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# Cumulative Risks of Multiple Criminal Justice Outcomes in New York City

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## Abstract

Previous research has provided estimates of the cumulative risk of felony conviction and imprisonment in the United States. These experiences are, however, also the rarest; most of what happens in the criminal justice system occurs at the level of the misdemeanor rather than the felony. This article addresses our limited understanding of the scope of subfelony justice by providing estimates of the cumulative risk of several lower-level arrest outcomes for one jurisdiction: New York City. Because of excess life table events contributed by nonresidents of New York City, estimates are likely upwardly biased relative to the true values. Nonetheless, they allow us to (1) assess the cumulative risk of misdemeanor conviction and jail sentences and (2) determine to what extent those who enter the world of subfelony justice are distinct from those with felony or imprisonment records.

**Keywords** Subfelony justice · Misdemeanor enforcement · Cumulative risk · Criminal justice system · New York City

## Introduction

Demographic research on the American criminal justice system has focused on the most extreme forms of contact: felony conviction and imprisonment. Central to this body of research is a set of studies that employ life table techniques to demonstrate variations in the risk of own imprisonment (Pettit and Western 2004), parental or kin

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imprisonment (Chung and Hepburn 2018; Wildeman 2009), and felony conviction (Shannon et al. 2017). These studies lay bare the reach and racial inequalities of the criminal justice system.

Felony arrest, however, is not the most common form of criminal justice encounter (Kohler-Hausmann 2013, 2014, 2018; LaFountain et al. 2012; Natapoff 2012, 2015), nor is imprisonment the most common outcome. Although the literature provides insight into high-level criminal justice contacts and the most serious outcomes from those contacts, knowledge about subfelony justice is surprisingly limited. Notably lacking is an equivalent set of estimates that allow researchers to (1) assess the cumulative risk of misdemeanor conviction and jail sentences and (2) determine the extent to which those who enter the world of subfelony justice are distinct from those with felony or imprisonment records.

This article addresses these gaps in one jurisdiction: New York City. The choice of site is driven both by data availability and by New York City's role as a pioneer in policing tactics emphasizing low-level enforcement. We employ demographic methods to estimate the cumulative risk of misdemeanor conviction, felony conviction, and receipt of a jail or prison sentence by sex, racial/ethnic group, and birth cohort. Notably, we are able to estimate the lifetime risk of misdemeanor conviction *alone*—without any prior or subsequent felony conviction—and demonstrate how that risk has changed as misdemeanor enforcement increased in New York City. This allows us to address the question of how low-level convictions extend *beyond* the populations touched by felony justice.

## Subfelony Justice in the Era of Mass Imprisonment

The world of subfelony justice dwarfs that of felony justice. Natapoff (2012) suggested the metaphor of an iceberg, with only the well-studied outcomes of felony conviction and imprisonment visible above the surface. As with any iceberg, more is hidden below. For example, Stevenson and Mayson (2018) estimated that the arrest rate for misdemeanor offenses, nationally, was roughly 15 times that for violent felonies over the period 1995–2015. They found that misdemeanor case filings outnumbered felony case filings by a ratio of at least three to one.

Subfelony convictions bear penalties. The National Inventory of Collateral Consequences of Conviction documented 354 legal and regulatory sanctions triggered in New York State by different types of misdemeanor conviction (Justice Center 2018). Such sanctions can range from being barred from adopting or fostering a child to being denied tenancy in a public housing authority building to being ineligible for a barber shop owner's license.[1] Even those arrested but not convicted of a misdemeanor can see their odds of future employment decline (Uggen et al. 2014), and Internet access has vastly expanded employers' access to subfelony criminal justice records (Lageson 2016; Lageson and Maruna 2018).

In New York City, misdemeanor arrests have increased as felony arrests have fallen (Kohler-Hausmann 2018). New York City saw approximately 148,000 felony arrests in 1990, 113,000 in 2000, and 92,000 in 2010.[2] Over the same period, however,

[1] The full list can be accessed at https://niccc.csgjusticecenter.org/search/?jurisdiction=35.

[2] Misdemeanor arrest numbers from the New York State Department of Criminal Justice Services (DCJS), on file with the authors.

misdemeanor arrests increased dramatically: in 1990, there were approximately 118,000 misdemeanor arrests in the city; in 2000, that number stood at 224,000; and by 2010, it exceeded 251,000, representing an increase of 113 % from 1990 levels. Consistent with national trends—as well as with New York City–specific patterns in stop, question, and frisk (Fagan et al. 2010; Gelman et al. 2007)—the increase in low-level enforcement has been largely experienced by the city's black and Hispanic population.

How do these arrest rates—and their uneven racial distribution—translate into long-term criminal justice marks (i.e., convictions and sentences)? Aggregate arrest numbers cannot tell us about the incidence of criminal conviction, much less the overlap between populations touched by different levels of the criminal justice system. The cumulative risk of misdemeanor conviction is a valuable measure of how subfelony enforcement translates into legal outcomes—and thus permanent status markers—and the scope of its effects within subpopulations.

A well-developed body of demographic research has highlighted the distinctions between cross-sectional rates of a given criminal justice event (e.g., imprisonment) and the cumulative risk of the same. The former provides information on point-in-time exposure; the latter allows us to measure the share of the population ever to bear the associated burdens. Cumulative risk measures the likelihood of transition to a given level of contact (treated as an absorbing state) and does not account for repeated exposure; after the first death (or conviction, or prison sentence, and so on), there is no other. This literature has documented variations in lifetime risk of imprisonment by cohort, race, education, and geography (Muller and Wildeman 2016; Pettit and Western 2004; Western and Wildeman 2009). It has also looked beyond imprisonment to the larger population that has a felony conviction or has been sentenced to supervised release in the form of probation without ever going to prison (Jacobs 2015; Manza and Uggen 2006; Pettit 2012; Phelps 2017; Shannon et al. 2017).

Data limitations have been a serious constraint to research on misdemeanors. Researchers are barely able to produce national estimates of the overall number of misdemeanor case filings (Stevenson and Mayson 2018). Studies of imprisonment and felony conviction risk rely on data for which no clear analog exists in the world of subfelony justice, at least nationally. Administrative data, however, offer a unique opportunity to construct estimates of cumulative risk for New York City.

## Data and Methods

Our goal in this article is to demonstrate (1) how cumulative risk of two key lower-level criminal justice outcomes—misdemeanor conviction and receipt of a jail sentence—vary by cohort, race/ethnicity, and sex; and (2) how they compare to risk of felony conviction and imprisonment. To do so, we follow the life table methods developed by Bonczar and Beck (1997) and later elaborated by Pettit and Western (2004) and Wildeman (2009). These authors relied on data from multiple waves of the Survey of Inmates in State and Federal Correctional Facilities. No similar surveys provide sufficient data about misdemeanor convictions to extend analyses to subfelony justice. In the absence of such surveys, we take advantage of administrative data.

Data come from the New York State Department of Criminal Justice Services (DCJS), the agency responsible for collecting and maintaining records from local courts and law enforcement agencies and generating criminal histories.[3] The data contain the entire population of individuals convicted of a misdemeanor or felony in New York City in seven sample years: 1980, 1985, 1990, 1995, 2000, 2005, and 2010. Following their first criminal conviction, the DCJS assigns individuals a unique New York State ID (NYSID), which is linked to their fingerprints. Based on NYSID, the DCJS provided full arrest and conviction histories for individuals convicted in the seven sample years. We observe all instances of probation, jail, prison, and parole both prior and subsequent to the event that led to sample inclusion. Because the DCJS can maintain a stable NYSID only following first conviction (or one of a small set of other events), these sequences are sometimes left-censored (i.e., we may fail to observe arrests preceding first conviction) but are complete from first conviction onward. They contain some demographic data about each defendant, including age, race/ethnicity, and sex.

The life table methods noted earlier rely on four types of counts, disaggregated by year, age, race/ethnicity, and sex: (1) first criminal justice events, (2) population with a previous criminal justice event, (3) population counts, and (4) mortality counts. DCJS data effectively provide us with the first sort of count, and from those we estimate the second. Because an individual's NYSID is stable upon first conviction, we can determine for every member of the sample in each sampled year whether that conviction was a first-time or a higher-order event. Based on these data, we produce counts, aggregated by sex, race/ethnicity, and five-year age groups, of first events in multiple categories. (The full set of categories and the underlying logic are provided in the online appendix.) Because of data limitations, we include non-Hispanic white, non-Hispanic black, and Hispanic individuals, but we exclude New Yorkers of all other racial/ethnic groups. We adjust these tallies of events to account for sex-, race/ethnicity–, age-, and period-specific net commuting patterns in New York City. (The process and logic are described in the online appendix.) To estimate the population with a previous event, we aggregate counts of first-time events from previous years for the applicable age category. Decennial census data from IPUMS (Ruggles et al. 2017) provide us with population counts for New York City. We derive population counts for noncensus years by interpolating adjacent age groups across census years making an assumption of stable growth or decline over the period. Death counts are taken from mortality reports periodically issued by the Office of Epidemiology and Statistics in the New York City Department of Health.

Note that our results represent estimates of the cumulative risks that New Yorkers face of experiencing several criminal justice outcomes and that these estimates are likely to be systematically upwardly biased. Some convictions and jail/prison sentences that we record will be to nonresidents of New York City, and some residents of New York City will receive convictions or jail/prison sentences outside New York City that we do not observe in our data.[4] The former represent "excess" life table events in many cases whereas the latter are "missing" events only for those individuals who never experience that particular event in New York City. Even though we expect that the latter

[3] Data were provided by the DCJS to author Kohler-Hausmann in the form of micro-level arrest incidents and de-identified individual ID numbers. The analysis, opinions, findings, and conclusions expressed herein are those of the authors alone and not those of the DCJS. Neither New York State nor the DCJS assumes liability for its contents or use thereof.

[4] Available evidence, discussed in the online appendix, suggests that rates of the former are small.

is a nonzero number, particularly given the specificity of some of our outcomes, the former is likely larger. However, in the absence of data that would allow us to adjust our counts for sex-, race/ethnicity–, age-, and period-specific relative risk of these two events, we operate under an assumption that they offset each other. Further research should attempt to assess the validity of this assumption and provide more reasonable bounds.

In recognition that individuals may experience multiple sorts of criminal justice events over their life course and to better understand the overlap between populations entangled in the worlds of felony and subfelony justice, we provide multiple estimates of the cumulative risk of misdemeanor conviction. In increasing order of specificity, these estimates are as follows:

1. *Cumulative risk of misdemeanor conviction, including those with felony convictions*. How common is it for New Yorkers to be convicted of a misdemeanor? This estimate provides the overall scale of subfelony conviction and is not exclusive of felony conviction.
2. *Cumulative risk of misdemeanor conviction, excluding those with felony conviction*. How common is it for New Yorkers to be convicted of *only* a misdemeanor? This estimate allows us to see how individuals experiencing misdemeanor conviction are or are not separate from those experiencing felony conviction.
3. *Cumulative risk of misdemeanor conviction, excluding both those with felony convictions and those with misdemeanor convictions from felony arrest.* How many New Yorkers have misdemeanor conviction records from only misdemeanor arrest? This estimate allows us to assess the extent to which risk of misdemeanor conviction results from low-level arrests as opposed to charge reduction from felony arrests.

Later categories represent subsets of the prior categories. For example, the second group (those with a misdemeanor conviction but no felony conviction) represents a subset of the first (the total population of individuals with a misdemeanor conviction). In that case, the cumulative risk of holding both a misdemeanor *and* a felony conviction can be calculated by subtracting estimate 2 from estimate 1.

## Results

Table 1 presents the cumulative risks to age 40–44, by race/ethnicity, sex, and cohort, of misdemeanor and felony conviction.

As an example of interpretation, we find that white male New Yorkers born in 1961–1965 had an 8.31 % chance of being convicted of a misdemeanor at least once by age 40–44 (upper-left entry in Table 1). Reading down the first column, we find that 5.64 % of white men in this cohort held a misdemeanor conviction in the absence of a felony conviction, and 2.09 % held only a misdemeanor conviction from a misdemeanor arrest. Put another way, two-thirds (5.96 / 8.86 = .68) of these white men with a misdemeanor conviction held *only* a misdemeanor conviction, and one-quarter (2.26 / 8.86 = .25) held *only* a misdemeanor conviction from a misdemeanor arrest. Their black peers had more than four times the risk of a misdemeanor conviction: 36.71 % of

**Table 1** Cumulative risk to age 40–44, by birth cohort, sex, and race/ethnicity

| | | Male | | | Female | | |
|---|---|---|---|---|---|---|---|
| Birth Cohort | Type of Conviction | White | Black | Hispanic | White | Black | Hispanic |
| Born in 1961–1965 | Misdemeanor conviction | 8.31 | 36.71 | 26.28 | 2.20 | 8.30 | 4.70 |
| | Without felony conviction | 5.64 | 14.42 | 12.15 | 1.82 | 4.84 | 2.62 |
| | Without felony conviction or misdemeanor conviction from felony arrest | 2.09 | 3.92 | 3.77 | 1.13 | 2.19 | 1.05 |
| | Felony conviction | 3.24 | 25.45 | 18.14 | 0.48 | 4.12 | 2.67 |
| Born in 1966–1970 | Misdemeanor conviction | 6.73 | 29.81 | 21.18 | 1.54 | 6.05 | 3.67 |
| | Without felony conviction | 4.40 | 10.02 | 8.83 | 1.20 | 3.36 | 2.25 |
| | Without felony conviction or misdemeanor conviction from felony arrest | 1.52 | 3.27 | 2.93 | 0.75 | 1.38 | 0.94 |
| | Felony conviction | 3.16 | 22.36 | 16.79 | 0.41 | 3.56 | 2.27 |
| Born in 1971–1975 | Misdemeanor conviction | 5.46 | 26.11 | 17.98 | 1.18 | 3.69 | 2.52 |
| | Without felony conviction | 3.74 | 9.00 | 7.27 | 0.96 | 2.19 | 1.67 |
| | Without felony conviction or misdemeanor conviction from felony arrest | 1.45 | 2.55 | 2.45 | 0.53 | 0.66 | 0.82 |
| | Felony conviction | 2.36 | 21.12 | 15.61 | 0.32 | 2.06 | 1.40 |
| Born in 1976–1980 | Misdemeanor conviction | 4.87 | 26.75 | 16.69 | 0.93 | 3.55 | 2.25 |
| | Without felony conviction | 3.41 | 9.56 | 6.29 | 0.77 | 2.28 | 1.65 |
| | Without felony conviction or misdemeanor conviction from felony arrest | 1.46 | 3.43 | 2.28 | 0.36 | 0.74 | 0.81 |
| | Felony conviction | 1.96 | 19.09 | 13.36 | 0.24 | 1.77 | 0.99 |
| Born in 1981–1985 | Misdemeanor conviction | 4.42 | 23.91 | 15.33 | 1.02 | 3.75 | 1.69 |
| | Without felony conviction | 3.23 | 9.62 | 6.91 | 0.85 | 2.62 | 1.30 |
| | Without felony conviction or misdemeanor conviction from felony arrest | 1.52 | 3.20 | 2.70 | 0.49 | 1.04 | 0.58 |
| | Felony conviction | 1.53 | 13.28 | 9.00 | 0.27 | 1.33 | 0.60 |

black male New Yorkers of this cohort had a misdemeanor conviction by age 40–44. Proportionately fewer black males had a misdemeanor conviction in the absence of a felony conviction (14.42 / 36.71 = .39) or from a misdemeanor conviction only (3.92 / 36.71 = .11).

Figure 1 offers a visual representation of the misdemeanor risk estimates from Table 1. Each panel presents a race- and sex-specific stacked bar plot; each bar shows the cumulative risk of misdemeanor conviction to age 40–44 by birth cohort. The bar is decomposed into risk contributed by those who experienced both misdemeanor and felony conviction (in white on the bottom), those who experienced misdemeanor conviction from felony arrest (in gray in the middle), and those who experienced misdemeanor conviction only from misdemeanor arrest (in black on the top).

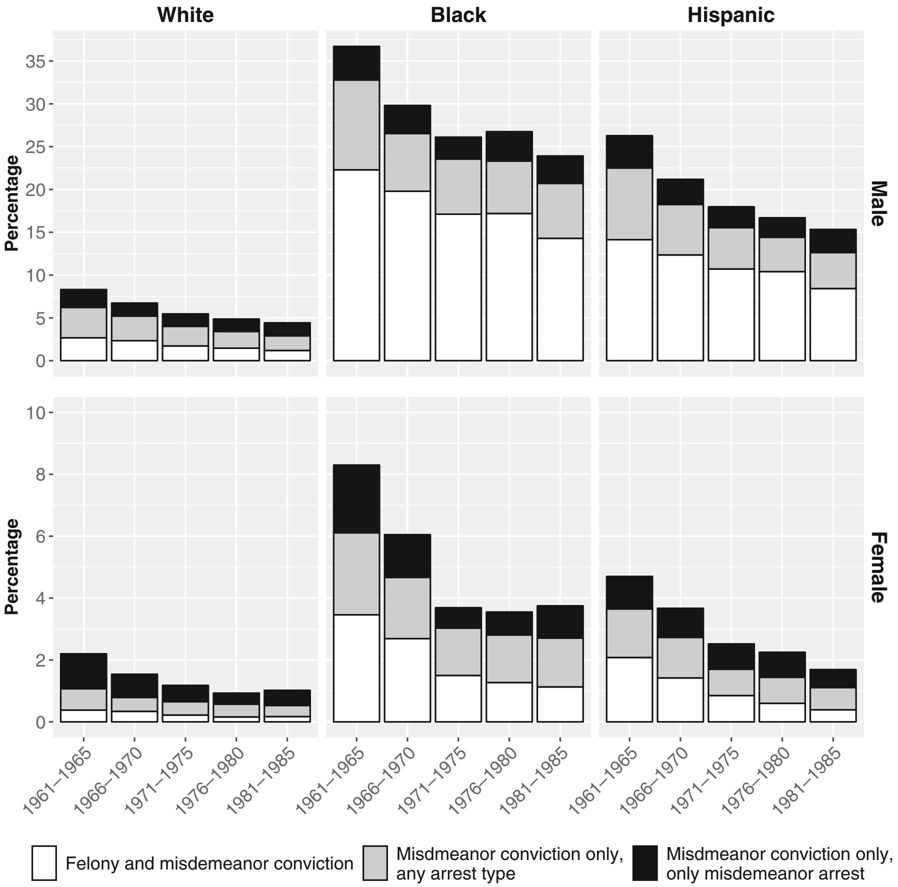


**Fig. 1** Cumulative risk of misdemeanor conviction to age 40–44, by cohort, disaggregated into risk of holding a misdemeanor and a felony conviction; risk of holding a misdemeanor conviction stemming from felony arrest; and risk of holding a misdemeanor conviction only from a misdemeanor arrest. Each panel is a separate race/ethnicity–sex combination; note the shift in *y*-axis between the top and bottom panels.

Four patterns are noteworthy. First, as the prior literature leads us to expect, cumulative risks of all conviction outcomes were higher for black New Yorkers than whites and higher for men than for women. Racial disparities were much larger for cumulative risks of felony conviction than for misdemeanor conviction. The ratio of black risk to white risk was, within conviction type, relatively stable across cohorts: black men faced approximately four to five times the lifetime risk of a misdemeanor conviction (not exclusive of felony conviction) and seven to nine times the risk of a felony conviction as their white peers. The racial disparities were the lowest for misdemeanor conviction alone resulting from misdemeanor arrest. These risk ratios were similar, if somewhat lower, for women.

Second, Hispanics—who are often omitted from analysis of this sort—experienced cumulative risks of each of these conviction events that fell between those observed for whites and blacks, albeit closer to the latter than the former. For instance, among male New Yorkers born 1966–1970, 6.7 % of white men experienced a misdemeanor conviction by their mid-40s compared with 21.2 % of Hispanic men and 29.8 % of black men. Put another way, Hispanic men in this cohort had more than three times the risk of this outcome (relative to their white peers), whereas black men had more than four times the risk.

Third, among those at risk of holding a misdemeanor conviction, a much larger percentage of white than black or Hispanic New Yorkers held *only* a misdemeanor conviction (in the absence of a felony conviction). This is reflected in Fig. 1, which shows that the proportion of overall misdemeanor conviction risk contributed by those who receive both misdemeanor and felony convictions (in white on the bottom) was much larger in the black and Hispanic panels.

Finally, we observe a pattern within race and conviction type of declining cumulative risk across cohorts. For example, black men's cumulative risk of a misdemeanor conviction dropped from 36.7 % in the 1961–1965 cohort to 26.1 % in the 1971–1975 cohort and further downward to 23.9 % in the 1981–1985 cohort. Comparing the 1981–1985 cohort with the 1961–1965 cohort, risks of each type of conviction declined by between 28 % and 53 % for white men, between 18 % and 48 % for black men, and between 28 % and 50 % for Hispanic men. Declines in cumulative risk were higher among women across the board.

These patterns, cumulatively, yield an unexpected conclusion. As the absolute number of black and Hispanic misdemeanor arrests increased over several decades—and as black-to-white and Hispanic-to-white arrest ratios rose—neither the cumulative risk of misdemeanor conviction from misdemeanor arrest nor the respective racial disparities relative to whites increased markedly for members of these minority groups. This suggests that the important site to study racial disparities in the subfelony world may be prevalence and frequency of arrest, especially arrests that do not lead to a criminal conviction.

Table 2 presents an equivalent set of estimates for cumulative risk of receipt of a jail or prison sentence and for risk of receiving only a jail sentence. Among white men born in 1961–1965, 3.67 % were sent to jail or prison by age 40–44; the majority (2.09 / 3.67 = .57) were sent only to jail (i.e., never received a prison sentence). A much smaller

**Table 2** Risk of jail or prison sentences to age 40–44, by birth cohort, sex, and race/ethnicity

| | | Male | | | Female | | |
|---|---|---|---|---|---|---|---|
| Birth Cohort | Sentence | White | Black | Hispanic | White | Black | Hispanic |
| Born in 1961–1965 | Jail/prison | 3.67 | 28.98 | 19.22 | 1.08 | 5.78 | 2.97 |
| | Jail only | 2.09 | 11.46 | 7.50 | 0.93 | 3.77 | 1.69 |
| Born in 1966–1970 | Jail/prison | 3.27 | 23.74 | 16.55 | 0.82 | 4.24 | 2.26 |
| | Jail only | 1.86 | 8.40 | 6.27 | 0.63 | 2.56 | 1.19 |
| Born in 1971–1975 | Jail/prison | 2.64 | 21.88 | 14.73 | 0.46 | 2.14 | 1.33 |
| | Jail only | 1.64 | 9.43 | 6.14 | 0.35 | 1.49 | 0.96 |
| Born in 1976–1980 | Jail/prison | 2.20 | 20.25 | 12.67 | 0.44 | 1.82 | 0.86 |
| | Jail only | 1.43 | 8.66 | 5.86 | 0.35 | 1.30 | 0.62 |
| Born in 1981–1985 | Jail/prison | 1.76 | 16.12 | 10.27 | 0.41 | 1.77 | 0.73 |
| | Jail only | 1.14 | 7.49 | 5.16 | 0.32 | 1.32 | 0.58 |

percentage of minority group members (relative to their white peers) received only a jail sentence. Across cohorts, roughly 60 % of white men who were sent to jail or prison by their mid-40s were sent *only* to jail; for black and Hispanic men, the average was approximately 40 %.

In comparing Tables 1 and 2, note the stable pattern whereby cumulative risk of misdemeanor conviction is greater than that of risk of a jail or prison sentence, which is in turn greater than that of felony conviction. Misdemeanor convictions rarely lead to imprisonment, but felony convictions can lead to either prison or jail; neither sort of conviction by definition leads to a carceral sentence. As such, we would expect risk of prison or jail to be somewhat higher than that of felony conviction—because it takes into account jail sentences stemming from misdemeanors—and somewhat lower than that of misdemeanor conviction.

## Conclusion

This article presents estimates of the cumulative risk of misdemeanor and felony conviction and receipt of a jail or prison sentence for New York City residents, by race/ethnicity, sex, and birth cohort. These estimates allow us to demonstrate, for America's largest jurisdiction, the cumulative risk of subfelony legal outcomes. We find that across cohorts, 4 % to 9 % of white male New Yorkers, 24 % to 37 % of black male New Yorkers, and 15 % to 26 % of Hispanic male New Yorkers have ever been convicted of a misdemeanor. The cumulative risk of misdemeanor conviction is much smaller for female New Yorkers.

These estimates allow us to assess the extent to which the populations affected by felony and subfelony justice are distinct. If the penal mechanisms triggered in the subfelony world and the populations that flow through it are meaningfully distinct from those of the felony world, we may need to revise our understanding of *how* the criminal justice system functions as an inequality-transmitting institution and expand our estimates of the *scope* of its operations. We find that the cumulative risk of misdemeanor conviction is, as expected and in all cases, larger than that of felony conviction. Depending on race/ethnicity, sex, and birth cohort, between 34 % and 83 % of New Yorkers convicted of a misdemeanor are never convicted of a felony. These ratios—risk of misdemeanor conviction without a felony conviction to overall risk of misdemeanor conviction—are lowest for black and Hispanic male New Yorkers. We see a similar pattern with regard to carceral sentences: among those sentenced to jail or prison, white New Yorkers (relative to their minority peers) have been much more likely to be only sentenced to jail rather than prison.

We find a pattern of declining cumulative risk of each criminal justice outcome over time: those born more recently are at lower risk of misdemeanor conviction, felony conviction, and prison or jail sentences than members of previous cohorts. This finding is consistent with observed national-level and New York State–specific declines in imprisonment rates and conviction rates, including drops in misdemeanor filing rates and drug-related prison admissions (Mauer and Ghandnoosh 2015; Pfaff 2017; Stevenson and Mayson 2018). Nonetheless, work remains to square these finding with the previously cited increases in misdemeanor arrest rates in New York City. A number of questions merit future study. What roles do declining crime rates and lowered thresholds of suspicion for low-level arrest play in these shifts? What link can be established between our findings and the "broken windows" policing model (a collection of tactics that intentionally intensified subfelony enforcement in the city from 1994 onward)? It is important to keep in mind that the estimates of cumulative risk that we present here allow us to assess rates of transition into the absorbing states of "ever convicted" or "ever sentenced." The lived experience of those with the mark of a misdemeanor conviction or a jail sentence may vary widely and in ways that such estimates cannot capture. Future work should measure processual and iterative encounters with the criminal justice apparatus over the life course, attempting to better describe changes to both misdemeanor arrest frequency and conviction rates.

We caution again that the estimates presented here are likely biased upward because the number of life table events contributed by nonresidents of New York City likely exceeds the number of missed events (cases in which a resident experiences a given event elsewhere but never in the city). A large number of nonresidents flow through New York City each year; our adjustment for net commuting patterns only partially corrects for life table events likely contributed by these individuals. In the online appendix, we discuss how these patterns may affect our results. However, without additional data about the frequency of either excess or missed life table events by age, race/ethnicity, sex, period, or event type, we are unable to systematically adjust our estimates.

This article examines the scope of subfelony justice in New York City. However, the enforcement and even the definitions of misdemeanor justice vary considerably across the country (Stevenson and Mayson 2018). We hope that further work will provide comparable estimates for other jurisdictions. We also strongly encourage the collection and harmonization of more data on subfelony justice.

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**Online Supplement to "Cumulative Risks of Multiple Criminal Justice Outcomes in New York City"**

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**Review of Life Table Methods**

This article closely follows the lifetable methods developed by Bonczar and Beck (1997) and later elaborated by Pettit and Western (2004). In this supplement, we briefly review these methods and describe how we disaggregate competing risks. For the sake of clarity and comparability we maintain Pettit and Western's notation.

These methods depend on accurate rates, between ages x and x+n, of both mortality (${}_nM_x^D$) and first criminal justice event (${}_nM_x^E$; i.e., first misdemeanor conviction, first felony conviction, and first time sent to prison or jail). The latter is calculated as the count of individuals in the age group experiencing first event (${}_nF_x^E$) divided by the total population in that age group (${}_nC_x$ ) minus those who have previously experienced the event (${}_nS_x^E$):

$${}_nM_x^E = {}_nF_x^E/({}_nC_x - {}_nS_x^E)$$

Calculation of age-specific mortality rates is somewhat simpler because there is no group that has previously experienced the event (death) yet remains in the population (i.e., ${}_nS_x^D$ = 0 by definition). Given age-specific death counts (${}_nF_x^D$), the mortality rates can thus be calculated as:

$${}_nM_x^D = {}_nF_x^D/{}_nC_x$$

The combined risk of "death" (either from mortality or first criminal justice event), ${}_nM_x$, is simply the sum of these two proceeding rates. The probability of first event occurring between ages x and x+n, ${}_nq_x^E$, can then be calculated as:

$${}_nq_x^E = \frac{(n)({}_nM_x^E)}{1 + (n - {}_na_x)({}_nM_x)}$$

(Wachter 2014:154). The ${}_na_x$ term in the denominator adjusts for the timing of events within the interval; we assume, following Pettit and Western (2004:158), that events are evenly distributed and thus set ${}_na_x = n/2$. An equivalent risk term (${}_nq_x^D$) is calculated for mortality. Each of these risk terms is calculated for every combination of age group (starting from age 15-19 and running up to age 40-44), sample year, race/ethnicity, and sex. That is, there is one ${}_nq_x^E$ estimate for white men age 15-19 in 1980, one for white men age 20-24 in 1980, and so on and so forth. Given seven sample years, six age groups, three race/ethnicities (non-Hispanic white, non-Hispanic black, and Hispanic), and two sexes, this yields 252 (=7*6*3*2) such estimates.

These risk terms ( ${}_nq_x^E$ and ${}_nq_x^D$) form the backbone of cohort life-tables. These tables require us to move from thinking in terms of sample years to thinking in terms of birth cohorts. There are five cohorts—starting with those born 1961-1965 and ending with those born 1981-1985—about whom we have sufficient data to make claims. For each race-sex-cohort combination we create a lifetable starting with the 15-19 age group and running up to age 40-44.[1] We begin with a radix $l_{15}$ = 100,000, an imaginary population that will experience the appropriate race-, sex-, and age-specific risks calculated in the previous step. Within each age group the population is reduced by the number of individuals who die ( ${}_nd_x^D = {}_nq_x^D \times l_x$) and the number who experience the first event ( ${}_nd_x^E = {}_nq_x^E \times l_x$). The cumulative risk of the given event occurring is the sum of the total individuals lost to the event at each age category:

$$\sum_X {}_nd_x^E / l_{15}$$

Where, in our case, x = 15, 20, 25, 30, 35, and 40 and n = 5. Because more recent cohorts have not yet experienced older ages (e.g., those born 1981-1985 were age 25-29 in 2010), we assume stability of race/sex/age-specific risks from 2010 for older ages.

The mechanics of carrying out these calculations relies on four types of counts, disaggregated by age, race/ethnicity, sex, and period: (1) first criminal justice events ( ${}_nF_x^E$), (2) population with a previous criminal justice event ( ${}_nS_x^E$), (3) population counts ( ${}_nC_x$ ), and (4) mortality counts ( ${}_nF_x^D$).

DCJS data effectively provides us with the first sort of count, and from those we estimate the second. Because an individual's NYSID is stable upon first conviction, we are able to determine for every member of the sample in each sampled year whether that conviction was a first-time or a higher-order event. Based on these data, we produce aggregate counts, by sex, race/ethnicity, and five-year age groups, of the number of events in multiple categories, described below. These estimates are then adjusted to account for net commuting patterns within the age-sex-race/ethnicity-period group; this adjustment is described in the next subsection.

To estimate the population with a previous event, we aggregate counts of first-time events from previous years for the applicable age category. As an example, the number of individuals age 40-44 in 2010 with a previous misdemeanor conviction is calculated as the number of first-time misdemeanors for those aged 35-39 in 2005 plus those aged 30-34 in 2000 and so on down the age distribution. We multiply these sums by five to account for the five years of exposure between sample years. For earlier years where prior samples are unavailable, we assume stability of age-specific counts from 1980.

For population figures ( ${}_nC_x$ ) we rely on decennial Census counts of the population of New York City. Because of problems in how population counts are reported (by race, especially in

[1] Until very recently, all persons aged 16 or older were automatically treated as adults within the New York criminal justice system. While our youngest age group extends to age 15, first events before age 16 are rare.

earlier periods) in City reports, we rely on IPUMS microdata to derive estimates of the overall population in each five-year age group (Ruggles et al. 2017). Population is stratified by sex and race/ethnicity, with the latter split into non-Hispanic white; non-Hispanic black; non-Hispanic "other" (aggregating across all other racial groups)[2]; and Hispanic (the total population in the sex-age group minus the non-Hispanic white, non-Hispanic black, and non-Hispanic other counts). Population counts of the non-Hispanic "other" group help us to better estimate the population of our three racial/ethnic groups. However, because of limited mortality count data, especially in earlier periods, we are unable to extend our cumulative risk estimates to members of this group.

Population counts for non-Census years (1985, 1995, and 2005) are derived by interpolating adjacent age groups across Census years. For instance, the population age 15-19 in 1985 is derived using the population age 10-14 in 1980 and 20-24 in 1990 and assuming stable growth over the 1980-1990 period.[3] This strategy implicitly assumes that migration into and out of the city was spread evenly across the period. Long-term migration patterns do affect the demographic composition of the city, but we assume that in- and out-migration patterns by sex, race/ethnicity, and birth cohort are not systematically related to likelihood of either participation in criminal activities or conviction/incarceration.

Death counts (${}_nF_x^d$) are taken from mortality reports periodically issued by the Office of Epidemiology and Statistics in the New York City Department of Health. The death counts for 2000, 2005, and 2010 are usable as reported; the earlier tables require some editing. There are two problems. First, the 1980 and 1985 tables split deaths (by age group) into white, not white, and unknown. Second, the 1990 and 1995 tables have Hispanic as a non-exclusive category, allowing the potential of double counting (i.e., a death can be in both "white" and "Hispanic").

To solve the second problem, we started by taking the counts of Hispanic deaths as accurate. The remainder of deaths we distributed between white, black, and other. For white and black deaths we multiply the remainder of deaths by (1) the proportion of the given race within the age- and year-specific non-Hispanic population counts (from above) and (2) the average (over 2000-2010) over- or under- representation of race-specific deaths within the age category, calculated as (e.g., for whites):

$$\frac{white\ deaths/(white\ deaths + black\ deaths)}{white\ population/(white\ pop + black\ pop)}$$

Other deaths are then the remainder of non-Hispanic deaths minus white and black deaths.

To solve the first problem, we analyzed the ratios of deaths to population in the later years. Specifically, we looked at the ratio of the share of deaths (for whites, blacks, and those of other races) to their respective shares of the population. For instance, in 2000 black males made up 31% of the male population aged 15-19 and contributed 38% of deaths, yielding a death-share-

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[2] In the 2000 and 2010 IPUMS Census data, respondents are allowed to specify more than one race. Among all non-Hispanic individuals reporting two or more races, we divided them into the white, black, and other categories proportional to the representation of those three groups within the age-specific category.

[3] In this case, the population is calculated as ${}_{10\text{-}14}P_{1980}$*e^(5*(ln(${}_{20\text{-}24}P_{1980}/{}_{10\text{-}14}P_{1980}$)/10)).

to-population-share ratio of 1.23. For 2000, 2005, and 2010 the black ratio is consistently above 1 (sometimes closer to 2), the white and "other" ratios are below 1 (with only a few exceptions), and the Hispanic ratios are close to or slightly below 1. There is some variance by age in whether the white or other race ratio is lower. We calculated age-specific averages (by race) over the full 1990-2010 period and then just over the somewhat-more-reliable 2000-2010 period; averages are similar. In both 1980 and 1985, blacks and Hispanics make up similar shares of the population (between 20% and 30% each) with those of other races making up 5% or less. Because the white share of deaths in 1980 and 1985 is higher than in subsequent years, we deflated those counts; we multiply the total number of deaths by the average age-specific death shares over the period 1990-2014. We then subtract the number of adjusted age-specific white deaths from the number of age-specific total deaths; for Hispanic and other race counts we multiply the remainder by the race-specific share of the non-white population and then by the race-specific ratio of death share to population share. For the 45-49 age group we use the ratios for the 40-44 age group. Black counts are calculated by subtracting the (adjusted) white, other, and Hispanic counts from the total.

**Adjustment for Net Commuting**

The vast majority of individuals entangled with the criminal justice system in New York City are residents of the city. A recent report from the Misdemeanor Justice Project at John Jay College indicates that 87.9% of misdemeanor arrests in the city in 2014 were of individuals whose IDs listed a New York City address; the remaining 12.1% includes members of New York City's growing homeless population, thus over-stating the scale of non-resident arrests (Warner et al. 2016). Unpublished New York Criminal Justice Agency (NYCJA) data provided to the authors indicates that 93.2% of all prosecuted arrests in New York City in 2016 were of city residents (including homeless residents).

Nonetheless, non-resident criminal justice engagement may pose a problem for our estimates. If we count criminal justice events contributed by non-New Yorkers while maintaining the denominator of New York City populations, we risk over-estimating the cumulative risk of various events. By the same token, residents of the city may be convicted/incarcerated outside of New York City; in missing these events we risk under-estimating cumulative risk. If the two rates—local convictions of non-residents and external conviction of residents—are equivalent, they effectively cancel one another out. Our analysis makes such an assumption; we are unaware of any data that would allow us to test this assumption across the full set of criminal justice events that we measure.

There is, however, at least one pattern that does systematically increase non-resident exposure to New York City and for which we have sufficient data to account: commuting. Commuters—primarily residents of New Jersey, Connecticut, Long Island, or Westchester County who work in the city—represent one group of non-New Yorkers who regularly spend time in the city. Some of these individuals may end up being convicted or incarcerated in the city as well. The NYCJA data cited above provides reason to believe that residents of proximate areas (commuting zones) are the source of most non-resident arrests: of the 7.8% of prosecuted arrests in New York City in 2016 that were of non-residents, nearly half had a home address in one of just three proximate counties (Westchester, Suffolk, and Nassau).

There is also a sizable (albeit smaller) group of New York City residents who commute out of the city for work, thereby reducing their daily exposure to the city. Census Bureau data allows us to measure in- and out-commuting patterns—as well as average travel time, hours worked, and weeks worked—and thereby adjust event counts by age-, race/ethnicity-, sex-, and period-group.

Take as a stylized example, a population of 5,000 white, male, New Yorkers aged 15-19 in 1980. We observe that 600 of these individuals work outside of the city and that out-commuters spend 25% of their total annual time out of the city. Meanwhile, 900 white, male, 15-19-year-old non-residents work in the city; these in-commuters spend 33% of their time in the city. We calculate an age-, race/ethnicity-, sex-, and period-specific adjustment factor for events observed within this group as:

$$adj_{arsp} = 1 - \frac{c_{arsp}^{in} * w_p^{in}}{pop_{arsp} - \left(c_{arsp}^{out} * w_p^{out}\right) + \left(c_{arsp}^{in} * w_p^{in}\right)}$$

Where $pop_{arsp}$ is the age-, race/ethnicity-, sex-, and period-specific population of New York City; $c_{arsp}^{in}$ and $c_{arsp}^{out}$ are the counts of, respectively, in- and out-commuters in the indexed group; and $w_p^{in}$ and $w_p^{out}$ are period-specific percentages of time that, respectively, in- and out-commuters spend working or commuting. For in-commuters this is time in New York City; for out-commuters it is time outside of the city. In our example, this works out to:

$$1 - \frac{900 * .33}{5000 - (600 * .25) + (900 * .33)} = .942$$

We multiply the counts of first events within this sex-race/ethnicity-age-period group (${}_nF_x^E$) by this adjustment factor before carrying out any life table calculations.

Data necessary to calculate these adjustment factors—both counts of in- and out-commuters and work and commute time—are drawn from the decennial Census for 1980, 1990, and 2000 and from five-year American Community Survey (ACS) estimates for 2010. We employ microdata from IPUMS (Ruggles et al. 2017); person weights are employed for all calculations. In-commuters are defined as non-residents of New York City who report working in the city; out-commuters are residents of New York City who report work and who work in any location that is not New York City. Period-specific work time percent ($w_p^{in}$ and $w_p^{out}$) is calculated in three steps: we add average weekly working hours and half of daily round-trip commuting time (multiplied by five)[4]; we multiply this sum by average weeks worked; and we divide this quantity by the number of hours in a year (24*7*52=8,736). For non-decennial years (1985, 1995, and 2005), we linearly interpolate the adjustment factors based on those calculated in adjacent periods. The total set of adjustment factors is presented in table A1.

[4] We take half of weekly commuting time under the assumption that roughly half of in- and out-commutes consists of time spent in New York City.

This process reduces the total number of events proportionate to the person-years of exposure contributed by in-commuters (while also accounting for the reduced exposure of out-commuters). This entails two assumptions. First, we assume an equal probability that residents and non-residents will be convicted of a crime and sent to jail or prison. If in-commuters are *more* criminogenic than locals and/or are *more* likely to be caught and punished for their actions, we may be insufficiently deflating event counts; if in-commuters are *less* criminally-engaged or *less* likely to be caught/prosecuted, we may be over-deflating counts. Second, we do not vary these adjustment factors by event type, thereby assuming an equal probability that in-commuters will be, for instance, convicted of a misdemeanor as they will be sent to prison. We know of no data that would allow us to test these assumptions.

Lastly, it bears noting that these adjustment factors do not account for (1) the movement of other populations through the city (and the events that they contribute) and (2) any differences in the rates at which these populations are convicted/incarcerated relative to the rates at which New York City residents are convicted/incarcerated outside of the city. The data cited above provide reason to believe that criminal justice system entanglements of non-residents of New York City represent a small percentage of cases prosecuted in the city; we do not know the rates at which New York City residents are convicted/incarcerated outside of the city. Both factors would affect estimates.

Consider two unlikely scenarios in which we miss no life table events for New York City residents. In the first, non-residents contribute 10 percent of each sort of criminal justice event for every group under analysis. Removing those excess counts would result in a 10 percent drop in our estimates across the board; ratios of events by sex, race/ethnicity, and event type would, however, remain stable. In the second scenario, white non-residents (regardless of age and sex) contribute 10 percent of each event type while black and Hispanic non-residents contribute only five percent of each event type. If excess counts were removed, cumulative risk estimates would drop by the given amounts, respectively by racial/ethnic group, and the ratios by race/ethnicity would shift (but not the ratios by event type or sex within race/ethnicity). Equivalent scenarios follow for variations by sex and event type (variations by age are of less concern because we are not analyzing age patterns). Missed events contributed by New Yorkers (which can also vary by race/ethnicity, sex, age, period, and event type) would, if observed, mitigate the effect of these excess counts.

It is tempting to speculate about the net effect of these excess and missed events. For instance, we think it is likely that non-Hispanic whites are significantly over-represented among the population of tourists visiting New York City and that the relative contribution of excess life table events is probably greater among whites than blacks or Hispanics. That would result in something akin to the second scenario described in the previous paragraph: we would overestimate white cumulative risks by more than we would overestimate the equivalent risks for blacks and Hispanics (and thus underestimate racial/ethnic disparities). However, at this time we have no additional data upon which to model the frequency of excess or missed events or their distribution by age, race/ethnicity, sex, period, or event. As such, any assessment of the extent to which estimates are upwardly biased and variations in this bias are inherently hypothetical. As more comprehensive data become available, future analyses should analyze the extent of this bias.

## Competing Risks

Over the course of their lifetimes, individuals may have varying entanglements with the criminal justice system. A large percentage of the population will never be convicted of any crime. Those who are convicted of a crime may receive one type of conviction (either once or repeatedly) or multiple forms. We operate with a simple system consisting of three types of convictions: felony convictions, misdemeanor convictions stemming from a felony arrest, and misdemeanor convictions stemming from a misdemeanor arrest. These are, over a lifetime, potentially overlapping events, as represented in Figure A1.

[Figure A1 Here]

Table 1 reports four types of conviction risk that can be represented using the notation from Figure A1 (where each independent segment of the Venn diagram is labeled with a letter). Risk of misdemeanor conviction is the union of the risk of misdemeanor conviction from felony arrest and the risk of misdemeanor conviction from misdemeanor arrest (B+C+D+E+F+G). Risk of misdemeanor conviction without felony arrest removes the intersections with risk of felony conviction (and thus C+F+G). Risk of misdemeanor conviction without felony conviction or misdemeanor conviction from felony arrest is simply the area represented as G in Figure A1. Risk of felony conviction is A+B+D+E.

In order to calculate these various risks, we use DJCS data to produce a series of count variables (these are the ${}_nF_x^E$ values described above). We produce five counts for each racial/ethnic-by-sex group in each period under observation:

1. Total persons with a first-time criminal conviction of any type;
2. Total persons with a first-time felony conviction (i.e., they may have a prior misdemeanor conviction);
3. Total persons with a first-time misdemeanor from a felony arrest (i.e., they may have either a prior felony conviction or a prior misdemeanor conviction from a misdemeanor arrest);
4. Total persons with a first-time felony conviction or misdemeanor conviction from felony arrest (i.e., they may have a prior misdemeanor conviction from a misdemeanor arrest); and
5. Total persons with a first-time misdemeanor conviction (i.e., they may have a prior felony)

Following the procedures laid out above, these counts allow us to calculate the following cumulative probabilities, respectively:

1. $P(F \cup M_F \cup M_M)$ = A+B+C+D+E+F+G
2. $P(F)$ = A+B+D+E
3. $P(M_F)$ = B+C+E+F
4. $P(F \cup M_F)$ = A+B+C+D+E+F
5. $P(M_F \cup M_M)$ = B+C+E+F+G+D

This provides us with the total risk of a misdemeanor conviction (#5) and total risk of a felony conviction (#2). Total risk of conviction (#1) minus total risk of felony conviction (#2) also gives us total risk of a misdemeanor conviction in the absence of a felony conviction. We then need only to calculate the cumulative risk of misdemeanor conviction from misdemeanor arrest in the absence of either felony conviction or misdemeanor conviction from felony arrest (i.e., $P(M_M \cap F^c \cap M_F^c) = G$). To get that number we can subtract the lifetime risk of felony conviction (#2) and the lifetime risk of misdemeanor conviction from felony arrest (#3) from the overall lifetime risk of conviction (#1). That is:

$$
\begin{aligned}
& P(F \cup M_F \cup M_M) = A + B + C + D + E + F + G \\
- \quad & P(F) = A + B + \quad D + E \\
- \quad & P(M_F) = \quad B + C + \quad E + F \\
& = \quad -B + \quad -E + \quad G
\end{aligned}
$$

This leaves us with the desired probability (G) but also two subtraction factors accounting for the joint probability of felony conviction and misdemeanor conviction from felony arrest (B+E). We need to add those two probabilities back in. They can be isolated fairly easily by using #2, #3, and #4 above:

$$
\begin{aligned}
& P(F) + P(M_F) = A + 2B + C + D + 2E + F \\
- \quad & P(F \cup M_F) = A + B + C + D + E + F \\
= \quad & P(F \cap M_F) = B + E
\end{aligned}
$$

We can then add that joint probability into the result of the preceding step and produce a clean estimate of the cumulative risk of misdemeanor conviction from misdemeanor arrest alone (G).

The same basic procedure is at work, albeit simplified, with calculation of prison and jail risk. Here we have three counts:

1. Total persons with a first-time jail sentence (i.e., they may have a prior prison sentence);
2. Total persons with a first-time prison sentence (i.e., they may have prior jail sentence); and
3. Total persons with a first-time jail or prison sentence

These correspond with P(Jail), P(Prison), and P(Jail ∪ Prison). Again the laws of probability allow us to determine $P(\text{Jail} \cap \text{Prison}^c)$ as P(Jail ∪ Prison) – P(Prison).

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**Fig A1** Venn diagram of multiple conviction risks

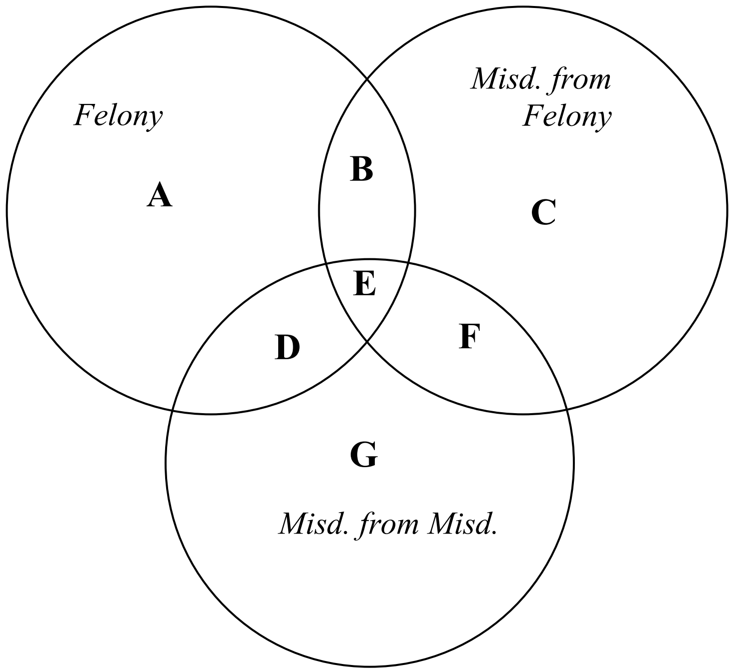

**Table A1** Age-, race/ethnicity-, sex-, and period-specific commuting adjustment factors

| | | Male | | | | | | | Female | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| **White** | | **1980** | **1985** | **1990** | **1995** | **2000** | **2005** | **2010** | | **1980** | **1985** | **1990** | **1995** | **2000** | **2005** | **2010** |
| | **15-19** | 0.992 | 0.990 | 0.989 | 0.991 | 0.993 | 0.993 | 0.994 | **15-19** | 0.994 | 0.993 | 0.991 | 0.992 | 0.993 | 0.994 | 0.994 |
| | **20-24** | 0.966 | 0.953 | 0.939 | 0.944 | 0.948 | 0.955 | 0.961 | **20-24** | 0.964 | 0.959 | 0.953 | 0.958 | 0.962 | 0.966 | 0.970 |
| | **25-29** | 0.945 | 0.929 | 0.913 | 0.922 | 0.932 | 0.938 | 0.945 | **25-29** | 0.968 | 0.955 | 0.943 | 0.947 | 0.951 | 0.957 | 0.963 |
| | **30-34** | 0.911 | 0.904 | 0.897 | 0.897 | 0.898 | 0.911 | 0.924 | **30-34** | 0.973 | 0.960 | 0.947 | 0.945 | 0.943 | 0.948 | 0.954 |
| | **35-39** | 0.877 | 0.882 | 0.887 | 0.875 | 0.863 | 0.870 | 0.878 | **35-39** | 0.972 | 0.962 | 0.953 | 0.944 | 0.935 | 0.938 | 0.940 |
| | **40-44** | 0.877 | 0.876 | 0.875 | 0.862 | 0.849 | 0.849 | 0.848 | **40-44** | 0.975 | 0.964 | 0.954 | 0.945 | 0.937 | 0.934 | 0.931 |
| | **45-49** | 0.882 | 0.873 | 0.865 | 0.863 | 0.862 | 0.854 | 0.847 | **45-49** | 0.972 | 0.965 | 0.959 | 0.951 | 0.944 | 0.932 | 0.921 |
| **Black** | | **1980** | **1985** | **1990** | **1995** | **2000** | **2005** | **2010** | | **1980** | **1985** | **1990** | **1995** | **2000** | **2005** | **2010** |
| | **15-19** | 0.999 | 0.998 | 0.997 | 0.998 | 0.999 | 0.999 | 0.998 | **15-19** | 1.000 | 0.999 | 0.998 | 0.998 | 0.998 | 0.998 | 0.998 |
| | **20-24** | 0.995 | 0.993 | 0.992 | 0.991 | 0.991 | 0.991 | 0.991 | **20-24** | 0.996 | 0.994 | 0.991 | 0.991 | 0.991 | 0.991 | 0.990 |
| | **25-29** | 0.990 | 0.987 | 0.984 | 0.984 | 0.983 | 0.982 | 0.981 | **25-29** | 0.993 | 0.990 | 0.988 | 0.986 | 0.985 | 0.986 | 0.987 |
| | **30-34** | 0.986 | 0.984 | 0.982 | 0.981 | 0.979 | 0.976 | 0.972 | **30-34** | 0.990 | 0.989 | 0.988 | 0.985 | 0.982 | 0.981 | 0.981 |
| | **35-39** | 0.985 | 0.983 | 0.980 | 0.976 | 0.971 | 0.967 | 0.962 | **35-39** | 0.989 | 0.987 | 0.985 | 0.981 | 0.978 | 0.976 | 0.975 |
| | **40-44** | 0.979 | 0.976 | 0.973 | 0.971 | 0.970 | 0.964 | 0.959 | **40-44** | 0.992 | 0.988 | 0.984 | 0.981 | 0.978 | 0.974 | 0.969 |
| | **45-49** | 0.986 | 0.981 | 0.976 | 0.978 | 0.979 | 0.971 | 0.963 | **45-49** | 0.991 | 0.989 | 0.986 | 0.984 | 0.982 | 0.977 | 0.973 |
| **Hispanic** | | **1980** | **1985** | **1990** | **1995** | **2000** | **2005** | **2010** | | **1980** | **1985** | **1990** | **1995** | **2000** | **2005** | **2010** |
| | **15-19** | 0.999 | 0.998 | 0.998 | 0.997 | 0.997 | 0.998 | 0.998 | **15-19** | 0.999 | 0.999 | 0.999 | 0.998 | 0.998 | 0.998 | 0.998 |
| | **20-24** | 0.994 | 0.992 | 0.990 | 0.990 | 0.989 | 0.988 | 0.986 | **20-24** | 0.995 | 0.994 | 0.992 | 0.992 | 0.992 | 0.991 | 0.990 |
| | **25-29** | 0.990 | 0.986 | 0.982 | 0.983 | 0.984 | 0.982 | 0.980 | **25-29** | 0.995 | 0.992 | 0.988 | 0.988 | 0.987 | 0.985 | 0.983 |
| | **30-34** | 0.984 | 0.980 | 0.977 | 0.977 | 0.976 | 0.973 | 0.969 | **30-34** | 0.994 | 0.991 | 0.988 | 0.987 | 0.986 | 0.982 | 0.978 |
| | **35-39** | 0.982 | 0.980 | 0.978 | 0.975 | 0.972 | 0.968 | 0.964 | **35-39** | 0.993 | 0.991 | 0.990 | 0.988 | 0.986 | 0.981 | 0.976 |
| | **40-44** | 0.976 | 0.977 | 0.978 | 0.976 | 0.974 | 0.966 | 0.959 | **40-44** | 0.994 | 0.991 | 0.988 | 0.987 | 0.985 | 0.982 | 0.980 |
| | **45-49** | 0.980 | 0.980 | 0.980 | 0.978 | 0.976 | 0.969 | 0.962 | **45-49** | 0.995 | 0.992 | 0.990 | 0.987 | 0.985 | 0.981 | 0.978 |