to mixed? Because the nature of this self-identification cannot be confirmed, the population basis for race-based incidence may have a distortion based on the way race is identified in NCRP, relative to the way it is self-identified in Census data. The distortion is not gross, but it is material.

Looking at the race categories reported in the NCRP for Georgia, the category "Two or More Races" is an allowable NCRP category, but it does not appear in the NCRP administrative data. Our solution is to treat the race category "mixed" as "black". We emphasize that our intent is to develop a methodology for estimating prison burden. All methodological choices, including the best way to categorize race, are open to criticism, and we welcome constructive criticism as a method to improve our estimates.

A few remaining data issues require mention. For present purposes, we require a prison term to exceed 7 days, before the term is considered to be a prison term for tabulating incidence. Most short prison stays are for purposes such as observation, administrative holds and shock incarceration. However, some are in fact sentences, so the NCRP-based method has a slight bias toward failing to report short-term first admissions. For example, from 2000 through 2015, 675,277 offenders were admitted to Georgia prisons. The number confined for more than seven days was 674,358. At least

in Georgia, this 7-day rule makes little difference because the ambiguity regards only about 1/10 of 1 percent of the admissions. Additionally, the NCRP-based method excludes admissions before the age of eighteen because the data do not capture time spent in juvenile detention. Survey-based methods suggest that the confinement of juveniles in prison is sufficiently rare that first-time admission tabulations should not be seriously distorted.

5. Georgia as an Illustration

This section uses Georgia as an illustration and provides details about calculations. For our purposes, there is nothing special about this state except that Georgia reports NCRP term files dating back to 1971. See table 1, which reports the state abbreviation, when that state first and last reported to the NCRP, the length of the reporting window, and the number of total prison admissions (first and repeat) during that reporting window. For this illustration, we principally use data from 2000 forward because this mimics the more typical NCRP data availability. We use a longer time-series from Georgia as a contrast.

Figure 1 has four panels. None of these panels uses imputation from older age cohorts. We will walk through each panel, from left to right, for the two top panels, then left to right for the bottom two panels. The first panel is the tabulated cumulative incidence for the 1982 age cohort. The curve begins at age 18 because we only consider prison terms as of age 18. The curve is based on tabulations because, for this cohort, we can observe this entire period in the NCRP data. The curve shows that, for the 1982 age cohort: (1) cumulative incidence increases at an

increasing rate through the early twenties and thereafter increases at a decreasing rate,[8] and (2) more than 5 of every 100 Georgians in the 1982 age cohort have experienced prison by the age of 33, and (3) unless the curve is mysteriously disrupted at age 33, cumulative incidence appears to be increasing past age 33.

The second panel shows the cumulative incidence for six age cohorts, 1982 through 1987. For improved resolution, we do not show a legend, but the cohorts are easily distinguished. The longest curve pertains to – and replicates – the curve for the 1982 age cohort that is displayed in the first panel. The shortest curve pertains to the 1987 age cohort. The second panel makes a simple point: incidence and cumulative incidence have remained fairly stable across these six contiguous age cohorts. The finding is not surprising, but it is important. If there were no stability, there would be little justification for synthetic estimation. However, incidence has not been perfectly stable. In fact, there appears to have been a slight decrease in cumulative incidence. We reserve a discussion of statistical testing until later.

The third panel reinforces the conclusion drawn from the second panel. The third panel shows the incidence instead of cumulative incidence for the 1982 age cohort. Incidence peaks at age 21 and declines thereafter at least until the age of 33. The fourth panel shows incidence instead of cumulative incidence over the six contiguous age cohorts. The impression is that incidence is alike across these six continuous panels, but there is undeniable year-by-year fluctuation.[9] The

[8] The curve's first derivative is positive throughout. The second derivative is positive during the early years and turns negative sometime after age 20. Hence cumulative incidence increases at an increasing and then at a decreasing rate.

[9] The cause for the fluctuation is not so clear. Population data come from the 2000 and 2010 Decennial Census with interpolations for non-census years. Comparing the 2009 interpolation with the 2010 census suggests that the

year-by-year fluctuation provides justification for averaging incidence across multiple nearest neighbor cohorts when performing imputations. Arguably, averaging will provide more accurate imputations.

Figure 2 projects the curves shown in figure 1 forward to an age approaching 70. As before, read the panels from left to right for the two top panels, then left to right for the bottom two panels. The first panel shows projections for the 1982 age cohort. Through age 33, panel 1 in figure 2 is the same as panel 1 in figure 1. For age 34, cumulative incidence is based on the average incidence at age 34 of age cohorts 1979 through 1981. Similar projections apply to age 35, 36, and so on. Because of this imputation, we call this the 1982 synthetic cohort. The second panel shows the projections for the 1982 through 1987 age cohorts. To distinguish the curves, we have made the curves progressively thinner for younger cohorts (the 1982 age cohort has the thickest curve; the 1987 age cohort has the thinnest curve). In fact, the curves are practically indistinguishable visually.

The third panel shows incidence for the 1982 age cohort. This curve is identical to its counterpart in panel 1 of figure 1 until the age of 33; after that point, incidence is based on the average incidence for older age cohorts. Notice that there is a slight bump in incidence at age 34. Because the imputations look backward toward older cohorts, and because incidence seems to be trending downward according to panel 2 of figure 1, the bump implies that at age 34 estimated incidence is slightly biased upward. This bump will appear in other figures that aggregate across states. The final panel shows the observed and projected incidence for the 1982 through 1987

interpolations have a significant amount of error. Our approach has been to base population projections on a linear interpolation of the estimates provided by the Census.

age cohorts. The curves differ slightly until age 33. Thereafter, they are necessarily the same because they are based on the same imputations. That is, they all have the same nearest neighbors. For example, we cannot observe age 34 incidence for any of the cohorts born in 1982 and later. For them, we impute incidence as the average incidence at age 34 for the 1979-1981 age cohorts. When making this calculation, the 1979-1981 age cohorts are the nearest neighbor for everyone born on or after 1982.

Based on the first panel of figure 2, we project that 7-8 of every 100 Georgians born in 1982 will spend some time in prison during their lifetimes. The risk of first-time incarceration is greatest during an offender's early twenties and decreases progressively thereafter. We revisit three caveats raised earlier. About 7-8 of 100 Georgians in the 1982 birth cohort will spend some time in prison provided immigrants into Georgia and emigrants leaving Georgia have the same risk of prison as do native born Georgians. This may be false, so the 7-8 of 100 estimate is approximate. Also, the approximation is based on observed prison admissions until the age of 33, but past the age of 33, the approximation presumes the incidence for older cohorts is the same as the incidence for the 1982 age cohort. Because the approximations are based on the nearest neighbor older cohorts, the approximations are likely reflective of reality, but of course there is no way to place a confidence interval on that presumption. Finally, we are unable to precisely identify first-time admissions for offenders who were admitted and released from other states, moved to Georgia, and were then re-admitted to prison in Georgia. Our inability to identify first-time admissions when an offender is an immigrant from another state means that the 7-8 in 100 estimate is probably biased upward. We will discuss the size of likely bias in the section on bias.

Georgia is special in the sense that we can observe prison admissions since 1971, and therefore we can tabulate first-time admissions for the age cohorts 1961 through 1967 into their fifties. Figure 3 shows results from this tabulation; however, we have made an accommodation. To translate from raw numbers to a rate, we divided by the approximate age cohort size as of 2010. Because of deaths, this might seem to bias the cumulative incidence upward, but that is probably not true. Because of immigration into the states, the size of these age cohorts grew from 2000 to 2010. The cumulative incidence rates may in fact be understated. Nevertheless, they are larger than the cumulative incidence rates experienced by the 1982 through 1987 age cohorts. We do not make much of this comparison between the older and younger cohorts. The principal point is that except for differences in scale, the shapes of the cumulative distributions look much alike. No labels appear, but the cohorts are easily distinguished. The 1961 cohort has the longest curve. The 1967 cohort has the shortest curve. The variation across these curves suggests modest trends. A comparison of figure 3 with figures 1 and 2 suggests that incidence has fallen materially over time. We do not make much of that observation because from a methodological viewpoint, Georgia is the only state that provides such a long time-series, so generalization is limited, and we have not accurately captured the changing population composition for these older cohorts.

6. Eighteen Key States as an Illustration

Eighteen states have reported term records from 2000 through at least 2014. Table 1 identifies the states and reports the number of admissions (not necessarily first-time admissions) between 2000 and the last reporting date (as of the date of this paper). The states are sorted by the length of the reporting window; the first eighteen are used in this current analysis. Georgia appears as one of the 18 states and accounts for about 5% of admissions. Note the very large share

represented by California. This is especially noteworthy because of the recent realignment of California state prisons that shifted prison burden from state prisons to county jails (Lofstrom & Martin, 2015).

Figure 4 is the counterpart to figure 1. While figure 1 pertains to the state of Georgia, figure 4 pertains to the average across the 18 states. The average is a weighted average – California gets more weight than any other state because it has the largest population. To clarify, we do not compute incidence for each state and then average across the states, but rather, we aggregate all admissions and all population data across the states to create an artificial super-state comprising 18 states and compute incidence the same way that we did for Georgia. One difference in the figures is that figure 4 only tracks incidence through age 32. This is because 4 of the 18 states have only reported through 2014 (as of when this analysis was performed), so including incidence during 2015 would require imputation for those four states, and we want this figure to be based purely on tabulation.

Cumulative incidence is somewhat lower when aggregated across these 18 states and compared with Georgia. Between 4 and 5 of every 100 members of the 1982 age cohort have entered prison by the age of 32. Looking at the second panel, it appears that the cumulative incidence of prison has been falling across the progressively younger age cohorts. We can perform a nonparametric test of a trend over time by examining cumulative incidence by age 27. An older age cohort always has a higher cumulative incidence rate than do all younger age cohorts. There are 6! (! means factorial) ways that the curves could be arranged if they were randomly assigned an order. Under the null hypothesis of random rankings, the probability that the older cohorts would always have higher cumulative incidence than the younger cohorts is 1/6! = 1/720. It seems

unlikely that this arrangement would happen by chance so we conclude that this trend toward decreasing cumulative incidence is statistically significant.

The highest incidence of prison appears to be at about age 20 for the 1982 age cohort and at about the same age for the other age cohorts. Looking especially at the 4th panel, it appears that the incidence for the older cohorts is higher than the incidence for the younger cohorts (the older the cohort, the longer the line). This of course is consistent with the observations made about the 2nd panel. We can still construct synthetic cohorts, but we must be aware that synthetic cohorts will probably overstate cumulative incidence because they may fail to capture this general downward shift in incidence over time. On the other hand, we do not know when this trend began, so the direction of the bias is uncertain.

Figure 5 is the counterpart to figure 2. The first two panels show synthetic estimates for incidence and cumulative incidence for the 1982 age cohort and for the 1982-1987 age cohorts, respectfully. For Georgia, the cumulative incidence approached 8 in 100. For the 18 states combined, the cumulative incidence is near 7 in 100. This does not necessarily mean that Georgia is more prone to incarcerate members of its population, because states use their prisons in different ways. We might consider the 7 in 100 as our best estimate of the percentage of Americans (native born and foreign born) who will be incarcerated across these 18 states. However, while this 7 in 100 figure is descriptive, we consider the trends represented in figure 4 to be more important because they impart information about contemporary trends. The cumulative incidence of prison appears to be decreasing over time.

Note the bump in the curve around the age of 33. We saw this bump at age 34 when inspecting Georgia data, where we considered it to be an upward bias in the synthetic estimates due to a

general decrease in the incidence across age cohorts. It suggests that the cumulative incidence estimates are biased upward by at least a small amount.

Without difficulty, we can stratify the estimates by population groups of interest. Figure 6 shows results for white males and figure 7 shows results for black males. Using the 1982 age cohort as a point of comparison, figure 6 shows that, across these eighteen states, somewhat over 4 of every 100 white males will be incarcerated by the age of 32. In comparison, nearly 20 of every 100 black males will be incarcerated by the age of 32. For both groups, the highest risk for first-time incarceration is somewhat near the age of 20, but that risk is lower for white males than for black males. Also, compared with the peak risk at about age 20 or 21, the risk is considerably higher between the ages of 18 and 19 for black males than for white males. These estimates confirm what we already believed about the disproportionate burden of prison by race. See appendix A for a discussion.

However, new information comes from inspecting panel 2 of figures 6 and 7. Comparing the 1982-1987 cohorts, there may be a trend toward lower cumulative incidence for white men, but if so the pattern is muddled by some reversals of the lines, some apparent ties, and in general no large differences. In contrast, for black men, the patterns seem distinct. Older cohorts always have higher cumulative incidence (as of age 28) than do all younger cohorts. As noted earlier, the probability that this pattern would emerge by chance is 1/720. It appears that the burden of prison has been declining especially for black males, although cumulative incidence for black males remains much higher than the cumulative incidence for white males.

We turn next to differences by gender. Many fewer women than men are held in state prisons. Figure 8 shows incidence and cumulative incidence for women. Contrasts between figure 8 and earlier figures are

apparent. At least through the age of 33, incidence and cumulative incidence are much lower for women than for men. The period of highest risk is later for women than for men, and we see the period of heightened risk as being dispersed, suggesting that first-time admissions are more delayed for women. Additionally, we see no evidence that incidence has been increasing or decreasing across the 1982-1987 age cohorts for women.

Although the above exercise only used data from 18 states, the exercise was useful because it made full use of NCRP data from states that provide the longest observation window – 2000 through 2014 or 2015. But many other states report for shorter windows, and we now turn to those states.

7. Adding States with Shorter Windows

The 18 states used above are special in that we observe admissions and releases from 2000 through 2014 or 2015, but many NCRP contributing states began reporting after 2000 (see table 1), and those states have relatively short observation windows. Nevertheless, the methodology applied to the 18 states is adaptable to states whose term records are available for shorter observation windows. This section discusses that adaptation. In fact, the only thing that inhibits using data from these states with shorter windows is that we need to replace heavier reliance on imputations to account for more missing data.

Consider Pennsylvania, a state that has contributed NCRP data since 2001 rather than since 2000. We cannot tabulate the incidence at ages 18 through 24 for members of the 1982 age cohort because we cannot observe incarceration in 2000 and the 7-year rule does allow us to compute incidence before the age of 25. The solution to this problem is to impute the ages 18 through 24 incidences from the nearest neighbors. In this case, the nearest neighbors are post-1982 cohorts. This imputation is probably of little significance for Pennsylvania. It only applies to the 1982 age cohort because incidence for younger cohorts is observed.

Some states have shorter time-series. Maine, at the extreme, has reported admissions only for 2012 through 2015. The methodology works for even such a short period, except that (1) the 7-year rule has to be reduced to a 2-year rule, and (2) steady-state assumptions are stronger. Because of relaxing the 7-year rule for Maine, first-time admissions are almost certainly overstated for that state. With good cause, we might have excluded Maine from the analysis, but looking to the future, when the NCRP will expand to include more data, we included Maine for completeness.

Figure 9 has a familiar structure, but here the synthetic cohort curves are based on data from all the states that have contributed admission data to the NCRP since at least 2012. Also, we have not artificially constrained the follow-up period to 2014, so when 2015 data are available, we will use them. The implications from figures 5 and 9 are essentially the same despite the fact that figure 5 is based on fewer states than is figure 9. We again conclude that roughly 7 of 100 of individuals who are members of the 1982-1987 age cohorts will serve some prison time over their lifetimes. For reasons already explained, these estimates are probably biased upward, but we will argue that the bias is probably not large.

Although all states currently report to the NCRP, table 1 does not list all states because at the time that data were assembled for this report, the NCRP-team could not link term records across 7 states (AR, CT, HI, ID, LA, VA, VT). These states cannot enter the estimation. They comprise roughly 7 percent of the prison population across the United States, so unless the missing states have incidence rates that are wildly divergent from the included states, their admission should not affect substantive findings. Furthermore, we are hopeful that these states will eventually contribute data useful for constructing term records to the NCRP.

8. Bias

Recently, BJS has asked states to report FBI numbers with their NCRP submissions, thereby eventually allowing researchers to link offenders across states and to link state-level offenders with federal offenders. This data collection is in its early stages, but the NCRP data analysis team has made preliminary tentative estimates that 94% of the time, when an offender is released from prison and is readmitted to prison, the readmission is to a prison within the same state. About 2% of the time the readmission is to a prison within another state. And about 3% of the time the readmission is to a federal facility.

The 2% estimate is deceptively high for current purposes because it pertains to offenders who return to prison. In fact, NCRP data show that roughly two of every three state offenders who enter state prison will not return to prison (Rhodes et al., 2014), so as an estimate of cross-state readmissions, the 2% estimate is too high. We conclude that estimates of incidence reported in this paper are biased upward, because some offenders are released from prison in state A and reenter prison for the first time in state B, but that if cross-state migration could be considered, the estimates would not change much.

Regarding federal offenders, between 2000 and 2014 (fiscal year for 2014), nearly 1 million offenders were sentenced for felonies and serious misdemeanors (hence they are eligible for prison terms under guidelines) in federal court, but only about 61% were U.S. citizens likely to be at risk for incarceration in state prison. Of those U.S. citizens, about 31% had not previously been incarcerated; that is, nearly 120 offenders per state per year were admitted to prison for the

first time because of a federal conviction.[10] We conclude that the estimates of incidence reported in this paper are biased downward as a result of omitted federal prison terms, because some offenders who appear to enter state prison for the first time had served a prison term under federal custody, but that if federal prisons could be taken into account, the estimates would not be much changed. That is, according to table 1, which reports the number of admissions in each state since 2000, a difference of 120 offenders per state per year is unlikely to have a large impact on estimates. Furthermore, a separate BJS program, the Federal Justice Statistics Program (FJSP), captures federal prison terms. Should BJS decide to integrate NCRP and FJSP, this bias will be reduced in the future.

A different kind of bias comes from using Census estimates that impute population totals based on the decennial census. The sizes of these errors are difficult to judge, but clearly, they exist, because every new census brings adjustments. There is some risk that trends may result merely from erroneous population counts, although this risk is mitigated by aggregation across states.

Thus, while we acknowledge biases toward over counting first-time admissions, we think the bias is not large, and ongoing data collection will lead to improvements in future estimates.

[10] The statistics come from a tabulation of data from the United States Sentencing Commission. Estimates are conditioned on the offender having a criminal history category of 1. This category indicates that the offender had not previously served a period of confinement of more than 60 days. Given that our criterion for a prison term is 7 days, this classification provides an upward bias to the estimates. However, it is possible to have a higher criminal history category without having served a previous prison term, so the estimate may be biased downward.

9. Conclusions

We have drawn comparisons between our new NCRP-based methods and the survey-based methods of Bonczar (2003a). Although they are based on birth cohorts, Bonczar's estimates conform more closely to our estimate of cumulative incidence of prison over a lifetime than do the other estimates we have reviewed. As reported earlier, Bonczar projected that "if rates of first incarceration remain unchanged, 6.6% of all persons born in the United States in 2001 will go to State or Federal prisons during their lifetime …" Bonczar's estimate is remarkably similar to the estimate provided in figures 5 and 9, despite the following differences in the populations:

(1) Bonczar's targeted baseline population is people born in the United States during 2001; our targeted population is people born between 1982 and 1987 and living in the United States. Because net immigration only accounts for about 5 percent of the population, and because 1987 is not far removed from 2001, the differences in the targeted populations are not large. Additionally, the estimates would be about the same if our assumption of an invariant risk set is correct.

(2) Bonczar's projections are based on incarceration patterns as of the dates of the five inmate surveys: 1974, 1979, 1986, 1991 and 2001. Our projections are based on incarceration patterns between 2000 and 2014. Because incidence has not changed greatly between 2000 and 2014, we would not expect projections to differ greatly because of this difference.

(3) Bonczar's estimates include incarceration in federal facilities, but we argued above that much federal incarceration is for non-citizens in the country who are here legally or

illegally (and hence would not be part of Bonczar's estimates) or for repeat offenders (and hence would not be first-time offenders). Nevertheless, our estimates do not account for those federal offenders.

(4) As noted above our estimates exclude the seven states for which offender records could not be linked, while Bonczar's estimates conceptually include all states.

Despite the differences in methodology, the survey-based estimates and the NCRP-based estimates are in substantive agreement, reemphasizing that the two methods are complementary. Given that the NCRP-based estimates are inexpensive to assemble, the NCRP provides a useful platform for nearly real-time updates of trends in incidence. The survey-based estimates continue to provide unique insight into prevalence.

We acknowledge limitations, but many of these limitations will decrease with ongoing improvements to the NCRP.

1. Seven states are missing from the analysis because the NCRP study-team lacks a reliable way to link multiple prison terms for the same offenders within those states. This is not an overwhelming problem because for most states the NCRP provides accurate linkages. Furthermore, the NCRP team is working with states on improving within state linkages. Over time, the number of states that lack reliable within-state linkages will fall and they may altogether disappear.

2. The NCRP does not afford cross-state linkages, and in a nation with cross-state population movement, this means that we are over-counting first-time admissions. We think this over-counting is minor based on work examining the proportion of offenders who are incarcerated in two or more states, but we cannot be sure. However, the NCRP is

working on methods for linking offenders across states, so in the future, whatever biases exist due to this factor will be reduced or eliminated.

3. We have not incorporated federal prison terms into the estimates, but the same promise of linking prison terms across states extends to linking state prison terms with federal prison terms. In the meantime, our best evidence is that the resulting bias is not large.

4. The sixteen-year window (2000 through 2015) might be seen as short and, for some states, the window is even shorter. The NCRP is updated annually, so the current window will over time be extended past 2015. Furthermore, states are sometimes able to back-fill currently missing data. The NCRP continuously improves with additional state participation.

We believe that trends in the incidence and cumulative incidence of first-time admissions for the 1982-1987 age cohorts are an important social statistic. Without the NCRP, comparable statistics are difficult to assemble and update in a timeframe that makes them most useful for policy purposes. The methodology introduced in this report should provide a welcome addition to other ways of tracking correctional statistics in the United States.

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Appendix A: A Brief Review of Racial Disparity

The racial disparity in mass incarceration has been documented by many scholars and is a key feature of the National Research Council's (2014) report. Citing primary sources (Beck and Blumstein, 2012; Langan, 1991) the NRC report shows that from 1925 to about 1980 black prison admission rates varied between 75 and 125 per 100,000, while white prison admission rates varied between about 25 and 45 admissions per 100,000, roughly a two to one ratio of black to white incarceration rates. The dramatic rise in prison admission and imprisonment rates for both black and white offenders began about 1980, and by 2000 the ratio of black to white imprisonment rates was 6.3 to 1, and as recently as 2010 the ratio was 4.6 to 1. As Western and

Pettit (2010) state "[the] cumulative risk of incarceration is important if serving time in prison confers an enduring status that affects life chances after returning to free society (Western and Pettit 2010, p. 10)." We cite some of the literature investigating this enduring impact, but the consequences of incarceration are not otherwise a subject of this paper.

While we do not use our estimates to deduce the causes of racial disparities, we acknowledge scholarship on the reasons for this racial disparity. One of the primary hypotheses was formulated by Blalock (1967) and has been labeled the racial threat theory. Blalock theorized that whites, members of the majority group, will perceive a threat from a growing black population and will use political, social and economic levers to control the perceived threat (Horowitz, 1985; Liska, 1992). The racial threat hypothesis is often tested by examining the relationship among geographically bounded levels of the ratios of black and white populations and measures of social control (Stolzenberg, D'Allesio, and Eitle,2004). Support for the racial threat hypothesis varies depending on the state-sanctioned form of control that researchers study (Dollar, 2014).

Other scholars make the argument that the reason for the racial disparity in imprisonment is that the criminal justice system is unjust to racial minorities. (Cole, 1999; Tonry, 1995). Cole argues that although there is always a tension between constitutional rights and the protection from crime, the line drawn between these competing interests depends on class and race. According to Cole, the law may appear to be neutral, but he asserts that it is applied differentially. As one example, Cole notes that the Supreme Court interpreted the Fourth Amendment to prohibit police from searching luggage, purses, or wallets without a warrant based on probable cause. However, the Court permits consent searches and Cole claims this tactic is often disproportionately used on young black men who are unaware that they can legally deny the police request. According to

some scholars, these and other policies and practices such as racial disparities in traffic stops, arrest, prosecution, and sentencing lead to an implicit two-tiered system of justice even though the law appears to be color-blind.

One of many outcomes of disparities in incarceration is related to the impact imprisonment has on reintegrating back into the community. There is evidence that a criminal record interferes with post-incarceration employment (Eberstatdt, 2016; Western, 2002), civic engagement (Uggen, Manza, and Thompson, 2006) and produces other forms of social inequality (Western and Pettit, 2010). The racial disparity in imprisonment extends to the post-release disparity in employability. This has the effect of amplifying the racial disparity in a criminal record by reducing post-release employment opportunities, and there is evidence that racial minorities are affected more by this tangible scarlet letter of imprisonment (Pager, 2003; 2008; Quillian and Pager, 2005).

Our paper is focused on the method of estimating the lifetime cumulative risk of criminal justice contacts, specifically adult imprisonment, rather than on the theoretical underpinnings of the causes and effects of those contacts. Therefore, the literature review in the main text pertains to the estimation methods.