

**INSTITUTIONS IN CHILDHOOD AND THE TRANSITION TO ADULTHOOD:  
CONSEQUENCES OF CRIMINAL JUSTICE AND CHILD WELFARE SYSTEM  
CONTACT IN THE UNITED STATES**

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**INSTITUTIONS IN CHILDHOOD AND THE TRANSITION TO ADULthood:  
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Cornell University 2020

This dissertation investigates the implications of foster care placement and incarceration for living arrangement transitions and health in early life. First, I use the 1997 National Longitudinal Survey of Youth to propose an expanded conceptualization of home-leaving that incorporates institutional transitions typically excluded from such analyses. Using life table and regression analysis, I find that this institution-inclusive measure estimates earlier first home-leaving in the transition to adulthood than conventional methods, particularly for young adults who are Black and have lower levels of parental education. Second, I use inverse probability-weighted regression and the National Survey of Child and Adolescent Wellbeing to estimate associations between foster care placement and care and living arrangement instability among children with similar risks of entry into foster care. Although foster care is associated with greater instability overall, analysis of only “excess” changes finds that foster care is linked to less instability in children’s living arrangements and persistently greater instability in their primary caregiver relationships. Finally, the third chapter uses linked administrative data from New York City to estimate associations and causal effects of gestational paternal incarceration on infant birth outcomes. Counter to prior research on paternal incarceration and health, I find evidence of negative effects of paternal incarceration on likelihoods of adverse infant birth outcomes. Combined, these analyses situate experiences of institutionally involved children and families within a broader understanding of family life and health in the United States.

## BIOGRAPHICAL SKETCH

Youngmin Yi completed a Bachelor's degree in Economics and French from Wellesley College. She also holds a Master's degree in Sociology, with a minor in Demography and concentrations in Inequality, Stratification, and Mobility and Race and Ethnicity from Cornell University, where she also pursued her doctoral training. Her research examines the role of policy institutions in shaping diversity and inequality in family life and health. Her articles have been published in several peer-reviewed journals including *American Journal of Public Health*, *Journal of Marriage and Family*, *Social Forces*, and *Socius*. Prior to beginning her studies at Cornell University, she was a researcher and coordinator of the internship and fellowship programs at the Institute for Women's Policy Research. There, her work focused primarily on social inequality in employment and economic security in the United States, motivating her pursuit of advanced sociological and demographic training. In July 2020, Youngmin began a position as Assistant Professor of Sociology at the University of Massachusetts Amherst.

## **DEDICATION**

To my dear parents, Inkyung Kang Yi and Insu Yi

and

In loving memory of and in living tribute to my grandmother, Youngsun Sophia Kang.

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## INTRODUCTION TO THE DISSERTATION

Social inequality in health and family life in the United States (U.S.) is shaped by institutions that purportedly address social problems and meet societal needs (Mare 2011; Roberts 2002; Wakefield and Uggen 2010; Wildeman and Muller 2012). Perceived as a set of institutions of social control that only affect those deserving of or necessitating state intervention, the child welfare and criminal legal systems<sup>1</sup> are in fact far-reaching (Enns et al. 2019; Kim et al. 2017), with consequences that extend beyond the targeted individual or family (Clear 2007; Comfort 2008; Edwards 2016; Elliott and Reid 2019). More specifically, the child welfare and criminal legal systems have the explicit objectives of responding to and controlling child maltreatment and crime, but have broader implications that may impact health and social relationships at all levels of social organization (e.g. Foster and Hagan 2015; Wildeman, Edwards, and Wakefield 2020; Wildeman and Wang 2017). A sociological approach to studying institutional contact centers this broader understanding of the child welfare and criminal legal systems and the lives that they touch.

Through this lens, this dissertation looks beyond the behaviors or outcomes that are “supposed” to be detected and regulated by these systems (i.e. child maltreatment and crime) and into the fundamental roles these institutions play in shaping other critical, even primary, elements of social life and organization (e.g. Comfort 2008; Perry 2006; Pettit and Western 2004; Reich 2008). How might child welfare and criminal legal system involvement relate to

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<sup>1</sup> I use the term “criminal legal” rather than “criminal justice” to refer to the broader policy apparatus designed to identify and respond to allegations of criminal activity. “Criminal legal,” unlike “criminal justice,” includes criminalization of particular actions or behaviors, law enforcement, trial and conviction of suspected offenders, and punishment, rather than the justice system’s responsibilities of assessment of guilt and sentence determination (Gardner 2007; Moore 1997). I also use this term to recognize that policies operating under the name of “justice” are not universally understood or interpreted to be just (e.g. Robinson 2001; Stuntz 1997).

social definitions and experiences of family and the life course? How do the people to whom we are tied create linkages between institutions and our own life chances, perhaps even before our own lives begin? This dissertation empirically engages these overarching questions through three investigations of the relationship between child welfare and criminal legal system involvement and the transition to adulthood, childhood care and living arrangements, and health in early life, while also critically evaluating the definitions, measures, and comparisons used in the study of institutional contact and its correlates and consequences to date.

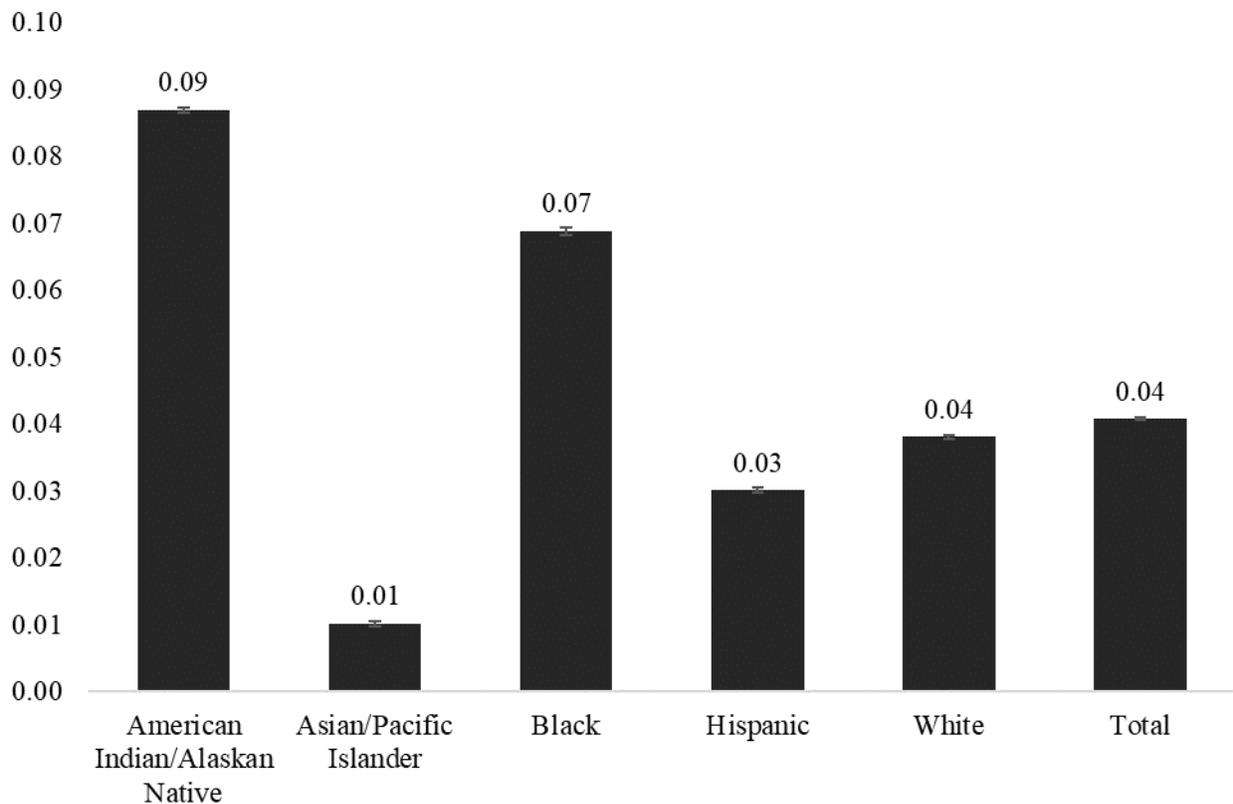
### **The Prevalence and Distribution of Institutional Contact**

To contextualize this exploration of the child welfare and criminal legal systems in the U.S., it is important to detail their reach. Over two million adults are currently incarcerated in a jail, state prison, or federal prison in the United States (Kaeble and Cowhig 2018), with an additional four million adults under other forms of supervision within the correctional system (i.e. on probation or parole; Kaeble and Cowhig 2018). Cross-sectional totals of those confined in jail or prison and the imprisonment rate have declined since the mid-1990s and 2008, respectively (Bronson and Carson 2019; Kaeble and Cowhig 2018). However, the U.S. continues to have the highest national imprisonment rate in the world with 655 adults imprisoned per 100,000 adults in the national population, (Bronson and Carson 2019; Walmsley 2018).

Cumulative risks of child welfare system involvement are similarly high (Kim et al. 2017; Edwards, Wildeman, and Wakefield 2020), including risks of foster care placement, which may be surprising given that foster care is a policy intervention intended for the most severe conditions of child maltreatment (Yi, Edwards, and Wildeman 2020). For every 100,000 children

in the U.S., 593 are in foster care on any given day.<sup>2</sup> Children come to the attention of the foster care system after being reported to a child protective services agency for alleged maltreatment, screened in for further investigation, and deemed to require removal from their care and living arrangements for their protection and wellbeing. Children might also enter foster care should they, though not alleged or confirmed as victims of maltreatment themselves, be in a home environment that has been identified as high risk for another child.

**Figure 1. Cumulative Risks of Foster Care Placement by Age 18, United States, 2016**



Notes: Adaptation of results from Yi, Edwards, and Wildeman 2020. Error bars report 95% confidence intervals.

<sup>2</sup> Based on author’s calculations using data from the U.S. Census Bureau (2020) and the U.S. Department of Health and Human Services Administration on Children and Families, Children’s Bureau (2019).

Contact with both of these systems is indeed prevalent, and unequally distributed across the U.S. population, as well (Enns et al. 2019; Muller and Wildeman 2016; Yi, Edwards, and Wildeman 2020). As has been documented extensively, those who are socially marginalized, and especially those identifying or racialized as Black, are substantially more likely to come into direct contact with this institution (e.g. Edwards, Esposito, and Lee 2018; Goel, Rao, and Shroff 2016; Pettit and Western 2004). Persons from these same groups are also more likely to enter foster care as children. As shown in Figure 1, children racialized or self-identifying as Black or American Indian/Alaskan Native are two to three times more likely than White children and several times more likely than Asian/Pacific Islander children to ever be placed in foster care.<sup>3</sup>

### **Beyond Protection and Punishment**

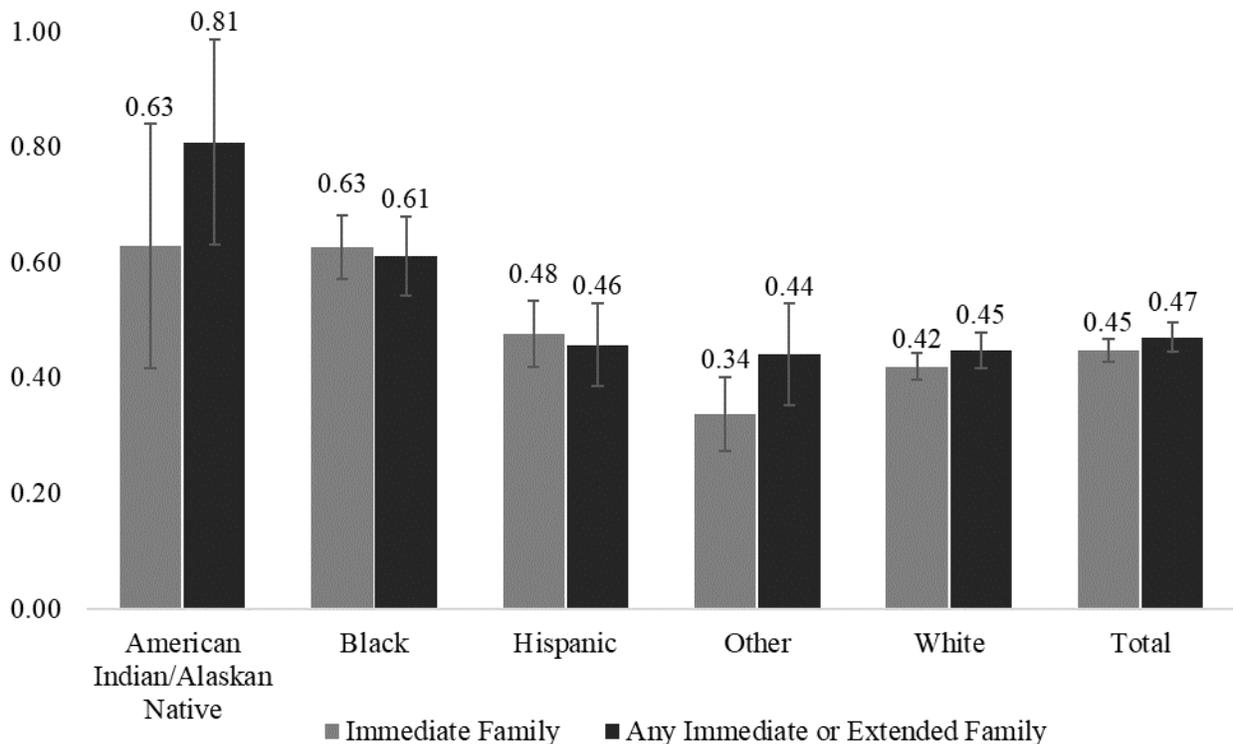
In addition to being dimensions of social inequality in their own right, experiences of institutional contact are part of the U.S. landscape of inequality in other key ways. First, direct contact with the child welfare and criminal legal systems is negatively associated with outcomes in virtually all domains of life (e.g. Gilbert et al. 2009; Wakefield and Uggen 2010). Adults who are criminal justice-involved are more likely to have been previously in poor health and socially and economically disadvantaged (Pettit and Western 2004; Wildeman and Wang 2017) and are more likely to face later adverse experiences, such as poor employment outcomes and relationship dissolution (e.g. Apel 2016; Pager 2008). Child welfare-involved youth are similarly disadvantaged, with high likelihoods of material insecurity, health problems, and parental incarceration, among other hardships (e.g. Sedlak et al. 2010; Turney and Wildeman 2017).

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<sup>3</sup> In CPS records, children's racial/ethnic identity may be inconsistently captured across agencies, caseworkers, and cases, using a combination of self-identification by the child's family of origin or the child themselves or assigned or assumed by caseworkers and agency staff (Johnson-Motoyama et al. 2018; U.S. D.H.H.S. 2016).

Another pathway through which these institutions are related to social inequality and stratification is through their impacts on those linked to the institutionally-involved. Indeed, ties to these institutions through close others is quite common. Recent research estimates that 45 percent of non-institutionalized U.S. adults have had at least one immediate family member incarcerated (Enns et al. 2019). When considering incarceration of extended family, as well, that proportion rises to 64 percent of U.S. adults, as shown in Figure 2. These experiences are substantially more common among non-Hispanic Black adults. Well over half, or 63 percent, of Black adults have ever had an immediate family member incarcerated and 80 percent have ever had any family member—immediate or extended—incarcerated for at least one night.

**Figure 2. Prevalence of Family Incarceration, 2019**



Notes: Adaptation of results from Enns et al. (2019). Estimates are weighted to be representative of the U.S. household-based adult population and capture respondents’ reports of family member jail or prison incarceration for at least one night. Error bars report 95% confidence intervals.

These social connections to institutional exposure have found to be consequential, or at the very least, strongly associated with broader patterns of disadvantage. Compared to children of parents without histories of incarceration, those of ever-incarcerated parents have substantially worse health, poorer educational outcomes, and are also more likely to be involved with the criminal legal system later in life themselves (Foster and Hagan 2015; Turney and Goodsell 2018; Wildeman, Goldman, and Turney 2018; Wildeman 2020). There is also important, though much less, work identifying negative associations of wellbeing with the criminal legal system involvement of siblings and other relations (e.g. Beckley et al. 2019; Bhuller et al. 2018).

In the case of child welfare, although the system is designed to intervene on behalf of children, their broader family (especially caregivers) and community systems are also affected, potentially positively through access to support and resources (e.g. Berger and Font 2015) or negatively through disruption of family and support networks (e.g. Roberts 2002). Even the possibility or fear of criminal legal and child welfare system involvement can shape people's navigation of daily life, by necessitating a re-orientation of parenting practices and community interactions to focus on system avoidance or minimizing risks of apprehension, for example (e.g. Elliott and Reid 2019; Clear 2007). There is also evidence, as in the case of incarceration and criminal legal system involvement, of intergenerational transmission processes in child welfare system contact; the caregivers of children confirmed as maltreatment victims were often also themselves victims of maltreatment or previously involved with child welfare services in their youth (e.g. Simon and Brooks 2017).

### **Challenges of Comparison and Inference**

The rapid expansion of sociological and demographic work on these institutions has comprehensively detailed our understanding of their roles as systems of social control that are

correlates of and risk factors for an array of adverse outcomes for children and families (e.g. Wakefield and Wildeman 2014; Turney and Goodsell 2018). However, attempts to compare children with and without experiences of institutional contact continue to face many critical challenges, with identification of causal effects of family incarceration and child welfare system involvement being even more difficult (e.g. Berger et al. 2016; Wildeman and Turney 2014).

Some of these limitations pertain to data quality and inference. First, data sources central to our current understanding of incarceration and social life are often limited by the unavailability or scarcity of precise and reliable measures of either institutional contact or outcomes and correlates of interest, such as physical health and household and family composition (e.g. Sabol 2016; Geller, Jaeger, and Pace 2016; Wildeman 2020). Existing scholarship also grapples with the difficulty of identifying appropriate comparison groups for those who are child welfare-involved, incarcerated, or experience incarceration of a loved one (e.g. Berger et al. 2016; Perry 2006; Phillips et al. 2006; Wildeman 2020). The relative disadvantage and multifaceted hardships faced by children and families exposed to the child welfare and criminal legal systems make it difficult to clarify the mechanisms through which institutional contact might shape outcomes and inequality and the magnitude of those effects. From a policy perspective, these challenges present difficulties to assessing the efficacy of policies and interventions proposed or implemented to respond to child maltreatment and crime.

Other limitations are more substantive and near-epistemological, including the relative lack of a sociological and population perspective in empirical analyses and data collection related to involvement with the child welfare system—and, to a lesser degree, the criminal legal system—and their correlates and consequences. This precludes the analysis of these systems as institutions that produce and enforce socially constructed definitions of family, childhood,

parenthood, and citizenship, and, in doing so, shape health and family inequality in the U.S. (Lerman and Weaver 2014; Pettit 2012; Putnam-Hornstein et al. 2011; Reich 2005, 2008; Simmons 2020; Wildeman and Waldfogel 2014).

### **This Dissertation**

With these limitations and persistent challenges in mind, this dissertation attempts to situate the experiences and consequences of institutional contact within our existing understanding of the broader social demography of family life and health in early life in the U.S. More specifically, this dissertation uses three quantitative analyses of survey and administrative data to examine the relationships between (1) institutional contact and the timing, cumulative risk, and nature of home-leaving in the transition to adulthood, (2) foster care placement and instability in care arrangements in childhood, and (3) paternal incarceration in gestation and infant birth outcomes. Each one grapples with persistent methodological and conceptual challenges to empirical analyses of the implications of foster care and incarceration for outcomes that have long been core to sociological and social demographic research.

The first chapter, “Leaving Home, Entering Institutions: Implications for Home-Leaving in the Transition to Adulthood,” considers the role of institutions with residential components—such as the military, higher education, and the carceral system—in shaping a key transition during a critical stage of the life course. The analysis proposes a more expansive way of conceptualizing home-leaving, one that captures experiences that are marginalized and often excluded from analysis of the timing and risk of milestones in early adulthood. It then goes on to illustrate the perhaps unsurprising but important social disparities in home-leaving that are missed in studies of the departure from the parental home. I do so by estimating life tables of first departure from the parental home by age 31, using nationally representative data from the

1997 National Longitudinal Survey of Youth. I find that including non-household-based settings (i.e. correctional facilities, military barracks, college dormitories) yields earlier estimates of the timing of first home-leaving than conventional measures, particularly for young adults who identify as non-Hispanic and Black and those with lower levels of parental educational attainment. This article was published in the *Journal of Marriage and Family* in 2019 (Yi 2019).

The second chapter, “Instability in Care and Living Arrangements: Putting Foster Youth in Context,” examines change and instability in care and living arrangements of children who are and are not placed in foster care after investigation as potential victims of child maltreatment. This analysis uses data from a nationally representative panel data set of children investigated for child maltreatment, the National Survey of Child and Adolescent Wellbeing, and inverse probability-weighted regression methods to compare the experiences of foster and non-foster youth. I find, overall, that children ever in foster care are substantially more likely to experience changes in their primary caregivers, the availability of a secondary caregiver, and their living arrangements. However, when considering only excess instability—or changes other than children’s initial removal from unsafe environments and transitions back to permanent settings—foster youth have more stability in their living arrangements while continuing to experience greater instability in their primary caregiving relationships. Although family, household, and care arrangement change has been studied extensively in other work, this is perhaps the first analysis to draw direct comparisons of those experiences between foster and non-foster youth. The implications of this work hold particular significance for evaluating the efficacy of foster care placement as a tool for establishing stability and safety in the family lives of vulnerable children.

The third article, “Estimating Causal Effects of Paternal Incarceration on Infant Birth Outcomes in New York City” analyzes linked birth, death, and jail records to estimate

associations of paternal incarceration on key infant birth outcomes within the population of live births in New York City. In addition to using several modeling strategies, the analysis leverages variation in the timing of paternal jail incarceration relative to an infant's birth to construct comparison groups that are statistically indistinguishable on observable characteristics, moving towards plausibly causal estimates of gestational paternal incarceration. The results of this investigation run counter to prior scholarship that consistently estimates negative descriptive associations and effects of paternal incarceration on child health and wellbeing (e.g. Wakefield and Wildeman 2014; Wildeman, Goldman, and Turney 2018). I find that paternal jail incarceration during the gestational period is modestly associated with lower predicted probabilities of preterm birth and admission to a neonatal intensive care unit, with no evidence of variation in this relationship across racial/ethnic groups or with respect to differences in the frequency, timing, duration, and conditions of paternal jail incarceration.

Taken together, this dissertation makes a few overarching contributions to our understanding of the implications of child welfare and criminal legal system involvement in the U.S. First, these analyses provide new insights about the ways in which foster care placement and own and paternal incarceration are woven through and impact the early life course, sometimes in counterintuitive ways. Second, each chapter leverages data that are either new or scarcely used in sociological and demographic scholarship and/or innovative measures and comparison groups to elucidate the relationship between institutional contact and consequences for life transitions, living arrangement instability, and health. In this way, this dissertation not only engages long-standing challenges of internal and external validity in research on the foster care and incarceration (e.g. Berger et al. 2016; Courtney 2000; Wildeman and Turney 2014), but also proposes and demonstrates methodological strategies for doing so.

Finally, the empirical findings and lingering questions developed in this dissertation highlight key areas for additional investigation. My work points towards three themes and topics, in particular. Future research should elucidate the data collection and production processes that produce administrative records related to institutional contact and consider how those processes—and assumptions about them—might shape our estimates of the prevalence, distribution, and consequences of those experiences. Future work should also continue to explore heterogeneity in the implications of institutional contact for wellbeing and life chances with respect to the timing, and sequencing of institutional exposure over the life course, as well as the integral role those systems of social control currently and will continue to play in shaping the institution of the family and inequality therein.

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# CHAPTER I: LEAVING HOME, ENTERING INSTITUTIONS: IMPLICATIONS FOR HOME-LEAVING IN THE TRANSITION TO ADULTHOOD

## Abstract

The departure from the parental home is considered an important milestone in the transition to adulthood. However, studies of the timing, prevalence, and nature of home-leaving do not generally incorporate the full range of household-based and non-household-based experiences that young adults face during this period of the life course—such as military service and incarceration. With the growing prevalence and uneven distribution of these institutional experiences, consideration of the role these institutions play in the home-leaving transition becomes increasingly important to our understanding of this life event. This study conducts life table analysis of the 1997 National Longitudinal Survey of Youth (N=6,501) to estimate overall and race/ethnicity-specific cumulative risks of first home-leaving by age 31, using two measures of home-leaving: one that relies exclusively on household rosters and a second that incorporates information about departures for institutions beyond the family and household. Multinomial regression is then used to estimate young adults' likelihoods of first home-leaving for different types of arrangements and institutional settings. The author finds that the institution-inclusive definition of home-leaving yields earlier and higher overall risks of first home-leaving by age 27 and that there is meaningful racial/ethnic variation in the types of transitions young adults make when they leave the parental home for the first time. These results indicate that variations in the definition of "adult" transitions can impact conclusions about the prevalence, timing, and racial/ethnic variation in key life events.

## **Introduction**

During the transition to adulthood, young people become increasingly independent from their parents and childhood contexts. In the United States (U.S.), this includes departure from the parental home (Furstenberg et al. 2004; Settersten and Ray 2010). A large multidisciplinary body of research has detailed the many facets of this transition, including its timing and prevalence and changes therein associated with broader social shifts, such as women's higher rates of labor force participation (e.g. Goldscheider and Goldscheider 1999; van den Berg, Kalmijn, and Leopold 2018). These studies have focused predominantly on parental co-residence and family/household formation, with less consideration of the impacts of non-familial and non-household institutions—such as the military, criminal justice system, and postsecondary education—on this milestone. In this study, I suggest that an institutional perspective that combines exploration of dynamics around the parental home with our knowledge of the expansion and unequal distribution of young adults' institutional contact is important to our understanding of racial/ethnic inequality and stratification early in the life course.

Recent trends in the transition to adulthood provide compelling empirical motivation for revisiting our definition of home-leaving. First, over recent decades, the criminal justice and higher education systems, two key social institutions with residential aspects (e.g. on-campus housing and prisons), have expanded rapidly and penetrated the life course to a historically unprecedented degree (Pettit and Western 2004; Wakefield and Wildeman 2013). Second, this institutional reach is consequential, linked to residential attainment, family and relationship processes, and economic (in)security (Herbert, Morenoff, and Harding 2015; Maroto 2015; Turney 2015; Warner 2015). Third, institutional contact is unevenly distributed across racial/ethnic, socioeconomic, and gender groups (Pettit and Western 2004; Rumbaut 2007). As

these trends and their consequences continue to unfold, a simultaneous consideration of these experiences becomes increasingly important to our understanding of the transition to adulthood.

This study uses cohort life table and multinomial regression analysis of the 1997 National Longitudinal Survey of Youth (NLSY97; U.S. Bureau of Labor Statistics, U.S. Department of Labor, 2015) to (1) illustrate the impact of institutions on our estimation of the timing and cumulative risk of first home-leaving in the transition to adulthood and (2) examine racial/ethnic differences in the nature of this milestone. The analysis centers on the comparison of “roster-based” and “institution-inclusive” measures of home-leaving. The latter includes transitions to postsecondary education, military service/training, incarceration, group homes, and homelessness to examine how the ways we conceptualize home-leaving and “adult” contexts in early life shapes insights about the timing and nature of home-leaving experiences and its patterning across racial/ethnic groups.

In this analysis of U.S. resident young adults, I find that an institution-inclusive perspective yields higher cumulative risk estimates of first home-leaving by age 31 and that young adults first depart at younger ages than described by a standard household-based definition of the first home-leaving event. Additionally, in using an institution-inclusive measure of home-leaving, I find that the contexts to which young adults first leave the parental home differ across racial/ethnic groups. Hispanic young adults are substantially less likely than those who are White or Black to leave the parental home for the first time for college and more likely to leave to enter living arrangements with non-parental others. Black young adults are more likely than White and Hispanic young adults to first leave the parental home for incarceration.

## **Theoretical Background**

### ***The Sociocultural Context of Home-Leaving***

Home-leaving in the transition to adulthood has long been a core topic for those examining the life course (Goldscheider et al. 1999; Guttman, Pullum-Piñón, and Pullum 2002). This research estimates the timing and prevalence of this transition, detailing social variation therein (e.g. Seltzer and Bianchi 2013; White 1994). Taken together, this literature finds that most young adults have left the parental home at least once (e.g. Dey and Pierret 2014) but that the timing of this departure varies (Furstenberg et al. 2004; Settersten and Ray 2010). Black, Hispanic, and economically disadvantaged young adults are more likely to be living with parents and to leave the parental home later than their White, more advantaged peers (e.g. Britton 2013; Houle and Warner 2017; Lei and South 2016; Sandberg-Thoma et al. 2015). Additionally, young adults are leaving the parental home later and returning at higher rates, although most consider home-leaving a key milestone for adulthood (Furstenberg et al. 2004; Sandberg et al. 2015).

Studies have varied in their measurement of home-leaving, exploring shifts in this transition as components and reflections of sociocultural and economic change. Beginning in the 1970s, home-leaving grew increasingly associated with non-marital events—such as labor force entry and non-marital cohabitation—motivating an updated exploration of the precipitating events and conditions around departures from the parental home (e.g. Goldscheider and Goldscheider 1999; Kahn, García-Manglano, and Goldscheider 2017). Contemporary definitions of what constitutes a departure from the parental home range from stricter definitions that equate it with the establishment of an independent residence (e.g. Buck and Scott 1993) to broader ones that include temporary or “semi-independent” transitions such as leaving for college or to live with roommates (e.g. Goldscheider and DaVanzo 1989; Houle and Warner 2017).

The ways in which these settings are described reflect the idea that an “adult” has permanently transitioned into a family and/or household living arrangement that is socially and physically distinct from one’s parents as well as custodial or non-familial shared or custodial settings (Berzin and DeMarco 2010; Qian 2012). For example, although research always considered cohabitation with a marital partner an “adult” living arrangement, home-leaving studies often question the “adulthood” of an individual in other non-romantic co-residential arrangements, although these arrangements are not uncommon among adults in the U.S. today (i.e. living with non-relatives; U.S. Census Bureau 2018).

### ***Institutions in the Transition to Adulthood***

With this recognition of the sociocultural contingency of the definition and measurement of home-leaving, we can also reconsider the role of non-familial/non-household institutions in analyzing and describing the home-leaving experience. Families and households are not the only institutions in which individuals become embedded in young adulthood, not the only institutions that affect their living arrangements and residential trajectories, and not the only institutions that structure their first adult social roles and positions. As presented in Bronfenbrenner’s ecological conceptualization of the transition to adulthood, with age, an individual is more likely to come into contact with these other institutions and systems as they transition from having life experiences centered on the family of origin (Bronfenbrenner 1977, 1986).

More specifically, as people approach and turn age 18 in the U.S., they become eligible for military service, can begin postsecondary education, are considered adults in the criminal justice system (and are thus at higher risk of incarceration), and leave the legal purview of their parents and childhood caregivers. With expanded pursuit of postsecondary schooling and reach of the criminal justice system, likelihoods of entry into these contexts have increased, although

differentially so. For example, while upper- and middle-income White women are more likely to go to college and to move to do so (DiPrete and Buchmann 2006; Ovink and Kalogrides 2015), low-income Black men are at higher risk of ever being incarcerated than those of other groups (Pettit and Western 2004), and men, African Americans, and those of lower socioeconomic status are more likely to enlist in the military (McLean and Elder 2007; Seeborg 1994).

These experiences and transitions have long been studied as part of young people's entry into adult social positions and roles over the life course (e.g. Hogan and Astone 1986; Kelty, Kelykamp, and Segal 2010; Rumbaut 2007). However, these "institutional transitions," with the exception of transitions to higher education (e.g. De Jong-Gierveld, Liefbroer, and Beekink 1991; Goldscheider, Thornton, and Young-DeMarko 1993; Mulder and Clark 2002), have generally been examined independently of the home-leaving experience, although they, like departures from the parental home and into new families and households, are linked to shifts into adult roles and responsibilities (Britton 2013; Raphael 2007; Warner 2015).

### ***Conceptualization and Measurement of Home-Leaving Events***

This disconnect between studies of home-leaving and studies of other institutions in the transition to adulthood is indeed in part due to a normative conceptualization of home-leaving and what types of life events qualify as an adult transition away from the home. However, the relative lack of attention to the interaction between these social institutions and home-leaving pathways is also in part due to the characteristics of the data currently available to look at these residential patterns. Goldscheider and Waite (1993, 219) provide a detailed discussion of this feature of the 1966-1980 National Longitudinal Surveys of Young Men and Young Women (NLS), noting the post-survey linkage of respondents back to primary residences and the need to

take this into consideration when examining young adults' residential mobility. This feature is shared within the broader family of NLS studies used in research on home-leaving.

In the 1979 and 1997 National Longitudinal Surveys of Youth (NLSY), for example, if a respondent was not in the primary residence because they were away at college at the time of interview, the linking process reports them as co-residing with individuals named in the household roster of the primary residence, while simultaneously using 4-year college enrollment as criteria for a respondent to qualify as “independent” in the 1979 NLSY (U.S. Bureau of Labor Statistics, U.S. Department of Labor, n.d.; Goldscheider and Waite 1993). This is an indicator of some incongruence between our conceptualization of adulthood and how it is or is not reflected in measurement of other transition to adulthood events. Other surveys spanning this life stage that have been used to study home-leaving, such as the Panel Study of Income Dynamics (PSID) and the National Longitudinal Study of Adolescent to Adult Health (Add Health), face both related and additional challenges to the integration of institutional transitions, such as non-interview of institutionalized panelists (e.g. PSID; Lei and South 2016) and less frequent data collection (e.g. Add Health).

Standard practices in home-leaving research, then, often mark periods of incarceration, military barracks, or entry into shared living arrangements in college during the transition to adulthood as time still in the parental home, although these very experiences are considered markers of or turning points into adulthood. The degree to which this is a concern or challenge for research on the transition to adulthood varies by study objective. If focused strictly on household and family formation, this may be a non-issue. However, there are a couple key reasons to revisit the standard definition and measurement of departures from the parental home.

First, there is much evidence that the transition to adulthood is changing as a function of social institutions beyond the family and household. This is similar to the role that college-going and women's rising labor force participation played in the changing departure from the parental home in the mid-to-late-20<sup>th</sup> century. Second, entry into these institutions plays a role in shaping residential attainment and trajectories in ways that are consequential for family and household formation, among other events and milestones that have long been known to be linked to the home-leaving event. A reconsideration of ways to measure and define home-leaving could serve to update our view of this experience to reflect the diverse ways in which young adults move out of their childhood homes and provide insights about the relationship between institutions and living arrangements of young adults in this critical stage of the life course.

This study proposes and explores a novel definition of home-leaving that accounts for institutional contact as an important aspect of the transition to adulthood. Here, I focus on first departures from the parental home that introduce physical residential separation between the young adult and their parent(s) or place the individual in an adult position (i.e. a householder living with a parent). This analysis focuses on first home-leaving because the life course is path-dependent and subject to the cumulative impacts of experience; the initial context of home-leaving is therefore likely to be consequential in shaping later opportunities and constraints. Also, although returns and subsequent departures will be important to investigate in future work, a focus on the first event makes a full exploration of this new definition of home-leaving tractable.

This article centers on two questions: first, how does the inclusion of institutional transitions into our measurement of home-leaving shape our understanding of the timing and risk of first departures from the parental home? Second, do conclusions about racial/ethnic

differences in home-leaving experiences differ across measures, and, if so, how? I anticipate that the inclusion of institutional transitions will yield higher estimates of any first home-leaving and earlier average home-leaving in the transition to adulthood. Given racial/ethnic variation in postsecondary education matriculation, military service, and criminal justice system contact, I also expect that the inclusion of institutions in defining the home-leaving event will facilitate a closer examination of important variation in young adults' pathways out of the parental home.

## **Data, Measures, and Methods**

### ***Data and Sample***

To examine young adult's home-leaving experiences, I draw on a data set that has been used extensively for the study of this life event: the 1997 National Longitudinal Survey of Youth (NLSY97). In so doing, I place this analysis in dialogue with a rich literature on home-leaving in the U.S. that has also drawn on these data. The NLSY97 is furnished by the U.S. Bureau of Labor Statistics and follows a sample of adults that was nationally representative of the U.S. non-institutionalized population aged 12-16 as of December 31, 1996 (U.S. Bureau of Labor Statistics, U.S. Department of Labor, 2015). It surveys individuals as they move into and through early adulthood and collects detailed information about their household and living arrangements, making it appropriate for this study of residential experiences in the transition to adulthood.

The analyses presented here follow these individuals from 1997 through 2015, the most recent year of data available, when observed respondents were aged 30 to 36 years. The analytic sample is restricted to those with information on their own race and ethnicity, sex, disability status, U.S. citizenship status, and at least one parent's educational attainment, resulting in a loss of 891 respondents. I also restrict the analysis to those who reported living with at least one

parent (biological, adoptive, or step-parent) in the first round; 95.4% of all respondents met this criterion and those who did not were already excluded due to covariate missingness, leading to no additional loss in the analytic sample. The sample is restricted further to those who are observed at three points in time: at baseline (1997), in the last round (2015), and at least once in between, resulting in a final analytic sample of 6,501 individuals, or 72% of the full baseline NLSY97 sample of 8,984 individuals. A statistical comparison of the characteristics of the full NLSY97 and analytic sample indicates that the analytic sample is extremely similar to the full sample of respondents (Appendix Table 1A.1).

### ***Measuring First Departures from the Parental Home***

I compare two different measurements of departures from the parental home, called the *roster-based* and *institutional-inclusive* measures. For the roster-based approach, young adults are coded as living with a parent(s) at each year of the survey if they name at least one parent in the household roster in that round. This is consistent with prior work that uses independent/own residence as the basis for identification of a home-leaving event (e.g. Ruggles 2007; Sandberg-Thoma, Snyder, and Jang 2015). The second approach, the *institution-inclusive* measure, modifies the practice of linking youth back to primary residences in the NLSY97 by incorporating information about other respondent statuses and experiences at the time of survey that affect their residential conditions but are not captured by the household roster.

Beginning with the same foundation as the roster-based measure, I use an array of additional measures describing the respondent's residential context. This information allows me to determine whether the respondent is incarcerated (jail/prison); away for postsecondary education (dormitories, fraternities/sororities, university-sponsored units); living in military barracks; in a group home, treatment center, or shelter; has died; or has become a householder

(renter/owner, even if a parent is a household member) or is living in a household in which their parent is not a member, according to the household roster. These categories are mutually exclusive; an individual can only experience a first home-leaving event via one of these pathways. This coding procedure is detailed further in Appendix 1B.

A noteworthy distinction between the institution-inclusive measure of home-leaving and those observed in prior work on this topic is that here, transitions away from the parental home to establish one's own household ("independent" pathway) capture the types of departures that often serve as the focus of home-leaving research, such as departures to establish one's own household or moving in with other adult roommates. In the institution-inclusive measurement, those who report living in housing for postsecondary schooling, incarceration facility, military housing, in an independent household, or group home/treatment center/shelter are coded as having left the parental home, although the household roster may indicate the presence of a parental figure in the primary household. In the roster-based approach, however, these same individuals would be described as living with their parent(s) (Goldscheider and Waite 1993). By incorporating institutional information, this measurement may provide a richer illustration of the living arrangements and diversity of social positions and contexts, favorable or not, that characterize early adulthood in the United States.

### ***Key Characteristics***

In addition to exploring the implications of this conceptual shift for our understanding of overall trends in home-leaving in early adulthood, I am interested in examining the consequences of incorporating institutional transitions for our understanding of racial/ethnic variation in that experience. Therefore, I also examine home-leaving risks and pathways with respect to racial/ethnic identification, accounting for the ways that racial stratification may interact with

variation across parental education, sex, nativity, and disability status groups. A respondent's *race/ethnicity* is captured using measures of the child's self-reported race and Hispanic ethnicity to be grouped into one of the following mutually-exclusive categories: non-Hispanic White, non-Hispanic Black, Hispanic, and non-Hispanic other race. I use *parental education* as an indicator of socioeconomic background; this is a dichotomous measure that reports the number of years of schooling obtained by the respondent's biological and/or residential parents, less than 12 years of schooling or 12 year or more years. If more than one parent's information was available, this measure reports the maximum number of years of schooling reported by any one parent. *Sex* is a binary indicator that notes whether the respondent was identified as male or female at baseline. As the NLSY97 survey instrument uses the term "sex" rather than "gender" for this item, I also use the term "sex" (U.S. Bureau of Labor Statistics, U.S. Department of Labor, 2012). *U.S. citizenship* is identified using a baseline measure of whether the respondent was born in the U.S. (a birthright U.S. citizen) and follow-up measures in the 2001-2004 and 2006-2015 surveys that update their citizenship status. *Disability status* draws on measures in the 1997 and 2002 surveys and identifies whether the respondent or their parents report that the respondent had a sensory, physical, emotional/mental, cognitive, or chronic health condition that limited their daily work and school activities.

### ***Analytical Approach***

To examine how an institution-inclusive conceptualization of home-leaving might impact our understanding of the timing and prevalence of first departures from the parental home in the transition to adulthood, I use cohort life tables (Preston, Heuveline, and Guillot 2001). Life tables have most often been used to examine mortality and health over the life span but can also be used to study other events. Examples of other applications of this method include studies of entry

into poverty (Rank and Hirschl 1999) and children's risks of foster care placement (Wildeman and Emanuel 2014). In this study, the event of interest is the respondents' first departure from the parental home, measured annually until 2011 and biennially from 2011-2015 in the NLSY97.

I determine whether the respondent has yet left their parents' home for the first time at each age using the roster-based and institution-inclusive measures described above to construct two separate sets of life tables. In both life tables, a respondent remains a member of the sample "at risk" of first home-leaving in the next age interval of the life table until the age at which they are first identified as not living with a parent. Upon first measurement as having left the parental home, they are removed from the "risk pool" of potential first-time home-leavers for subsequent age intervals. For example, if an individual entering the study at age 18 is identified as living in the parental home at ages 19 and 20, they will remain in the denominator of the risk estimate for the whole sample at ages 19 and 20. If then identified as not living with a parent at age 21, they are removed from the sample at risk of first departure for the rest of the study period.

Aggregating up from this individual-level information, I calculate age-specific counts of those leaving the parental home for the first time, those at risk of doing so, and those who are no longer at risk. These three pieces of information form the core of the life tables: the hazard functions that yield age-specific and cumulative risks of first departure by age 31. The life tables begin at 14 because no one in the NLSY97 living with at least one parent in 1997 reported leaving the parental home before age 14, either because the NLSY97 began after they turned 15 or they did not leave before 14. The life tables end at 30, the age through which all respondents have aged by the 2015 interview. Those observed beyond the age of 30 and not observed residing apart from their parent(s) by then are coded as not having left by age 31.

Using these steps, two sets of life tables are estimated: one using the roster-based definition of home-leaving and the second using the institution-inclusive definition. Again, the institution-inclusive measure includes home-leaving for higher education, military service, incarceration, independent living (own rental/owned housing), and other social institutions (group home, treatment center, shelter). For both sets of life tables, I account for exit from the NLSY97 study due to the respondent's death between survey rounds; ultimately, this does not affect the analyses as all respondents in the analytic sample leave the parental home via another experience prior to any recorded death.

In addition to estimating these risks for the full analytic sample, I estimate risks by race/ethnicity to explore how this expanded definition of home-leaving might differentially impact our understanding of this life transition for young adults of different racial/ethnic groups. Estimates for all respondents are weighted to account for the complex sampling design of the NLSY97. By stratifying the life table analysis with respect to race/ethnicity, the group-specific estimates account directly for dimensions along which the NLSY97 sampling weights are based; as such, race/ethnicity-specific estimates are unweighted (Bollen et al. 2016; Solon, Haider, and Wooldridge 2015). I estimate 95% confidence intervals using Greenwood's method, which allow me to assess whether the estimates are statistically distinguishable between measures and racial/ethnic groups (Greenwood 1926).

Finally, to examine variation in young adults' first home-leaving experiences, I use unweighted multinomial logistic models to examine racial/ethnic variation in the likelihood that an individual's first home-leaving pathway is to a context other than an independent household (own/rent, alone/shared). I estimate three models: (1) a baseline model that predicts young adults' home-leaving pathway as a function of their racial/ethnic group as well as their baseline

age and the number of NLSY97 rounds they are observed, (2) a full model that adjusts for other key characteristics and contexts that may be associated with a respondent's home-leaving pathway (sex, parental education, U.S. citizenship, and disability status), and (3) an interacted model that adds interaction terms of race/ethnicity with sex and parental education.

## **Results**

A weighted statistical description of the sample is presented in Table 1.1. The top panel shows that just under half of the sample identified as female (49%) and that the majority of the sample identified as non-Hispanic White (69%). Fourteen and 13 percent of the analytic sample identified as non-Hispanic Black and Hispanic, respectively. These individuals' average number of years of parental schooling at baseline was 14 years. Analytic sample members had an average baseline age of 14 years old and was observed in nearly all NLSY97 rounds (14 of 15 rounds).

The bottom panel presents a description of respondents' initial exits from the parental home, as captured by the two measures of home-leaving described above. The institution-inclusive measure estimated that a higher proportion of young adults had left the parental home for the first time by age 31 (97%) than the roster-based measure (89%) and that this event was taking place earlier, on average, than estimated using the roster-based measure (21 years vs. 22 years of age). Three-quarters of those who left the parental home at least once by the age of 31 left to independent household settings (76%), the focus of most home-leaving research to date. However, over one-quarter of young adults exited the parental home via other experiences. Eighteen percent of young adults first left for residential arrangements tied to their pursuit of postsecondary education (dormitories, fraternities/sororities), four percent moved to military barracks for service and training, two percent left for incarceration, and under two percent had

not yet left at the age of 31. The remainder of this analysis explores the empirical consequences of integrating perspectives on young adult residential transitions and institutional contact for our understanding of the home-leaving experience as well as racial/ethnic disparities therein.

**Table 1.1. Analytic Sample**

	<u>Mean (SD) or Proportion</u>
<b>Social/Demographic Characteristics</b>	
Female	0.49
Race/Ethnicity	
White, Non-Hispanic	0.69
Black, Non-Hispanic	0.14
Hispanic	0.13
Other, Non-Hispanic	0.05
Parental Education (Years)	13.69 (2.88)
12 Years or Less	0.44
13 Years or More	0.57
U.S. Citizenship Status	0.97
Disability Status (1 = has disability)	0.13
Age at 1 <sup>st</sup> Observation (in 1997)	14.35 (1.50)
Rounds Observed	14.12 (2.00)
<b>Home-Leaving Characteristics</b>	
Left Parental Home by 31	
Roster-Based Measure	0.89
Institution-Inclusive Measure	0.97
Age at 1 <sup>st</sup> Home-Leaving <sup>+</sup>	
Roster-Based Measure	21.64 (3.02)
Institution-Inclusive Measure	20.56 (3.08)
Pathway to 1 <sup>st</sup> Home-Leaving	
Independence	0.76
College	0.18
Military	0.04
Jail/Prison	0.02
Group Home/Homeless	0.00
Not Yet Departed by 31	0.02

Notes: N=6,501. Standard deviations are reported in parentheses. Estimates are weighted.

<sup>+</sup>Indicates measures that apply only to those who have ever left the parental home over the study period, for which the sample sizes were 5,934 and 6,393 for the roster-based and institution-inclusive measures, respectively.

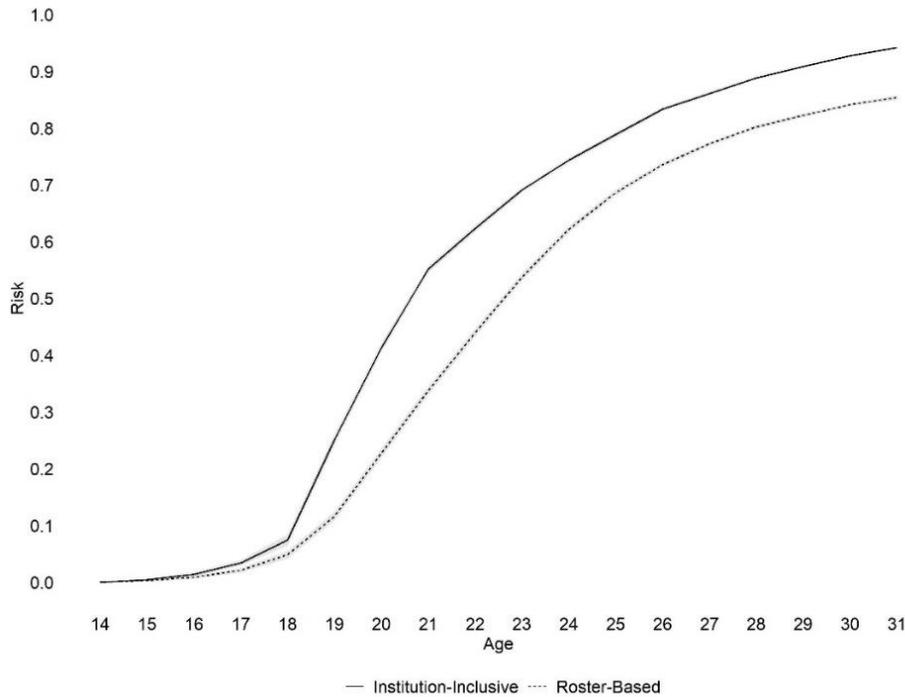
### *Cumulative Risks of First Home-Leaving by Age 31*

To examine how the inclusion of institutional transitions might impact our understanding of the risk and timing of first departures from the parental home in the transition to adulthood, I compared cumulative risks of first home-leaving estimated using cohort life tables and the roster-based and institution-inclusive definitions of home-leaving described above. Figure 1 presents the risk plots for these results, with corresponding estimates and confidence intervals reported in Appendix Table 1A.2. The discrepancy between the risk curves is stark. The roster-based estimates yielded a gradual increase in first home-leaving with age, with a slight acceleration in early years of adulthood (beginning at age 18). However, with the inclusion of residential transitions to postsecondary, carceral, military, and other non-household/family institutions, the risk of first home-leaving spiked between 18 and 21, yielding higher cumulative risks of first home-leaving than the roster-based measure for the duration of the study period. At ages younger than 17 years, the cumulative risk of first home-leaving did not differ statistically across measures. However, by the late teens, the institution-inclusive measure yielded substantially and statistically higher estimates of first home-leaving: the roster-based measure estimated that by age 18, five percent of respondents had left the parental home while the institution-inclusive measure estimated that seven percent had experienced that event.

As shown thus far, it was reasonable to expect that the inclusion of institutional transitions would yield higher risks of first home-leaving when young adults are first eligible to enroll in the military or begin postsecondary education. It would also be reasonable to expect that these two sets of estimates might converge, indicating that those who do not leave their parents' homes early in this life stage for institutions later "catch up" in leaving the home for the first time as they make family and household transitions. However, the persistent discrepancy

between the two sets of estimates indicates otherwise: at age 31, the roster-based and institution-inclusive measures estimated that 85% and 94% had ever left the parental home, respectively.

**Figure 1.1. Cumulative Risk of First Home-Leaving, By Measure**



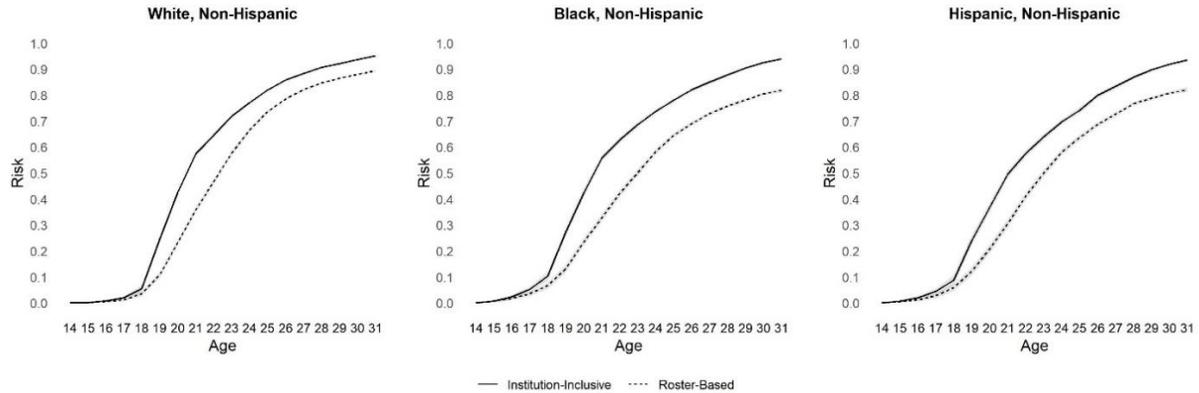
Notes: N=6,501. Estimates are weighted.

### ***Racial/Ethnic Variation in First Home-Leaving***

The analyses discussed so far illustrated the implications of incorporating institutional transitions into our conceptualization and measurement of events constituting home-leaving in the transition to adulthood. However, it is possible that the salience of such experiences varies across racial/ethnic groups, meaning that institutions may be differentially impactful on home-leaving timing and risk. Given what we already know about disparities in institutional contact and matriculation with respect to race/ethnicity, gender, socioeconomic background, and other dimensions of social difference, this is highly plausible (e.g. Pettit and Western 2004; Ovink and Kalogrides 2015). To explore this possibility with respect to racial/ethnic differences, I turned to

cumulative risk estimates using the roster-based and institution-inclusive measures of home-leaving by race/ethnicity, the results of which are presented in Figures 1.2 and 1.3 and in tabular form in Appendix Table 1A.3.

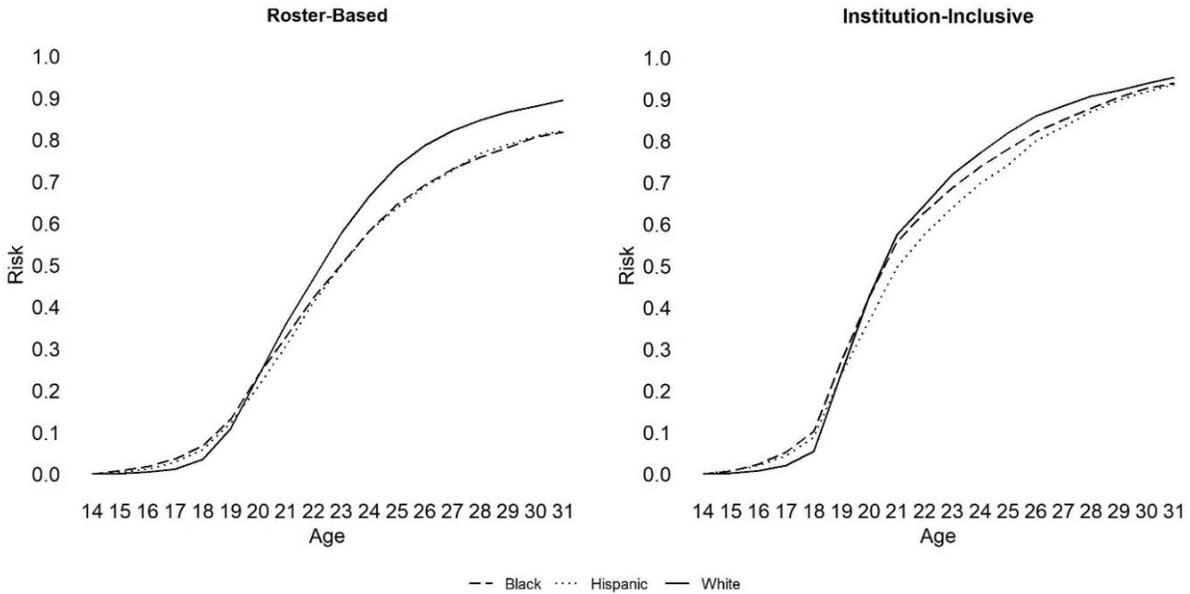
**Figure 1.2. Cumulative Risks of First Home-Leaving, By Measure and Race/Ethnicity**



Notes: N = 6,288. Estimates are unweighted and omit respondents of Other race/ethnicity.

A comparison of these race/ethnicity-specific roster-based and institution-inclusive cumulative risk estimates again shows that the latter yielded higher estimates of home-leaving risks and earlier estimated first departures (Figure 1.2). This held true for all racial/ethnic groups, although the degree to which the inclusion of institutions drove up home-leaving risks varied. In addition to differences in estimates of cumulative risks across the two measures, the two measures varied in their description of racial differences or inequalities in home-leaving risks.

**Figure 1.3. Cumulative Risks of First Home-Leaving, By Measure and Race/Ethnicity**



Notes: N = 6,288. Estimates are unweighted and omit respondents of Other race/ethnicity.

By re-arranging the risk curves to allow for comparisons within each home-leaving measure across racial/ethnic groups, as in Figure 1.3, it was possible to examine whether the two measures presented different portraits of racial/ethnic differences in home-leaving timing. The left panel shows that according to the roster-based measure, the timing of first departures from the parental home was distinct for White young adults, as they were less likely to leave the parental home early in this life stage compared to those who were Black or Hispanic. However, in the early twenties, the race/ethnicity-specific risks cross, with White respondents' cumulative risk of first departure exceeding that of others for the duration of the transition to adulthood.

With the inclusion of institutional transitions, White young adults' risk of first home-leaving later in the transition to adulthood again exceeded those of other racial/ethnic groups, with a similar, though earlier, crossover in cumulative risks (right panel, Figure 1.3). However, unlike the roster-based measure of home-leaving, institution-inclusive risks of departure among

Black and Hispanic young adults remained distinct in the middle years, converging only at the end of the study period. The institution-inclusive measure, thus, estimated more racial/ethnic variation in home-leaving risks in the transition to adulthood than the binary White-non-White contrast well-documented in prior home-leaving research and identified in the roster-based analysis.

### ***Racial/Ethnic Differences in Pathways of First Home-Leaving***

At this point, it is clear that the inclusion of institutions presents a somewhat different portrait of the timing, overall risk, and racial/ethnic differences in first departures from the parental home than a roster-based or family/household-based definition of home-leaving. But in addition to differences in timing and risk, an institutional approach that considered the family and household as part of a broader suite of social institutions that shape the transition to adulthood will also pick up differences in the nature of young adults' home-leaving experiences. This variation can be both symptomatic and predictive of stark inequalities in wellbeing and social inclusion or exclusion in early adulthood.

To explore this variation in home-leaving pathways across racial/ethnic groups, I now turn to unweighted multinomial logistic regression models. These models expressed likelihoods of experiencing different types of first departures from the parental home as functions of one's racial/ethnic group and other characteristics that are associated with various forms of institutional contact. This discussion focuses on racial/ethnic differences in home-leaving pathways with results presented in Table 1.2; full results are reported in Appendix Table 1A.4. Because coefficients estimated using categorical models are not comparable across specifications, all results are reported as predicted probabilities estimated with covariates set at observed values (Allison 1999; Long and Mustillo 2018).

**Table 1.2. Multinomial Regression Models of First Transition from the Parental Home**

<b>Model 1</b>						
	<b>Independent</b>	<b>College</b>	<b>Military</b>	<b>Jail/ Prison</b>	<b>Group Home/ Homeless</b>	<b>Not Yet Departed by 31</b>
<b>Race/Ethnicity</b>						
White	0.74 <sup>H</sup> (0.01)	0.20 <sup>B,H</sup> (0.01)	0.04 (0.00)	0.01 <sup>B</sup> (0.00)	0.00 <sup>B</sup> (0.01)	0.01 <sup>B</sup> (0.00)
Black	0.77 <sup>H</sup> (0.01)	0.13 <sup>W,H</sup> (0.01)	0.03 (0.00)	0.05 <sup>W,H,O</sup> (0.00)	0.00 <sup>W,O</sup> (0.00)	0.02 <sup>W</sup> (0.00)
Hispanic	0.88 <sup>W,B,O</sup> (0.01)	0.06 <sup>W,B,O</sup> (0.01)	0.03 (0.01)	0.02 <sup>B</sup> (0.00)	0.00 (0.00)	0.02 (0.00)
Other	0.75 <sup>H</sup> (0.03)	0.18 <sup>H</sup> (0.03)	0.02 (0.01)	0.00 <sup>B</sup> (0.00)	0.00 <sup>B</sup> (0.00)	0.03 (0.01)
Age, Observed Rounds				X		
Sociodemographic Characteristics						
Race Interactions						
<b>Model 2</b>						
<b>Race/Ethnicity</b>						
White	0.76 <sup>H</sup> (0.01)	0.18 <sup>B,H</sup> (0.01)	0.04 (0.00)	0.01 <sup>B</sup> (0.00)	0.00 (0.00)	0.01 <sup>B</sup> (0.00)
Black	0.75 <sup>H</sup> (0.01)	0.15 <sup>W,H</sup> (0.01)	0.03 (0.00)	0.04 <sup>W,H</sup> (0.01)	0.00 (0.00)	0.02 <sup>W</sup> (0.00)
Hispanic	0.86 <sup>W,B,O</sup> (0.01)	0.07 <sup>W,B,O</sup> (0.01)	0.03 (0.01)	0.02 <sup>B</sup> (0.00)	0.00 (0.00)	0.02 (0.00)
Other	0.77 <sup>H</sup> (0.03)	0.15 <sup>H</sup> (0.02)	0.02 (0.01)	0.02 (0.01)	0.00 (0.00)	0.03 (0.01)
Age, Observed Rounds				X		
Sociodemographic Characteristics				X		
Race Interactions						

*(continued on next page)*

**Table 1.2, continued**

	<b>Model 3</b>					
<b>Race/Ethnicity</b>						
White	0.76 <sup>H</sup>	0.18 <sup>B,H</sup>	0.04	0.01 <sup>B</sup>	0.00	0.01 <sup>B</sup>
	(0.01)	(0.01)	(0.00)	(0.00)	na	(0.00)
Black	0.76 <sup>H</sup>	0.14 <sup>W,H</sup>	0.03	0.04 <sup>W,H</sup>	0.00	0.02 <sup>W</sup>
	(0.01)	(0.01)	(0.00)	(0.01)	na	(0.00)
Hispanic	0.86 <sup>W,B,O</sup>	0.07 <sup>W,B,O</sup>	0.03	0.02 <sup>B</sup>	0.00	0.02
	(0.01)	(0.01)	(0.01)	(0.00)	na	(0.01)
Other	0.77 <sup>H</sup>	0.15 <sup>H</sup>	0.03	0.02	0.00	0.04
	(0.03)	(0.02)	(0.01)	(0.01)	na	(0.02)
Age, Observed Rounds				X		
Sociodemographic Characteristics				X		
Race Interactions				X		

Notes: N = 6,501. Estimates are unweighted and presented as predicted probabilities estimated with covariates set at observed values. “X” indicates that the covariates listed in the corresponding row header were included in this model specification. Standard errors are reported in parentheses. “na” indicates that standard errors for this outcome were not able to be estimated because of small cell sizes; these estimates should be interpreted with caution. Capital superscript letters denote differences from <sup>W</sup>White, <sup>B</sup>Black, <sup>H</sup>Hispanic, and <sup>O</sup>Other respondents that are statistically distinguishable at the 95-percent confidence level.

Model 1, which, in addition to racial/ethnic group, adjusted only for baseline age and the number of years observed, indicates that transition to an independent residential arrangement identified exclusively via the household roster was the most common type of first departure from the parental home for all racial/ethnic groups (Table 1.2). However, among the remaining young adults whose first home-leaving was linked to a different experience, there was distinct patterning in transition type. Home-leaving for collegiate settings was the next most prevalent pathway for all groups, though most likely for those who were White (20%), then those who identified as being of another racial/ethnic group (18%) or as Black (13%), and least likely among those who identified as being of Hispanic ethnicity (6%). Differences were statistically distinguishable in comparing White and Black, Black and Hispanic, and White and Hispanic respondents.

Model 2 estimated the same associations while adjusting more comprehensively for the multidimensional constellation of social factors that are correlated with variation in family dynamics and institutional contact. Results from this model indicated that even after adjusting further for sex, parental education, disability, and nativity status, substantial racial/ethnic differences persisted. Young adults of Hispanic ethnicity were statistically more likely than those who were non-Hispanic to first leave the parental home for an independent family/household arrangement. In Model 1, White, Black, and Hispanic respondents were all statistically different from one another in their likelihoods of first departing for residential contexts linked to postsecondary education. However, after adjusting for additional characteristics, those who were Hispanic were half as likely as those of other racial/ethnic groups to make first transitions to collegiate settings (7% for Hispanic respondents vs. 18%, 15%, and 15% for White, Black, and Other race/ethnicity respondents, respectively). Young adults identifying as Black remained

significantly more likely to first leave the parental home for incarceration (4%) than those who identified as White (1%) or Hispanic (2%).

Finally, Model 3 explored the possibility that a model specification that attempts to directly recognize the intersection of racialized, gendered, and classed experiences in institutional contact in the transition to adulthood may impact estimates of racial/ethnic variation in home-leaving types (e.g. DiPrete and Buchmann 2006; Pettit and Western 2004). As illustrated in Table 1.2, estimated predicted probabilities of first exits from the parental home remained essentially unchanged with this more complex specification. Although this analysis focuses on racial/ethnic variation as a key component of social stratification in the U.S., the adjusted models also illustrate the salience of institutions in shaping home-leaving pathways with respect to other characteristics that serve as boundaries for the ascription of social status, namely, gender, parental education, nativity, and disability status (Appendix 1A, Table 4). In the interacted model (Model 3), for example, men were nearly four times more likely to first have left the parental home for the military (5% vs. 1%) and 20 times more likely to have left for incarceration than women (4% vs. 0.2%). Those whose daily lives were impacted by a disability were statistically less likely to have first left home for collegiate settings (11% vs. 16%).

## **Discussion**

This analysis of the NLSY97 illustrates the importance of incorporating intermediate and institutional moves away from the family into the study of home-leaving in the transition to adulthood. The life table estimates presented here lead to two key conclusions. First, designations of “adult” vs. “non-adult” experiences and transitions hold implications for even basic descriptive conclusions about the timing and prevalence of key life milestones. The

inclusion of institutional transitions to military service, incarceration, and pursuit of higher education, for example, indicates that young adults are both more likely to leave the parental home by the age of 31, and to do so earlier, than estimated when we privilege family/household transitions in our definition of qualifying departures from the parental home. This holds for young adults of all racial/ethnic backgrounds. Second, this expanded conceptualization of home-leaving leads us to varying conclusions about racial/ethnic patterning in the timing and cumulative risk of this experience. Both the roster-based and institution-inclusive analyses indicate that White young adults are more likely to have left the parental home by age 31 and that there is a cross-over between the cumulative risks of White young adults and those who identify as Black and Hispanic. However, while the roster-based analysis highlights a unique profile for Whites' first home-leaving risk, the institution-inclusive analysis reveals more heterogeneity across racial/ethnic groups.

Multinomial logistic regression models reveal important sociodemographic variation that questions whether the concept of a “timely” first departure from the parental home makes sense when the institutions young adults enter after first home-leaving have such dramatically different implications for their wellbeing. Although nearly all young adults have left the parental home at least once by age 31, these moves away from the family of origin are not necessarily indicative of positive progress into autonomy and adulthood. This is revealed through the expansion of the definition of home-leaving to include institutional contact or experiences that do not conform to a middle-class conceptualization of the transition to adulthood. Specifically, among young adults who leave home by age 31, there are important racial/ethnic differences that are reflective of broader disparities in institutional contact. Black young adults are more likely than those who identify as White or Hispanic to first transition away from the parental home for incarceration

and Hispanic young adults are both significantly less likely to be moving away to live in educational or training-related residential contexts or to transition into incarceration than those of other racial/ethnic groups. The implications of these racial and ethnic differences for immediate and later-life wellbeing are likely substantial. The regression analyses also reveal that characteristics and conditions beyond racial/ethnic group that map onto ascribed characteristics of social status and (dis)advantage—such as gender and disability status—are, too, associated with first home-leaving experiences.

Although these results provide new evidence of the divergent pathways of young adults, this study has limitations. First, despite the fact that the life table approach provides detailed accounting of the timing and prevalence of initial home-leaving experiences, the annual nature of the NLSY means that there are first exits from the parental home that may be missed between rounds of data collection. This is particularly true for pathways to college residence and incarceration, which often occur for intervals of time that are shorter than a year or may be more or less likely to be captured at certain times of the year. Therefore, it is possible that these figures underestimate the likelihood and risk of young adults' first transitions from the parental home for these particular institutional contexts or misidentify higher-order departures (2<sup>nd</sup> or 3<sup>rd</sup> times leaving the parental home after an initial departure and return) as first home-leaving because the data do not capture earlier residential changes. Supplementary analyses relying only on the annual data to assess the impact of irregular survey intervals (annual in 1997-2011 and biennial in 2011-2015) yield substantively similar results, although, as expected, a higher estimated proportion have ever left home in analysis of the longer period (see Appendix Table 1A.2).

Second, the data do not allow me to disaggregate departure types by detailed aspects of residential settings. The diversity of experiences across institutions and facilities make it difficult

to comment on how the relationship between race, ethnicity, home-leaving contexts, and later life outcomes might be moderated by institution-specific characteristics such as the type of university or college, incarceration facility type, and exposure to solitary confinement, among others. (Armstrong and Hamilton 2013; Turney and Conner 2019; Wildeman, Turney, and Yi 2016). Further, because the sample is restricted to those living with at least one parent at baseline, these analyses may be missing some of the most disadvantaged young adults, such as those who are homeless or in foster care as children. Relatedly, prior assessment of bias in the NLSY97 has indicated that sample attrition is non-random with respect to socioeconomic status and college enrollment (Auginbaugh and Gardecki 2007). Although this bias is not likely to be consequential for multivariate analyses (Auginbaugh and Gardecki 2007), this underlines the possibility that some of the most disadvantaged young adults' experiences are underrepresented in these analyses, perhaps underestimating the role of some institutions in shaping the timing and nature of home-leaving.

Finally, because this study's main objective is to offer a conceptual investigation of the implications of institutional transitions in our understanding of first home-leaving experiences, it leaves the exploration of returns to the parental home, mechanisms driving first departures, and repeat departures to future work. The extension of the institutional conceptualization of home-leaving presented here to questions of the permanence (or not) of institutional departures is important for future investigation, particularly within the context of constraints felt by young adults who face expanding student debt burdens and the fallout of the Great Recession (Houle and Warner 2017; Sandberg-Thoma, Snyder, and Jang 2015; Wiemers 2014).

Despite these constraints, these analyses make two broad contributions to the existing literature on home-leaving and the transition to adulthood and variation therein with respect to

race and ethnicity. One contribution of this study is to highlight social, and here, specifically racial/ethnic, differences in home-leaving experiences in the transition to adulthood. As a second contribution, these findings demonstrate the utility of a different measure of home-leaving that integrates considerations of social institutions beyond the home and family of origin. With the expansion of postsecondary education (Baum et al. 2013) and incarceration (Garland 2001; Western 2006) in the United States over the last several decades, documentation of differential exposure to and participation in these drastically different systems as part of the home-leaving process is important to our understanding of divergent trajectories in the life course. Although transitions to dormitories or higher education and incarceration may be intermediate stops in residential trajectories and more temporary in nature, these contexts are nonetheless significant in terms of their implications for racial/ethnic inequality and their relationships with key steps towards family and household formation in early adulthood.

## Appendices

### Appendix 1A. Supplementary Tables

**Appendix Table 1A.1 Comparison of Full NLSY97 and Analytic Samples**

	NLSY97 Sample	Analytic Sample
<b>Social/Demographic Characteristics</b>		
Female	0.49	0.49
Race/Ethnicity		
White, Non-Hispanic	0.67	0.69
Black, Non-Hispanic	0.15	0.14
Hispanic	0.13	0.13
Other, Non-Hispanic	0.05	0.05
Parental Education (Years)	13.63	13.63
	(2.88)	(2.88)
12 Years or Fewer	0.44	0.43
13 Years or More	0.56	0.57
Nativity (born outside of U.S.)	0.04	0.04
Disability Status (has disability)	0.13	0.13
Age at 1 <sup>st</sup> Observation (in 1997)	14.36	14.35
	(1.50)	(1.50)
Rounds Observed	14.03	14.36
	(2.14)	(1.54)
<b>Home-Leaving Characteristics</b>		
Left Parental Home by Age 31		
Roster-Based Measure	0.90	0.89
Institution-Inclusive Measure	0.97	0.97
Age at 1 <sup>st</sup> Home-Leaving <sup>+</sup>		
Roster-Based Measure	21.37	21.64
	(3.28)	(3.02)
Institution-Inclusive Measure	20.48	20.56
	(3.14)	(3.08)
Pathway to 1 <sup>st</sup> Home-Leaving		
Other	0.77	0.76
College	0.17	0.18
Military	0.03	0.04
Jail/Prison	0.02	0.02
Group Home/Homeless	0.00	0.00
Not Yet Departed by 31	0.02	0.02
<i>N</i>	8,984	6,501

Notes: Analysis of 1997-2015 data from the NLSY97. Estimates report weighted means and proportions with standard deviations in parentheses. <sup>+</sup>Indicates measures that apply only to those who have left the parental home by age 31, for which the sample sizes were 7,539 and 5,934 for the full and analytic samples using the roster-based measure, respectively, and 8,243 and 6,393 for the full and analytic samples using the institution-inclusive measures, respectively.

**Appendix Table 1A.2. Cumulative Risks of 1<sup>st</sup> Home-Leaving by Age 31, by Measure and Study Period**

	Main Analyses: 1997-2015 NLSY97 (N = 6,501)				Supplementary Analysis: 1997-2011 NLSY97 (N = 6,790)			
	Roster-Based Measure		Institution-Inclusive Measure		Roster-Based Measure		Institution-Inclusive Measure	
	<i>Cumulative Risk</i>	<i>95% CI (c)</i>	<i>Cumulative Risk</i>	<i>95% CI (c)</i>	<i>Cumulative Risk</i>	<i>95% CI (c)</i>	<i>Cumulative Risk</i>	<i>95% CI (c)</i>
<b>14</b>	0.00	[-0.00, 0.00]	0.00	[-0.00, 0.00]	0.00	[-0.00, 0.00]	0.00	[-0.00, 0.00]
<b>15</b>	0.00	[0.00, 0.01]	0.00	[0.00, 0.01]	0.00	[0.00, 0.01]	0.01	[0.00, 0.01]
<b>16</b>	0.01	[0.01, 0.01]	0.01	[0.01, 0.02]	0.01	[0.01, 0.01]	0.02	[0.01, 0.02]
<b>17</b>	0.02	[0.02, 0.03]	0.03	[0.03, 0.04]	0.02	[0.02, 0.03]	0.04	[0.03, 0.04]
<b>18</b>	0.05	[0.04, 0.05]	0.07	[0.07, 0.08]	0.05	[0.04, 0.05]	0.08	[0.07, 0.08]
<b>19</b>	0.12	[0.11, 0.12]	0.25	[0.24, 0.26]	0.12	[0.11, 0.13]	0.25	[0.24, 0.26]
<b>20</b>	0.23	[0.22, 0.24]	0.41	[0.41, 0.42]	0.23	[0.22, 0.24]	0.41	[0.41, 0.42]
<b>21</b>	0.34	[0.33, 0.35]	0.55	[0.55, 0.56]	0.34	[0.34, 0.35]	0.55	[0.55, 0.56]
<b>22</b>	0.44	[0.43, 0.45]	0.62	[0.62, 0.63]	0.44	[0.44, 0.45]	0.63	[0.62, 0.63]
<b>23</b>	0.54	[0.53, 0.54]	0.69	[0.69, 0.70]	0.54	[0.53, 0.55]	0.69	[0.69, 0.70]
<b>24</b>	0.62	[0.62, 0.63]	0.74	[0.74, 0.75]	0.62	[0.62, 0.63]	0.75	[0.74, 0.75]
<b>25</b>	0.69	[0.68, 0.69]	0.79	[0.78, 0.79]	0.69	[0.68, 0.69]	0.79	[0.79, 0.80]
<b>26</b>	0.74	[0.73, 0.74]	0.83	[0.83, 0.84]	0.74	[0.73, 0.74]	0.84	[0.83, 0.84]
<b>27</b>	0.77	[0.77, 0.78]	0.86	[0.86, 0.86]	0.78	[0.77, 0.78]	0.87	[0.86, 0.87]
<b>28</b>	0.80	[0.80, 0.81]	0.89	[0.89, 0.89]				
<b>29</b>	0.82	[0.82, 0.83]	0.91	[0.91, 0.91]				
<b>30</b>	0.84	[0.84, 0.84]	0.93	[0.93, 0.93]				
<b>31</b>	0.85	[0.85, 0.86]	0.94	[0.94, 0.94]				

Notes: Analysis of 1997-2015 data from the NLSY97. Estimates are weighted.

**Appendix Table 1A.3. Cumulative Risk of 1<sup>st</sup> Departure from the Parental Home by Age 31, by Measure and Race/Ethnicity**

	White, Non-Hispanic (N = 3,210)		Black, Non-Hispanic (N = 1,713)		Hispanic (N = 1,365)	
	<i>Roster-Based</i>	<i>Institution-Inclusive</i>	<i>Roster-Based</i>	<i>Institution-Inclusive</i>	<i>Roster-Based</i>	<i>Institution-Inclusive</i>
	0.00	0.00	0.00	0.00	0.00	0.00
<b>14</b>	[-0.00, 0.00]	[-0.00, 0.00]	[-0.00, 0.00]	[-0.00, 0.00]	[-0.00, 0.00]	[-0.01, 0.01]
	0.00	0.00	0.01	0.01	0.00	0.01
<b>15</b>	[-0.00, 0.00]	[-0.00, 0.00]	[0.00, 0.012]	[0.00, 0.01]	[-0.00, 0.01]	[0.00, 0.01]
	0.00	0.01	0.02	0.02	0.01	0.02
<b>16</b>	[0.00, 0.01]	[0.00, 0.01]	[0.01, 0.02]	[0.02, 0.03]	[0.01, 0.02]	[0.01, 0.03]
	0.01	0.02	0.04	0.05	0.03	0.04
<b>17</b>	[0.01, 0.02]	[0.01, 0.03]	[0.03, 0.04]	[0.04, 0.06]	[0.02, 0.04]	[0.03, 0.06]
	0.04	0.06	0.07	0.10	0.06	0.09
<b>18</b>	[0.03, 0.04]	[0.04, 0.07]	[0.06, 0.08]	[0.09, 0.12]	[0.05, 0.07]	[0.07, 0.11]
	0.11	0.24	0.13	0.27	0.12	0.24
<b>19</b>	[0.10, 0.12]	[0.23, 0.26]	[0.12, 0.15]	[0.26, 0.29]	[0.11, 0.14]	[0.22, 0.26]
	0.23	0.43	0.24	0.43	0.21	0.37
<b>20</b>	[0.22, 0.25]	[0.42, 0.44]	[0.22, 0.25]	[0.41, 0.44]	[0.19, 0.23]	[0.35, 0.39]
	0.36	0.57	0.33	0.56	0.31	0.50
<b>21</b>	[0.35, 0.37]	[0.57, 0.58]	[0.31, 0.34]	[0.55, 0.57]	[0.29, 0.32]	[0.48, 0.510]
	0.47	0.65	0.42	0.63	0.41	0.56
<b>22</b>	[0.46, 0.48]	[0.64, 0.65]	[0.41, 0.43]	[0.62, 0.64]	[0.40, 0.43]	[0.56, 0.59]
	0.58	0.72	0.50	0.69	0.50	0.64
<b>23</b>	[0.57, 0.59]	[0.71, 0.73]	[0.49, 0.51]	[0.68, 0.70]	[0.49, 0.51]	[0.69, 0.71]
	0.67	0.77	0.58	0.74	0.58	0.70
<b>24</b>	[0.66, 0.67]	[0.77, 0.78]	[0.57, 0.59]	[0.73, 0.75]	[0.57, 0.59]	[0.69, 0.71]
	0.74	0.82	0.65	0.78	0.64	0.74
<b>25</b>	[0.73, 0.74]	[0.81, 0.83]	[0.64, 0.65]	[0.77, 0.79]	[0.63, 0.65]	[0.73, 0.75]
	0.79	0.86	0.69	0.82	0.69	0.80
<b>26</b>	[0.78, 0.79]	[0.86, 0.86]	[0.68, 0.70]	[0.81, 0.83]	[0.68, 0.70]	[0.79, 0.81]
	0.82	0.88	0.73	0.85	0.73	0.83
<b>27</b>	[0.82, 0.83]	[0.88, 0.89]	[0.72, 0.74]	[0.84, 0.86]	[0.72, 0.74]	[0.82, 0.84]
	0.85	0.91	0.76	0.88	0.77	0.87
<b>28</b>	[0.84, 0.85]	[0.90, 0.91]	[0.75, 0.77]	[0.87, 0.86]	[0.76, 0.78]	[0.86, 0.88]
	0.87	0.92	0.78	0.90	0.79	0.90
<b>29</b>	[0.86, 0.87]	[0.92, 0.93]	[0.78, 0.79]	[0.90, 0.91]	[0.78, 0.80]	[0.89, 0.90]
	0.88	0.94	0.81	0.93	0.81	0.92
<b>30</b>	[0.88, 0.88]	[0.93, 0.94]	[0.80, 0.81]	[0.92, 0.93]	[0.80, 0.81]	[0.91, 0.92]
	0.89	0.95	0.82	0.94	0.82	0.94
<b>31</b>	[0.89, 0.90]	[0.95, 0.96]	[0.81, 0.83]	[0.93, 0.94]	[0.81, 0.83]	[0.93, 0.94]

Notes: Analysis of 1997-2015 data from the NLSY97. Estimates are unweighted. 95% confidence intervals reported in brackets

**Appendix Table 1A.4. Multinomial Regression Models of First Transition from the Parental Home, 1997-2015 NLSY97**

	<b>Independent</b>	<b>College</b>	<b>Military</b>	<b>Jail/Prison</b>	<b>Group Home/ Homeless</b>	<b>Not Yet Departed by Age 31</b>
<b>Race/Ethnicity</b>						
White	0.76 <sup>H</sup> (0.01)	0.18 <sup>B,H</sup> (0.01)	0.04 (0.00)	0.01 <sup>B</sup> (0.00)	0.00 na	0.01 <sup>B</sup> (0.00)
Black	0.76 <sup>H</sup> (0.01)	0.14 <sup>W,H</sup> (0.01)	0.03 (0.00)	0.04 <sup>W,H</sup> (0.01)	0.00 na	0.02 <sup>W</sup> (0.00)
Hispanic	0.86 <sup>W,B,O</sup> (0.01)	0.07 <sup>W,B,O</sup> (0.01)	0.03 (0.01)	0.02 <sup>B</sup> (0.00)	0.00 na	0.02 (0.01)
Other	0.77 <sup>H</sup> (0.03)	0.15 <sup>H</sup> (0.02)	0.03 (0.01)	0.02 (0.01)	0.00 na	0.04 (0.02)
<b>Sex</b>						
Male	0.74 (0.01)	0.14 (0.01)	0.05 (0.00)	0.04 (0.00)	0.00 (0.00)	0.02 (0.00)
Female	0.81* (0.01)	0.16 (0.01)	0.01* (0.00)	0.00* (0.00)	0.00 (0.00)	0.01 (0.00)
<b>Parental Education</b>						
< High School	0.85 (0.01)	0.07 (0.01)	0.03 (0.00)	0.03 (0.00)	0.03 (0.00)	0.00 (0.00)
High School +	0.71* (0.01)	0.22* (0.01)	0.03 (0.00)	0.01* (0.00)	0.01* (0.00)	0.00 (0.00)
<b>U.S. Citizenship</b>						
Non-Citizen	0.81 (0.03)	0.03 (0.01)	0.03 (0.01)	0.01 (0.01)	0.00 (0.00)	0.02 (0.01)
Citizen	0.77 (0.01)	0.03 (0.00)	0.03 (0.00)	0.02* (0.00)	0.00* (0.00)	0.02 (0.00)
<b>Disability Status</b>						
No Disability	0.77 (0.01)	0.16 (0.01)	0.04 (0.00)	0.02 (0.00)	0.00 (0.00)	0.02 (0.00)
Disability	0.83* (0.01)	0.11* (0.01)	0.02* (0.00)	0.03 (0.01)	0.00 (0.00)	0.02 (0.01)
Age, Rounds				X		
Sociodemographics				X		
Race Interactions				X		

Note: N=6,501. Estimates are unweighted and from the fully-adjusted model (Model 3). Results are reported as predicted probabilities, with covariates set at observed values. Standard errors are reported in parentheses. Superscripts and asterisks denote statistically distinguishable differences from comparison group or <sup>W</sup>White, <sup>B</sup>Black, and <sup>H</sup>Hispanic respondents at the 95-percent confidence level. “na” indicates that standard errors for this outcome were not able to be estimated because of small cell sizes, which yielded a highly singular variance matrix.

## ***Appendix 1B. Methodological Appendix***

This analysis employs *roster-based* and *institution-inclusive* measures of the timing and type of home-leaving pathways experienced in the transition to adulthood, constructed using the 1997 National Longitudinal Survey of Youth. This is a technical description of the procedure used to construct these measures, with relevant NLSY97 variable names noted in parentheses. STATA code is available upon request from the author.

First, the respondent's parental co-residential status is identified using constructed, household roster, and institutional contact measures at each survey round:

1. For both the roster-based and institution-inclusive measures, if the constructed baseline measure of parental co-residence (YPRG\_1300\_1997) indicates that the respondent (R) is not living with their parents, they are identified as having left the household by the start of the survey.
2. For subsequent survey years (1997-2011, 2013, 2015), the household roster (HH2\_REL) is used to determine parental co-residential status. The roster enumerates R's relationship to every household member. If at least one member is R's biological, adoptive, or step-parent, R is identified as still living with a parent.
3. If R was not interviewed in a given round and the reason given for non-interview status (RNI) shows that R has died, they are coded as having left the parental home. This concludes the construction of the *roster-based* measures.
4. Steps 4 and 5 are only applied to the *institution-inclusive* measure. If R lives with their parent(s) but is themselves identified as the householder, owner, or renter, they are coded as having left the parental home (YAST\_3310, YAST25\_3310, YAST\_3320, YAST\_CH1, YHOU\_CH1)
5. If additional measures indicate that R was actually elsewhere at the time of survey, they are coded as having left the home via one of a mutually exclusive set of pathways: college (YHHI\_4400), military service (RNI, YEMP\_MILFLAG), incarceration (RNI, YHHI\_4400), group home/homelessness (YHI\_4400), and not yet departed by the end of the study period. Remaining departures identified via the homeownership/renter and household roster recodes are described as "independent" pathways.

Next, this round-specific information about parental co-residential and living arrangements is summarized to determine timing and pathway of home-leaving.

6. R's age at first home-leaving is captured by noting R's age in the first survey round in which they are identified as not living with at least one parent. For the *roster-based* measure, this means that this is the first round in which they do not list a parent in the household roster (Step 2) or were not interviewed because they had died (Step 3). For the *institution-inclusive* measure, this means that this is the first round in which R does not list a parent in the household roster, was not interviewed because they had died, was living with a parent but as the householder (renter/owner) (Step 4), and/or because they were identified as living in a different residential situation (Step 5).
7. In the *institution-inclusive* measure, R's pathway of first home-leaving is captured by noting R's residential situation and/or institutional context in the first survey round in which they are not living with a parent as determined in Steps 4 and 5.

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## CHAPTER II: INSTABILITY IN CARE AND LIVING ARRANGEMENTS: PUTTING FOSTER YOUTH IN CONTEXT

### **Abstract**

Family instability is a key dimension of social inequality in the United States. If children are deemed at risk for child maltreatment, foster care placement is one potential reason for change in children's care and living arrangements. In this study, I analyze a nationally representative sample of child welfare-involved youth to examine whether associations between foster care and instability in care and living arrangements is driven by children's placement in foster care or by other acute hardships experienced by foster youth. I find that compared to children who do not enter foster care, those who do are more likely to experience change in their primary caregiver, availability of a secondary caregiver, and their living arrangement. When treating an initial transition into foster care as necessary, excluding it from measures of change, foster care appears to stabilize the structures of children's care arrangements, while the relationships that comprise those arrangements remain substantially more volatile. This is consistent for White, Black, and Hispanic children. In addition to answering an important question about the experiences of child welfare-involved youth, this study contributes to the more central situation of "institutionalized" family life—including that shaped by the child welfare system—in broader scholarship on family diversity, instability, and inequality.

## **Introduction**

Family instability is a key dimension of social inequality in the United States (U.S.; Cherlin and Seltzer 2014; Raley and Sweeney 2020; Raley and Wildsmith 2004; Roberts 2003; Wildeman 2009). Children's exposure to family change has been linked to higher levels of socioemotional and behavioral problems and delinquency (Cavanagh and Huston 2005; Crosnoe et al. 2014; Fomby and Cherlin 2007), poor mental health (Brown 2006), lower levels of school engagement and educational attainment (Aquilino 1996; Brown 2006), and instability in their own families as adults (McLanahan and Bumpass 1988; McLanahan and Percheski 2008). Further, these experiences are unequally distributed across the population, with Black, Native American, and socioeconomically marginalized children at higher risk of experiencing family instability (Berger et al. 2016; McLanahan and Percheski 2008; Perkins 2017; Turney 2014).

In the U.S., the subgroup of youth who enter the foster care system has received a particular type of attention in social scientists' and policymakers' investigations of family and household stability, specifically with regard to the instability that these children may experience in their foster placements (Fawley-King et al. 2017; Havlicek 2010). In the most high-risk cases of maltreatment, defined as child abuse and/or neglect, children may be removed from the home and placed in foster care. In these situations, we might expect that family disruption would have minimally negative or even positive impacts on child wellbeing. Indeed, the foster care system's primary goals include keeping children perceived to be in danger safe and facilitating their prompt transitions into nurturing and permanent family settings (U.S. Children's Bureau 2013). However, many children in the foster care system experience multiple changes in their care arrangements and fare quite poorly across all domains of wellbeing (e.g. Church, Gross, and Baldwin 2005; Courtney et al. 2005; Harris and Courtney 2003; Roberts 2003).

But is it foster care that explains these higher levels of instability in children's care and living arrangements? If so, is the heightened social instability of foster youth beyond that which is necessary to ensure their immediate safety? These questions are empirically difficult to answer, in large part due to the many differences between children who experience foster care placement and those who do not. Foster-involved children face disproportionately high levels of hardship and marginalization, making it difficult to determine whether instability in their care and living arrangements is due to foster care entry or other factors and experiences predictive of family and household instability (Berger et al. 2009; Turney and Wildeman 2017).

This study uses the National Survey of Child and Adolescent Wellbeing (NSCAW), the first and only nationally representative longitudinal survey of child welfare-involved youth, (Research Triangle Institute 2008; RTI International 2014) to examine whether foster care is likely to be the cause of foster youths' higher likelihoods and rates of instability in their care and living arrangements. I use inverse probability weighted regression models to compare the experiences of children ever in foster care to those of a strict comparison group: children who are also subjects of child maltreatment investigations and differ only in that they have never been placed in foster care. I first answer this question using measures that include any change in children's caregiving and living arrangements as instability, examining changes in (1) their primary caregiver, (2) whether the child has a regular secondary caregiver, and (3) their living arrangement. I then estimate the same models using measures that treat foster youth's initial entries into foster care and subsequent transitions to permanent caregivers as necessary for their protection and wellbeing, excluding changes coinciding with those transitions from measures of instability in their care and living arrangements. By using these two sets of measures, these analyses remain agnostic in the important and ongoing debate over the frequency with which

foster care placement is invoked as a response to maltreatment (e.g. Bartholet 1999, 2000, 2015; Guggenheim 2000; Roberts 2003; Wald, Carlsmith, and Leiderman 1988; Waldfogel 1998).

In treating any changes in children’s care and living arrangements as “excess” instability, I find that compared to children who do not enter foster care, those who do are more likely to experience a change in their primary caregiver (46 percentage points), availability of a regular secondary caregiver (8 percentage points), and their living arrangement (20 percentage points). Children who enter foster care are also likely to experience more changes. When I treat initial transitions into foster care and subsequent transitions to permanent arrangements as necessary, excluding them from measures of change, foster care appears to stabilize or have no effect on the structures of children’s care settings in the short- to medium-term. Foster youth are 7 percentage points less likely to experience a change in secondary caregiver availability and statistically indistinguishable from non-foster youth in their likelihood of experiencing a change in their living arrangement. However, the primary caregiver relationships of children ever in foster remain substantially less stable: these children continue to have a 30 percentage-point higher probability of change and more changes in their primary caregivers. In addition to answering an important question about the experiences of child welfare-involved youth, this study contributes to the more central situation of “institutionalized” family life—including that shaped by the child welfare system—in broader scholarship on family diversity, instability, and inequality.

## **Background**

### ***Family Instability and Child Wellbeing***

Children who experience higher levels of familial and household “churn” have poorer outcomes over the life course than those who experience comparatively greater stability in

family life (Cherlin 2009; Fomby and Cherlin 2007; Kim 2011; McLanahan and Percheski 2008; Raley and Sweeney 2020; Rackin and Gibson-Davis 2018). Strong associations of family instability with social, economic, and health outcomes, coupled with its unequal distribution across the population have made family instability a major component of U.S. social inequality (Brown 2006; Cherlin and Seltzer 2014; McLanahan and Percheski 2008). Low-income and poor families—and children in those families—are more likely than those who are comparatively materially advantaged to experience changes in their family and household structure and composition (Kim 2012; Lundberg, Pollak, and Stearns 2016; Schwartz and Han 2014). In addition to socioeconomic disparities in family instability exposure, there is marked racial/ethnic patterning in family instability in the U.S. Black and Native American children and parents, as well as those of Hispanic ethnicity and were born in the U.S. are also more likely to experience change and instability in family life than parents and children of other racial/ethnic and nativity groups in the U.S. (e.g. Brown, Stykes, and Manning 2016; Sweeney and Phillips 2004).

Further, prior scholarship has found that changes in the structure and composition of children's family and household contexts are negatively associated with an array of indicators of child wellbeing. Change and instability in children's living arrangements are associated with lower levels of children's cognition, educational attainment, and school engagement (e.g. Cavanagh and Huston 2006; Fomby and Cherlin 2007; Kim 2011; Mollborn 2016; Mollborn, Fomby, and Dennis 2012; Perkins 2019); poorer health (Mollborn 2016); and higher likelihoods of parent- and teacher-reported behavioral challenges (Fomby and Mollborn 2017; Fomby and Osborne 2017; Mitchell et al. 2015; Cooper et al. 2011). Changes in children's living arrangements also have negative impacts on parents' wellbeing (Kamp Dush 2013; Osborne, Berger, and Magnuson 2012; Williams and Dunne-Bryant 2006), which, in turn, are likely

negatively linked to declines in material and social resources (Osborne, Berger, and Magnuson 2012; Tach and Eads 2015) and negative effects on child wellbeing (Cavanagh and Fomby 2019; Hardie and Turney 2017).

Analyses that explore heterogeneity in these associations and effects, however, tell a more nuanced story. Some evidence suggests that negative associations between family and household change and children's wellbeing are moderated by the quality of family life prior to the family transitions in question. Take, for example, the case of parental separation or divorce. An extensive body of work concerned with the average effects of parental separation on children previously centered on evidence that this disruption was detrimental for children's risk of delinquency (Matsueda and Heimer 1987), and their likelihood of dropping out of school before completing a high school education (McLanahan and Sandefur 1994), among other negative outcomes (McLanahan and Percheski 2008; Raley and Sweeney 2020). However, analyses that further detail the circumstances proximal to the separation have found that the wellbeing of children following a divorce is contingent upon the quality of parent-child relationships before and after separation (Musick and Meier 2010; Yu et al. 2010) and the intensity of prior parental conflict (Amato, Loomis, and Booth 1995; Booth and Amato 1994, 2001).

There are codified and formal processes for facilitating such family transitions. Some of these are voluntary, such as divorce, remarriage, adoption, and emancipation. Others are involuntary and/or "collateral" family changes and disruptions that are caused by interventions for other policy objectives, such as detention, incarceration, and deportation. However, such "institutionalized" family changes have generally received markedly less attention in research taking a bird's-eye view of family diversity and complexity in the U.S. (Berger and Cancian 2020, p.483; Brandon 2001; Perry 2006; Reich 2008; Van Hook and Glick 2020). Analysis of

family change initiated by policy or external actors has largely been examined in separate bodies of work focused on shedding light on those policies and affected communities (e.g. Amuedo-Dorantes and Arenas-Arroyo 2018; Dreby 2012; Hall, Musick, and Yi 2019; Turney 2015; Wakefield and Wildeman 2013; see Perry 2016 for a key exception).

### ***Foster Care as Family Change and Instability***

One example of these types of “institutionalized” family change is the decision to place a child in foster care. Foster care placement, or the removal of a child from their home, is one potential response to allegations of child maltreatment. Child maltreatment—generally defined as abuse and/or neglect—is associated with an extensive array of poor outcomes, including homelessness and criminal justice involvement (Doyle and Aizer 2018), poor socioemotional and cognitive developmental outcomes (Font and Berger 2015), and poor mental and physical health both in childhood and into adulthood (Jonson-Reid, Kohl, and Drake 2012). Therefore, in the most extreme cases of substantiated maltreatment, in which a child is deemed to be at high or imminent risk of further maltreatment if left in their current care setting, decisions to place that child in foster care may, in fact, be protective and even actively beneficial.

In fact, the success of the foster care system is defined, in part, by its success in supporting children’s “permanency,” or their (re)integration into nurturing and stable family settings (U.S. Children’s Bureau 2018). Permanency outcomes can range from the child’s placement back with their family of origin, sometimes accompanied by in-home services to support and ensure the child and their family’s wellbeing, to termination of an original caregiver’s parental rights and the child’s placement with an adoptive family. Theoretically, this means that a child’s placement in foster care could *decrease* instability or, ideally at the very

least, have no effect on children's exposure to family instability beyond an initial set of disruptions with the purpose of keeping them safe.

However, the likelihood of this outcome is strongly tied to whether the child ultimately transitions to a permanent care setting after entering foster care (Courtney et al. 2011; Harris and Courtney 2003; Koh et al. 2014). Not all children in the foster care system transition to permanency, and their likelihoods of doing so and the rapidity with which they do are both linked to their wellbeing. Those who churn through multiple foster arrangements fare significantly worse than those who land in stable placements early in their involvement with the foster care system (e.g. Oosterman et al. 2007; Rubin et al. 2007; Ryan and Testa 2005). Sociodemographic differences in likely foster care trajectories and experiences echo well-documented patterns of family instability in the broader general child population. Compared to White children who enter foster care, Black, Hispanic, and Native American children in foster care are likely to be in the system for longer, less likely to transition into permanent family settings, and more likely to experience multiple placements (Burns et al. 2004; Garcia and Courtney 2011; Harris and Courtney 2003; Osgood, Foster, and Courtney 2010).

However, the degree to which foster care itself causes these elevated risks of change and instability remains an empirical challenge for two reasons. First, the social, demographic, and economic marginalization of children placed in foster care makes it difficult to disentangle foster care effects from effects of other dimensions of disadvantage on foster youths' exposure to care and living arrangement instability (Courtney 2000; DeGue and Widom 2009; Goemans et al. 2016; Turney and Wildeman 2015; Wildeman and Waldfogel 2014). In addition to the fact that many of these children are potentially suffering from the traumas of child maltreatment and severed social ties, many also demonstrate elevated levels of developmental and behavioral

problems and are disproportionately likely to come from homes and communities that are impacted by economic hardship, adverse health conditions, and neighborhood and/or domestic violence (e.g. Dong et al. 2004; Leloux-Opmeer et al. 2016). All of these acute stressors are also correlates of family instability (Fomby and Cherlin 2007; McLanahan and Percheski 2008). To disentangle these processes, analyses of the effects of foster care must compare children ever in care to similar children with no history of foster care, something that few studies have accomplished to date (Doyle and Aizer 2018, Yi and Wildeman 2018; for key exceptions, see Berger et al. 2009; Berzin 2008; Doyle 2007, 2008, 2013; Johnson-Reid and Barth 2000).

Second, interpretation of whether change in care and living arrangements among child welfare-involved youth is beneficial or detrimental for children's wellbeing is contingent upon one's perspective on current decisions to place children in foster care at all. Some argue that child welfare systems are overly punitive and unequally so across social groups, dissolving and traumatizing families in ways that reverberate through social structure (e.g. Roberts 2003), and that child welfare responses to maltreatment should seek to reduce removal from the home through alternative approaches (e.g. Waldfogel 1998). Others argue that we are not removing enough children from potentially dangerous living situations and that foster care placement, termination of parental rights, and adoption should follow more promptly and frequently as system responses to confirmed maltreatment (e.g. Bartholet 1999, 2000).

### ***Current Study***

In this study, I focus on two core questions about the potential role of foster care entry in shaping the family and household experiences of children in the United States. First, are children placed in out-of-home or foster care more likely to experience instability in their care and living arrangements than those who are not? Second, if there are indeed differences, does foster care

introduce “excess” instability into children’s family life beyond that which is necessary to protect them from what is suspected to elevated risks of harm? To answer these questions, I analyze data from the National Survey of Child and Adolescent Wellbeing (NSCAW) to estimate associations between foster care entry and children’s likelihoods and rates of instability in their care and living arrangements. I examine three dimensions of children’s living situations: their primary caregiver, the availability of a regular secondary caregiver, and their living arrangement or household type. These descriptors of children’s care and living arrangements are measured in two ways: the first set of measures includes any and all changes observed over the study period. The second set of measures include only changes other than those that could be interpreted as changes aligning with objectives of achieving children’s safety and permanency. More specifically, this second set of measures excludes a child’s initial placement in foster care and, if they enter foster care, a subsequent transition to a permanent caregiver.

Absent a randomized experiment, the ideal study of the effect of foster care on instability in children’s care and living arrangements requires data that meet four conditions: first, the data must include children who enter foster care and children who do not, for comparison. Second, as the question of instability is one of change, the data must observe these children over time. Third, the data must provide information about children’s pre-placement characteristics and factors predictive of foster care placement to account for the selectivity of children for whom removal from the home is deemed necessary. Finally, the data must provide information that allows for descriptions of care and living arrangements that apply to children in both foster and non-foster settings. The NSCAW data, a nationally representative longitudinal survey of children who were subjects of child maltreatment investigations, meet all of these criteria.

My analysis expands our understanding of family inequality and the consequences of child welfare system involvement in three key ways. First, I am able to directly compare the experiences of foster and non-foster youth, which is difficult due to the exclusion of one of these groups in most data sources available to study child welfare-involved youth or general patterns of family instability (Courtney 2000). Second, the rich detail of the NSCAW allows me to account for the selectivity of foster youth with respect to a comprehensive array of observed sociodemographic, economic, and system involvement-related characteristics. Finally, by bringing core family demographic questions of family change and instability to the realm of child welfare, I aim to more centrally situate the experience of a non-negligible and vulnerable but relatively understudied population in research on family diversity and inequality.

I anticipate that those who are ever placed in foster care will be more likely to experience changes in their care and living arrangements than children who are not, but that these differentials will be less pronounced among children of color, who are more likely to experience family and household complexity and instability. My expectations for comparisons of children who are similarly likely to enter foster care (as opposed to unadjusted estimates) are less clear. In the case that observed differences in foster and non-foster youths' likelihoods of experiencing changes in care and living arrangements truly be consequences of foster care placement, I would expect these differences to persist even after accounting for children's differential risks of foster care placement and transitions intended for child protection and permanency. However, if these differences are instead functions of systematic differences that are independently predictive of care and living arrangement change, such as poverty or parents' marital status, these differences should diminish with the strategies I use to account for children's selection into foster care.

## **Data, Measures, and Methods**

### ***The National Survey of Child and Adolescent Wellbeing (NSCAW)***

The National Survey of Child and Adolescent Wellbeing (NSCAW), the first and only nationally representative survey of child welfare-involved children, is a longitudinal survey of children who were subjects of maltreatment investigations but were not necessarily confirmed as victims and/or placed in foster care. This sampling design allows for a comparison of children who are viably and more similarly at risk of foster care placement than a comparison between children who enter the foster care system and those in the “general” child population who may or may not have had any contact with child protective services. The NSCAW data also contain detailed information about the characteristics and experiences of child welfare-involved youth and their caregivers and living arrangements over time. These data come from multiple reporters through child, caregiver, teacher, and caseworker survey instruments, with the support of computer-assisted technology for questions about sensitive topics (Dowd et al. 2002).

For the core analyses, I use the NSCAW I, the first of the two NSCAW panels.<sup>1</sup> The NSCAW I sample consists of 5,501 children whose child welfare investigations closed between October 1999 and December 2000, when the children were zero to 14 years old. Follow-up waves 2-5 were collected 12, 18, 36, and 59-97 months after the close of the initial investigation, respectively, and continued to survey sample members and caregivers even after the focal child’s exit from the child welfare system (meaning no further services, investigations, or foster care placement) (Dowd et al. 2002). To ensure that any differences in likelihoods of experiencing

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<sup>1</sup> I use the NSCAW I because of its longer survey period (5 vs. 3 waves), which allows for an analysis of rates/frequency of social contextual instability rather than a single change measure in addition to any change. Analysis of the NSCAW II yields substantively similar results, as discussed later.

caregiver and household instability between non-foster and foster youth can be plausibly attributed to foster care placement, I restrict the sample in two key ways. First, I restrict the sample to children whose first foster care placement is observed after the baseline interview (Wave 1). To do so, I exclude children who (a) were ever in foster care prior to Wave 1 (N=239), ascertained through retrospective survey items about foster care history and (b) children who were not observed in-home with a permanent caregiver (i.e. in foster care) at baseline (Wave 1; N = 1,467, of whom 1,091 also had pre-Wave 1 foster care histories). This ensures temporally that any estimated association between out-of-home care entry and social instability is identified subsequent to a child's first-ever entry into foster care and not a pre-existing trajectory of living arrangement change including prior foster care system involvement.

I restrict the sample further to exclude those who do not provide any additional caregiver/household information in or are entirely missing from subsequent waves, making it impossible to track changes in their living arrangements after Wave 1 (N = 104), and children who are missing information on their race/ethnicity (N = 14). Combined, these restrictions leave an analytic sample of 1,644 children: 716 non-Hispanic White children, 515 non-Hispanic Black children, 303 Hispanic children, and 110 children who are non-Hispanic and Native American or of another racial group. I preserve cases with missing information for measures other than the child's race/ethnicity, foster care history, and outcome measures of change by using multiply-imputed data (n=5) estimated through chained imputation using the `mi` procedures in Stata 14.2 (Graham 2009; Rubin 1987; StataCorp 2015).

A detailed statistical comparison of the analytic sample and the full nationally representative NSCAW I sample is presented in Appendix Table 2.1. On average, the children included in my analysis are likely to have been observed for longer (4.6 vs. 4.3 waves). They are

also likely to have had baseline caregivers who were older (29% vs. 48% with baseline caregivers aged 35 years or older), a biological parent (92% vs. 58%), unemployed (14% vs. 9%) and who were ever arrested (33% vs. 25%). Children in the analytic sample also had baseline caregivers with different child welfare system involvement. Baseline caregivers of children in the analytic sample were less likely to have had a prior substantiated child protective services report (30% vs. 34%) and less likely to have previously received child welfare services (36% vs. 40%), but more likely to have ever had other children placed in foster care (16 vs. 9%). Children in the analytic sample were statistically indistinguishable from those omitted from the analysis on key demographic and health characteristics.

As illustrated by the unweighted statistical description presented in Table 2.1, children in the analytic sample who are ever placed in foster care over the study period differ from children in the comparison group (never in foster care) in an array of substantively important ways. Children ever placed in foster care were more likely to be Black (36% vs 29%) and more likely to have a chronic health condition (34% vs. 28%) or behavioral problem (30% vs. 25%). The baseline caregivers of children ever in foster care were older (36% vs. 26% aged 35 years and older) and to be impacted by adverse conditions themselves, including cognitive disabilities (15% vs. 8%), chronic health conditions (23% vs. 18%), poor mental health (31% vs. 20%), financial hardship (45% vs. 33%), and low levels of social support (50% vs. 41%).

For children in both groups, many of their caregivers also had histories of institutional contact. Approximately one-third had ever been arrested (36% of those ever in foster care and 32% of those never in foster care). Substantial shares had prior child protective services (CPS) involvement, as a caregiver involved with a prior CPS investigation (48% overall; 57% of ever in foster care, 44% of never in foster care) or a prior CPS maltreatment substantiation (30% overall;

39% of ever in foster care, 26% of never in foster care), or as a recipient of other child welfare services (36% overall; 46% of ever in foster care, 32% of never in foster care). One-third of baseline caregivers were confirmed victims of childhood maltreatment themselves (33% overall, 41% of ever in foster care, 30% of never in foster care).

**Table 2.1. Unweighted Sample Descriptives, NSCAW I**

	Overall	Observed in Foster Care	
		Never	Ever
<b>Child and Caregiver Demographics</b>			
<b>Child Demographics</b>			
Age (Years)	5.55 (4.78)	5.69 (4.77)	5.26 (4.81)
Female	0.49	0.50	0.46
<b>Race/Ethnicity</b>			
White	0.44	0.45	0.41
Black	0.31	0.29*	0.36
Hispanic	0.18	0.19	0.18
Other	0.07	0.07	0.06
<b>Caregiver Demographics</b>			
<b>Age (%)</b>			
< 25 Years	0.31	0.30	0.32
25-35 Years	0.40	0.44***	0.32
35+ Years	0.29	0.26***	0.36
Female	0.93	0.92	0.94
<b>Race/Ethnicity (% Non-White)</b>	0.51	0.51	0.50
<b>Socioeconomic Context</b>			
<b>Caregiver Education (% Some Postsecondary)</b>	0.21	0.22	0.20
<b>Unemployment Status</b>	0.14	0.14	0.15
<b>Ever Arrested</b>	0.27	0.25***	0.33
<b>Family: Financial Hardship</b>	0.37	0.33***	0.45
<b>Family: Low Social Support</b>	0.44	0.41**	0.50
<b>Child Welfare Services Contact</b>			
<b>Alleged Maltreatment Type(s) (%)</b>			
Physical Abuse	0.31	0.30	0.32
Sexual Abuse	0.14	0.16***	0.09
Neglect	0.62	0.61	0.64
<b>Number of Types of Maltreatment</b>	0.33 (0.47)	0.33 (0.47)	0.32 (0.47)
<b>Alleged Perpetrator(s) (%)</b>			
Parent	0.70	0.70	0.70
Other Family	0.08	0.09	0.08
<b>Substantiated/Indicated Maltreatment</b>			
<b>Criminal Investigation</b>	0.27	0.29**	0.22
<b>Prior CPS Investigation</b>	0.48	0.44***	0.57
<b>Prior CPS Report Substantiation</b>	0.30	0.26***	0.39
<b>Prior Child Welfare Services Receipt</b>	0.36	0.32***	0.46
<b>Other Child Placed in Foster Care</b>	0.16	0.15**	0.20
<b>Own Maltreatment in Childhood</b>	0.33	0.30***	0.41

*(Continued on next page)*

**Table 2.1, continued**

<b>Child and Caregiver Health</b>			
<b>Child Health</b>			
Cognitive Disability	0.10	0.09	0.11
General Health	4.04 (1.03)	4.09** (1.00)	3.91 (1.08)
Chronic Health Condition	0.30	0.28**	0.34
Behavioral Problem	0.27	0.25*	0.30
<b>Caregiver Health</b>			
Cognitive Disability	0.10	0.08***	0.14
Physical Disability	0.02	0.02	0.02
Chronic Health Condition	0.19	0.18*	0.23
Mental Health	0.23	0.20***	0.31
Emotional Difficulty	0.34	0.32*	0.37
Alcohol Dependency	0.03	0.02	0.04
Drug Dependency	0.05	0.04*	0.07
Domestic Violence Victimization	0.37	0.37	0.37
Family Stress	0.68	0.65**	0.73
<b>Family/Household and Sociospatial Context</b>			
Poor Parenting (Caseworker Report)	0.50	0.45***	0.61
<b>Caregiver Time with Child</b>			
<6 Months	0.26	0.32***	0.12
6-24 Months	0.28	0.28	0.30
24-120 Months	0.30	0.27***	0.38
120+ Months	0.16	0.14***	0.20
Caregiver Ever Arrested	0.33	0.32	0.36
<b>Stratum</b>			
California	0.11	0.12	0.11
Florida	0.06	0.06	0.05
Illinois	0.07	0.07	0.06
Michigan	0.06	0.06	0.05
New York	0.07	0.08	0.07
Ohio	0.06	0.05*	0.09
Pennsylvania	0.06	0.06	0.06
Texas	0.11	0.10	0.12
Remainder	0.40	0.40	0.39
Number of Waves Observed	4.63 (0.67)	4.62 (0.68)	4.66 (0.66)

*(Continued on next page)*

**Table 2.1, continued**

<b>Baseline Care and Living Arrangement</b>			
<b>Primary Caregiver (PCG)</b>			
Type: Biological Parent	0.92	0.98***	0.78
Cohabiting w/Partner	0.17	0.18	0.17
Married	0.27	0.28	0.26
<b>Secondary Caregiver Present</b>	0.56	0.57	0.54
<b>Living Arrangement</b>			
Single Adult	0.61	0.63*	0.57
Extended Family Members	0.27	0.22***	0.37
Non-Family Members	0.13	0.13	0.13
<i>N</i>	1,341	917	424

Notes: Asterisks denote statistically significant differences between non-foster and foster youth at the following significance levels: \*\*\* $p < 0.001$  \*\* $p < 0.01$  \* $p < 0.05$ . Estimates are unweighted and based on one imputation of multiply-imputed data ( $n=5$ ) to allow for reporting of standard deviations, reported in parentheses.

### ***Instability in Care and Living Arrangements***

In the current study, I focus on three measures of change and (in)stability in the relationships and structures that define children’s care and living arrangements, taking care to use descriptions that translate across foster and non-foster settings. These measures identify changes in a child’s *primary caregiver*, *secondary caregiver availability*, and *living arrangement*. Together, they provide both relational and structural descriptions of the care arrangements of foster and non-foster youth and change and the number of changes experienced. Instability in each of these aspects of caregiving and household context are operationalized in two ways: as dichotomous measures of change (0 = no change, 1 = change) as well as count measures of the number of waves in which a change is observed (e.g. 0 = no change, 4 = change in every NSCAW wave after the baseline interview). Children’s baseline care and living arrangements are described in the bottom panel of Table 2.1. A change in *primary caregiver* is identified as a between-wave change in the person named as the child’s primary caregiver. The

measure of change in *secondary caregiver availability* indicates whether there is an acquisition or loss of a regular secondary caregiver.

Finally, the child's *living arrangement* is assessed using information from the child's household roster, which enumerates the child's relationship to each household member. These individuals are categorized as being one of the following mutually exclusive and collectively exhaustive relation types: parent figure (biological, step, adoptive, or foster parent), grandparent, sibling, other family member, or other non-family member. Using these household member-specific relationships, I construct binary descriptive indicators of whether the child is observed at any wave living in each of the following household types/living arrangements: single parent figure, two parent figure, extended family (non-sibling, non-parent figure family members), and extended non-family households (non-sibling, non-parent figure household members). A household could fit into more than one of the above categories; for example, a child living in a household with only their biological mother, stepfather, and maternal grandmother, would have a value of "1" for the measures indicating that the child was living in a two-parent figure and extended family household. Using this wave-specific information, I construct a dichotomous change indicator that identifies whether a child experienced a change from "0" to "1" or vice versa on at least one of the above measures of living arrangement type and a count measure that indicates how many times a child experiences a change along each of these dimensions over the study period, with values ranging from 0 (no changes) to 4 (a change in every wave).

For children observed in group home settings at any wave, all of the measures above are adjusted accordingly using the Group Home module, which was administered to a caregiver in the group home in which the child was placed. Children observed in group home settings are coded as having a "non-family" caregiver, as having a secondary caregiver if more than one

adult was responsible for the care of the child, and as being in a living arrangement with non-family members. The child is also noted as living with family members if at least one sibling was living in the same facility. For all measures and all children, if a child was missing information on whether a household member type was present or not in a particular wave, the between-wave periods including that wave (as start or end) were recorded as periods of stability.

Each of these dependent variables is coded in two ways. In the first set of dependent variables, the change and instability measures include all changes in primary caregiver, secondary caregiver availability, and living arrangement observed over the study period. I refer to these measures and analyses using these measures as “all transition” measures and analyses. In the second set of variables, which I refer to as “excess transition” measures, the change and instability measures exclude first-time transitions into foster care and transitions back to a setting with a permanent non-foster caregiver following that initial foster care entry. Substantively, the all transition measures align with the perspective that any instability in children’s living arrangements may be detrimental and/or that current use and levels of foster care placement are overly aggressive and too high. The excess transition measures allow me to examine whether foster care is accomplishing the system’s express objectives of stabilizing children’s care arrangements after what some argue is a necessary and protective disruption. These different measures of change and instability serve to recognize a long-standing, important, and contentious tension among child welfare scholars and advocates regarding whether, how often, and for whom foster care is used as a policy response to perceived domestic threats to a child’s wellbeing (e.g. Bartholet 1999, 2000; Font, Sattler, and Gershoff 2018; Guggenheim 2000).

### ***Foster Care Placement***

The key predictor of interest in these analyses is a summary indicator of whether a child is ever placed in foster care at any point after the baseline survey, either in response to the maltreatment investigation that resulted in their sampling into the NSCAW or to a subsequent investigation. This indicator is based on a combination of retrospective and point-in-time measures collected at each wave of the NSCAW. At each wave, the NSCAW reports whether the child is in foster care at the time of the survey and also asks about any foster care placements since the last date of interview. In the baseline interview, measures ask about any previous foster care placements since the child was born. Data processing by the NSCAW data custodians have resolved discrepancies across child, caregiver, and caseworker reports about this information (Dowd et al. 2008b). Using these data, I construct a summary measure that captures whether the child has been in out-of-home care at any point *after* Wave 1. Throughout the study, I occasionally describe the children who do meet this condition as “foster youth” (515 or 31% of the 1,644 children in the analytic sample) and those who do not as “non-foster youth.” The detailed characteristics of the analytic sample and these two subgroups are presented in Table 1.

### ***Socioeconomic, Demographic, and Maltreatment Investigation Characteristics***

The NSCAW also includes extensive measures of the child’s sociodemographic, economic, health, and behavioral characteristics; similar information about their primary caregiver; as well as extensive details about the nature of the maltreatment investigation that resulted in the child’s inclusion in the NSCAW, including a risk assessment module completed by the investigative caseworker. I draw on different subsets of this information for two stages of the analysis. To estimate the predicted probabilities of foster care placement needed for calculation of the inverse probability weights (discussed in further detail below), I use 31

measures of child, caregiver, and maltreatment investigation characteristics that prior research would suggest the characteristic to be associated with children's likelihood of foster care placement and that the measure of that characteristic in these data is statistically correlated ( $p < 0.10$ ) with a child's likelihood of placement in foster care over the study period.<sup>2</sup> The predictors include child and caregiver demographic characteristics (e.g. race/ethnicity, sex, age); socioeconomic context (e.g. caregiver educational attainment, family economic hardship); child and caregiver health, health behaviors, and daily functioning (e.g. cognitive disability, chronic health condition, child social skills and behavior, caregiver alcohol and drug dependency, caregiver domestic violence victimization); prior caregiver and family child welfare services involvement and characteristics of the maltreatment investigation (e.g. caregiver history of confirmed maltreatment in childhood, alleged maltreatment type, report substantiation); and additional socio-spatial contextual characteristics (e.g. baseline care and living arrangement, sampling stratum, CPS investigator's assessment of poor parenting). The full list of predictors used in this prediction model is reported in Appendix Table 2.2.

### *Analytic Strategy*

My capacity to determine whether foster care placement explains differences between foster and non-foster youths' differential risks of care and living arrangement instability depends on whether the analyses can adequately account for other characteristics and contexts predictive of instability in families and households. I therefore use a propensity-based strategy, proceeding

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<sup>2</sup> Alternative specifications of the prediction model that include all variables available in the data that prior research would suggest are linked to a child's risk of foster care placement were less precise (larger variance) and yielded more extreme values. I therefore elected to use the described more parsimonious but theoretically and empirically-motivated set of measures in the treatment model, as suggested in prior research (Thoemmes and Ong 2016). A statistical comparison of the performance of these specifications is presented in Appendix Table 2.2.

in three stages. First, I estimate the stabilized inverse probability weights (IPWs) using a multivariate probit regression model that estimates a child’s probability of out-of-home care placement as a function of a comprehensive array of pre-placement factors that may be associated with a child’s likelihood of being removed from the home, including characteristics of their initial alleged maltreatment report (Morgan and Harding 2006; Robins 1986; Thoemmes and Ong 2016). This “treatment” model includes a covariate vector,  $X$ , that includes a comprehensive set of measures that account for baseline child, caregiver, family, and maltreatment investigation characteristics. The estimated probit model is presented in Appendix Table 2.2 as marginal effects. The probit expression of a child’s foster care placement is

***Equation 2.1***

$$probit(T = 1 | X) = \phi(\gamma_0 + \gamma_1 X + \varepsilon)$$

The predicted probabilities from the multivariate probit regression model are then converted into stabilized IPWs, which are less susceptible to dependence on extreme observations than unstabilized IPWs (e.g. a child who is extremely unlikely to end up in foster care who is ultimately placed in foster care) and therefore likely to produce estimates with greater precision (Thoemmes and Ong 2016).<sup>3</sup> The stabilized weights for each individual  $i$  are calculated as follows, where  $T_i$  is the predicted probability of being placed in foster care, estimated using the probit model above,  $P(T = 1)$  is the proportion of children placed in foster

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<sup>3</sup> The stabilized IPWs differ from the unstabilized weights in that in calculating the weights, the stabilized IPWs use the proportion of children ever “treated” or placed in foster care,  $P(T = 1)$ , rather than 1 for simple inverses of the propensity score, as is done for calculation of the unstabilized weights. Evaluation of different weight specifications (unstabilized, stabilized, unstabilized and standardized, stabilized and standardized; not shown) corroborated recommendations in Thoemmes and Ong 2016 that suggested that the stabilized weight would likely yield estimates with the smallest variance. A statistical comparison of the performance of these specifications is presented in Appendix Table 2.2.

care, and  $X$  is the complete vector of covariates described above (Morgan and Harding 2006; Robins 1986; Thoemmes and Ong 2016; Wooldridge 2010):

*Equation 2.2*

$$w_i = \frac{P(T = 1)}{P(T = 1 | X)} + \frac{1 - P(T = 1)}{1 - (P(T = 1 | X))}$$

When applied to estimation procedures, the IPWs will more heavily weight children from lower-probability cases—children who are placed in foster care but are not as likely to be or children who are not placed in foster care who ultimately are—and down-weight children who are estimated to be very likely to experience the foster care history that they are observed to have had (with predicted probabilities closer to 0 or 1).

I then estimate a series of three nested logistic and three Poisson regression models of children’s likelihood of experiencing a change in their primary caregiver, availability of a secondary caregiver, and living arrangement and the numbers of changes experienced, respectively, as functions of foster care placement over the study period. The three models incrementally adjust more comprehensively for the selectivity of children who are ultimately placed in foster care. I begin with (1) an unweighted bivariate model of the focal relationships described above (Model 1) and then (2) proceed to a bivariate inverse probability-weighted model that applies the IPWs estimated in the first step of the analysis. Finally, I estimate a (3) a “robust” model that applies the IPWs and further adjusts the estimated association using control measures for characteristics on which the analytic sample differs from the full baseline NSCAW sample as well as characteristics for which the weighting procedure widens absolute normalized differences between the comparison groups.

The IPWs, applied in Models 2 and 3, allow for a comparison of children with statistically similar characteristics and risks of entry into foster care. This makes it more

plausible that any association I find between foster care and instability in children's care and living arrangements is due to their placement in foster care rather than other differences. The combination of the IPWs and additional regression adjustment, as specified in Model 3, is motivated in two important ways. First, it strengthens covariate balance on characteristics for which the IPWs modestly, though non-problematically, exacerbated differences across the foster and non-foster groups, refining the comparison between them (see Figure 2.1 and discussion below; Ho et al. 2007; Stuart 2010). Second, it controls for characteristics along which the analytic sample differs from the full NSCAW sample, addressing the sociodemographic differences in the average characteristics of the analytic sample relative to the national population of child welfare-involved youth in the U.S. (Bollen et al. 2016; Solon, Haider, and Wooldridge 2013; Winship and Radbill 1994).

Unequal residual variances underlying the models compromises the comparability of logistic regression coefficient estimates across specifications. Therefore, I report all results as marginal effects of predicted probabilities of any change and marginal effects of numbers of changes (Allison 1999; Long and Mustillo 2017). I also account for the fact that my observation of a child's care and living arrangement changes in the survey is in part a function of the number of waves for which they are observed in the data. To do so, in addition to including a measure of the total number of NSCAW waves for which a child is observed as a control in the logistic regression models of any change, as described above, I include the measure as an offset term in the count (Poisson) models of numbers of changes observed (Cameron and Trivedi 2013).

## Results

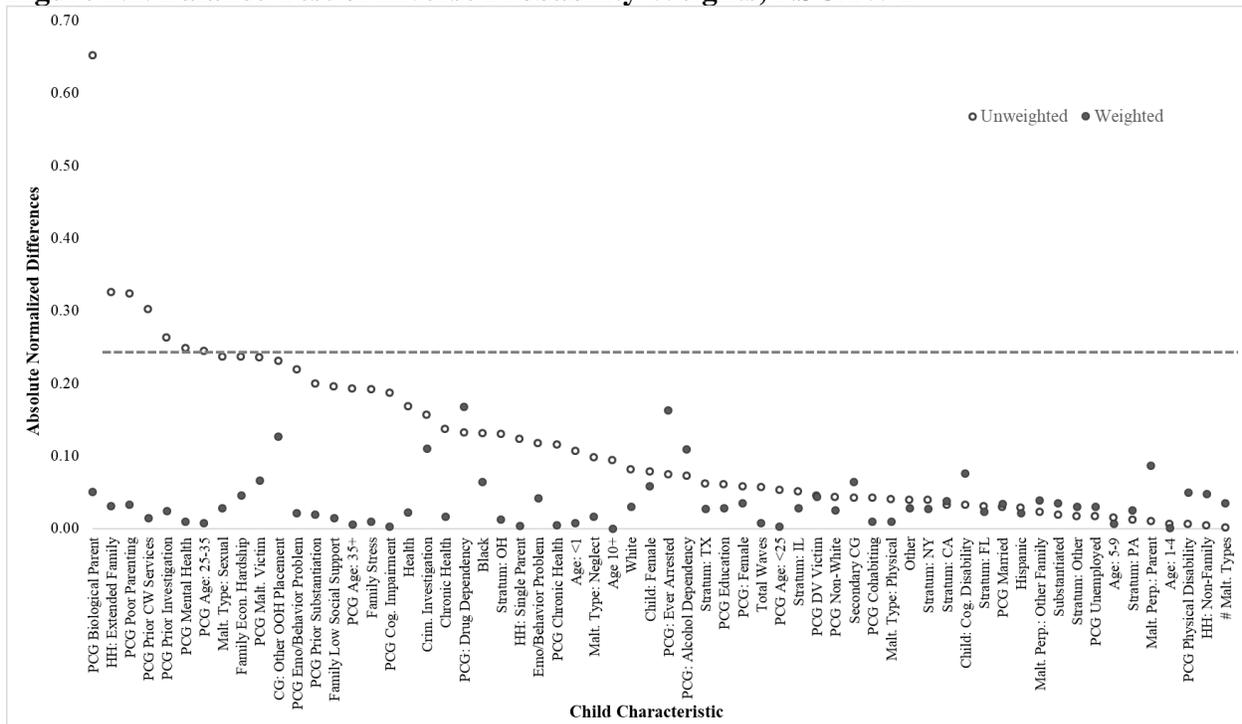
### *Assessment of the Inverse Probability Weights*

I begin by evaluating the balancing capacity of the IPWs that I use to account for children's differential risks of foster care placement. As described above, the IPWs are based on predicted probabilities from a probit "treatment" model that estimates a child's likelihood of enter foster care as a function of a comprehensive set of observed risk factors and baseline characteristics theoretically and empirically correlated with foster care placement (see Appendix Table 2.2). Baseline characteristics that emerged as especially predictive of a child's likelihood of being placed in foster care included family financial hardship, the caregiver's own history of maltreatment as a child, the investigative caseworker's report of the primary caregiver as exhibiting poor parenting, and the child's baseline living arrangement being a single adult-headed household. The predicted probabilities from this probit model then form the basis of the IPWs, or the assigned weight for each child in the analysis.

To test whether the IPWs will accomplish their objective of accounting for variation across the children ever and never placed in foster care, I compare unweighted and IPW-weighted absolute normalized differences between the two groups on observed baseline characteristics that were both included in and excluded from the probit model used to calculate the IPWs. The unweighted differences allow me to assess the degree to which the two groups vary along these observed characteristics, and along which dimensions. The weighted differences then allow us to assess the performance of the IPWs in re-weighting the children in each group such that the two are more similar on these observed characteristics. As recommended in prior methodological work, I consider an absolute normalized difference greater than 0.25 to be indicative of imbalance on the characteristic in question (Stuart 2010; Wooldridge 2010).

As previously shown (Table 2.1), the two comparison groups of children, those ever and never observed in foster care, differ statistically in some important ways that may be correlated with their likelihood of foster care placement and care and living arrangement change and instability. Figure 2.1 notes that a comparison of unweighted absolute normalized differences on these same characteristics shows that even without application of the IPWs, however, the two groups of children are only estimated to be statistically imbalanced along a few baseline characteristics: the investigative caseworker’s assessment that their caregiver demonstrates poor parenting skills, the primary caregiver’s prior receipt of child welfare services, whether the child’s primary caregiver was a biological parent, and whether their household included extended family members.

**Figure 2.1. Balance Test of Inverse Probability Weights, NSCAW I**



Notes: N=1,341. Markers note the absolute values of unweighted (empty points) and inverse probability-weighted (filled points) normalized differences in characteristics between children never and ever placed in foster care in the study period. The dotted line marks the value for the maximum acceptable absolute normalized difference for a balanced sample (Wooldridge 2010). “HH” is shorthand for “household” and “PCG” is shorthand for “primary caregiver.”

Turning to the weighted differences, it is clear that the application of the IPWs reduces any problematic differences to magnitudes well below the threshold and diminishes differences with respect to most characteristics. The fact that this is true even for measures excluded from the probit model used to calculate the IPWs suggests that this weighting procedure could plausibly mitigate some unobserved differences between the foster and non-foster youth in this analysis. For the characteristics for which absolute normalized differences increase with weighting, I include them in the multivariate regression models—along with those included to account for statistical differences between the analytic sample and all NSCAW children<sup>4</sup>—to achieve further balance across the foster and non-foster groups (Ho et al. 2007; Stuart 2010).

### ***Foster Care Placement and Any Change***

A description of the children’s baseline care and living arrangements is presented in Table 2.1. By design, nearly all, or 92%, of children in the analytic sample had a biological, adoptive, or step-parent as their primary caregiver, and the majority (56%) had a regular secondary caregiver. 61 percent of children lived in single adult-headed living arrangements, just over one-quarter of children lived with at least one extended family member, and 13 percent lived with at least one non-family member. Children who were ever in foster care were more likely than those who did not to have a non-parent as a primary caregiver (98% vs. 78%), less likely to be living in a single parent-headed living arrangement (57% vs. 63%), and more likely to be living with extended family (37% vs. 22%).

Table 2.2 provides a summary of the key outcome variables, indicators of change in care and living arrangements and the number of changes experienced. Using measures including all changes observed across the observation period, children who were ever in foster care were 1.2

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<sup>4</sup> There is some overlap across these two sets of measures.

to 5.8 times more likely to experience any change and likely to experience more changes than those who did not enter foster care over the study period (Table 2.2).

**Table 2.2. Measures of Care and Living Arrangement Change, NSCAW I**

	Overall	Ever in Foster Care	
		Never	Ever
<b>All Transitions</b>			
Primary Caregiver			
Any Change (Proportion)	0.35	0.14***	0.81
Number of Changes	0.53	0.17***	1.30
	(0.83)	(0.47)	(0.92)
Secondary Caregiver Availability			
Any Change (Proportion)	0.55	0.52***	0.63
Number of Changes	0.86	0.81**	0.97
	(0.94)	(0.94)	(0.92)
Living Arrangement			
Any Change	0.72	0.66***	0.86
Number of Changes	1.35	1.19***	1.71
	(1.14)	(1.12)	(1.11)
<b>Excess Transitions</b>			
Primary Caregiver			
Any Change (Proportion)	0.20	0.14***	0.35
Number of Changes	0.24	0.17***	0.39
	(0.51)	(0.47)	(0.57)
Secondary Caregiver Availability			
Any Change (Proportion)	0.47	0.52***	0.35
Number of Changes	0.68	0.81***	0.40
	(0.87)	(0.94)	(0.60)
Living Arrangement			
Any Change (Proportion)	0.63	0.66***	0.55
Number of Changes	1.05	1.19***	0.74
	(1.05)	(1.12)	(0.79)
<i>N</i>	1,644	1,129	515

Notes: Asterisks denote statistically significant differences between non-foster and foster youth at the following significance levels: \*\*\* $p < 0.001$  \*\* $p < 0.01$  \* $p < 0.05$ . Estimates are unweighted and based on one imputation of multiply-imputed data ( $n=5$ ) to allow for reporting of standard deviations, reported in parentheses.

To distinguish between more direct consequences of foster care and those of other experiences and characteristics in explaining the instability of the care and living arrangements of foster-involved youth, I now turn to a series of regression models. The results are reported as

marginal effects, or estimated percentage point differences in predicted probabilities of caregiver and living arrangement change (Table 2.3) and estimated differences in the predicted numbers of changes experienced (Table 2.4), using both the all-transition and excess transition measures. Moving from left to right across each table, the models account increasingly for selection into foster care. Model 1 is a bivariate regression that estimates a child's likelihood of experience change in each dimension of their care and living arrangements as a function of entering foster care after baseline. Model 2 applies the IPWs, and Model 3 adds covariate adjustments to improve balance across the comparison groups and to account for the selectivity of the analytic sample relative to the baseline NSCAW I sample.

As previewed by the descriptive statistics, unadjusted models of change inclusive of all transitions estimate strong associations between children's likelihoods of change in all three dimensions of their care and living arrangements (Table 2.3, column 1). Children ever placed in foster care were 67 percentage points more likely to experience at least one change in their primary caregiver, 10 percentage points more likely to experience a change in their secondary caregiver, and 20 percentage points more likely to experience a change in their living arrangement than children never observed in foster care over the study period. Estimates from inverse probability-weighted models (column 2), indicates that some of the disparity is captured by differential risk of placement in foster care as estimated by the observable characteristics in the treatment model estimated in the first stage of the analysis. However, these differences remain statistically distinguishable and substantial in magnitude. The covariate adjustment in Model 3 (column 3) leaves these estimates essentially untouched. In the full robust model, foster care is associated with a 45 percentage-point higher likelihood of a change in primary caregiver,

a 12 percentage-point higher likelihood of change in the availability of a secondary caregiver, and a 23-percentage point higher likelihood of experiencing a change in living arrangement.

**Table 2.3. Estimated Associations between Foster Care Placement and Likelihood of Care and Living Arrangement Change, NSCAW I**

	<b>Model 1</b>	<b>Model 2</b>	<b>Model 3</b>
<b>All Transitions</b>			
Primary Caregiver (PCG)	0.67*** (0.63, 0.71)	0.45*** (0.43, 0.48)	0.45*** (0.43, 0.47)
Secondary Caregiver (SCG) Availability	0.10*** (0.05, 0.15)	0.12*** (0.06, 0.17)	0.12*** (0.07, 0.17)
Living Arrangement	0.20*** (0.16, 0.24)	0.24*** (0.18, 0.30)	0.23*** (0.18, 0.29)
<b>Excess Transitions</b>			
Primary Caregiver (PCG)	0.21*** (0.16, 0.26)	0.17*** (0.12, 0.21)	0.16*** (0.12, 0.21)
Secondary Caregiver (SCG) Availability	-0.18*** (-0.23, -0.12)	-0.19*** (-0.24, -0.13)	-0.18*** (-0.24, -0.13)
Living Arrangement	-0.11*** (-0.16, -0.06)	-0.11*** (-0.17, -0.05)	-0.11*** (-0.17, -0.05)
Inverse Probability Weights		X	X
Covariates			X

Note: N=1,644. Asterisks denote statistically significant differences between non-foster and foster youth at the following significance levels: \*\*\*p<0.001 \*\*p<0.01 \*p<0.05. Estimates are based on logistic regression models of likelihoods of change estimated using multiply-imputed data (n=5). Results are reported as marginal effects (percentage-point difference in likelihood of change), with 95% confidence intervals in parentheses.

***Foster Care Placement and “Excess” Change***

Next, I examine whether these differences persist if focusing only on excess change, or, alternatively, whether the observed differences in caregiver and household change and instability are explained in full by the child’s first removal from the home and subsequent transition back to a permanent care setting. The excess transition measures used in this part of the analysis have the

substantive meaning of treating first-time foster care placements and subsequent transitions to permanent caregivers as necessary and/or beneficial for child protection and thus excludes them from my measurement of change. As shown in the bottom panel of Table 2.2, there are again statistically distinguishable baseline differences between youth ever and never placed in out-of-home care, though with very different patterning. Children ever in foster care are 2.5 times more likely than children never in foster care to experience a change of primary caregiver. However, foster youth are now statistically less likely than non-foster youth to experience change and instability in secondary caregiver availability and their living arrangements.

Next, I re-estimate the regression models using these excess transition measures. These results are presented in the bottom panel of Table 2.3. The estimated effects of foster care placement vary across components of children's care and living arrangements. Children who ever enter foster care remain statistically more likely to experience a change in their primary caregiver than children who do not go into foster care, with foster youth still 16 percentage points more likely to experience a change in primary caregiver in the robust models. However, with the excess transition measures, foster care placement is associated with statistically lower likelihoods of changes in the other two dimensions of children's care and living arrangements. Foster youth are 18 percentage points and 11 percentage points less likely to experience change in the availability of a regular secondary caregiver and their living arrangement, respectively.

### ***Foster Care Placement and Instability***

Beyond dichotomous measures of any change, another way to think about instability in these children's social contexts is to examine frequency or rates of change in children's care and living arrangements. The next set of models estimate the number of changes that children experienced as functions of ever being placed in foster care over the study period; these results

are presented in Table 2.4. Using all transition measures, foster youth are estimated to be likely to experience more changes, or greater instability (Table 2.4), in addition to being more likely to experience any change (Table 2.3). The robust model (column 3) estimates that children who entered foster care experienced 1.9 more changes in their primary caregiver, 0.2 more changes in their secondary caregiver availability, and 0.4 more changes in their living arrangement than children not observed in foster care over the five NSCAW waves.

**Table 2.4. Estimated Associations between Foster Care Placement and Number of Changes in Care and Living Arrangements, NSCAW I**

	<b>Model 1</b>	<b>Model 2</b>	<b>Model 3</b>
<b>All Transitions</b>			
Primary Caregiver (PCG)	2.00*** (1.84, 2.16)	1.85*** (1.58, 2.12)	1.87*** (1.60, 2.13)
Secondary Caregiver (SCG) Availability	0.16** (0.05, 0.26)	0.17** (0.05, 0.29)	0.18** (0.06, 0.30)
Living Arrangement	0.34*** (0.25, 0.42)	0.36*** (0.26, 0.46)	0.38*** (0.27, 0.49)
<b>Excess Transitions</b>			
Primary Caregiver (PCG)	0.80*** (0.60, 0.99)	0.58*** (0.28, 0.89)	0.59*** (0.32, 0.86)
Secondary Caregiver (SCG) Availability	0.16** (0.05, 0.26)	0.17** (0.05, 0.29)	0.18** (0.06, 0.30)
Living Arrangement	-0.50*** (-0.61, -0.38)	-0.49*** (-0.63, -0.35)	-0.48*** (-0.62, -0.33)
Inverse Probability Weights		X	X
Covariates			X

Note: N=1,644. Asterisks denote statistically significant differences at the following significance levels: \*\*\*p<0.001 \*\*p<0.01 \*p<0.05. Estimates are based on Poisson models with an offset term for the number of waves an individual was observed using multiply-imputed data (n=5). Results are reported as marginal effects (difference in number of changes) with 95% confidence intervals reported in parentheses.

Again, using the excess transition measures (Table 2.4), we see that foster care is associated with lower levels of instability in children's living arrangements (0.5 fewer changes). However, foster youth continue to experience approximately 0.6 more changes in their primary caregiver and 0.2 changes in their secondary caregiver availability than non-foster youth. These results indicate a mixed story about the consequences of foster care for children's exposure to instability in their care and living arrangements. In the short- to medium-term period for which these children are observed in the NSCAW, foster care appears to have modestly-to-significantly destabilizing effects for children's care and living arrangements if we consider all changes to be preventable and/or detrimental. If excluding first entries into foster care and subsequent transitions to permanency, these findings suggest that foster care has potentially stabilizing effects on the structure of children's care arrangements following protective disruptions. However, foster youth are still estimated to experience greater volatility in relationships potentially central to their care and immediate social contexts: their primary caregivers.

### ***Racial/Ethnic Disparities in Foster Care Placement and Change and Instability***

The estimates discussed up until this point provide compelling evidence of the consequences of foster care placement for children's care and living arrangement instability. However, given the unequal and disproportionate representation of children of color in the child welfare system at all levels (e.g. Yi, Edwards, and Wildeman 2020), one might be both interested in and concerned about whether these dynamics are inconsistent across racial/ethnic groups. To investigate this possibility, I conduct the same analysis separately for non-Hispanic White children, non-Hispanic Black children, and children of Hispanic ethnicity. Small cell sizes preclude analysis of children identified as being American Indian/Native American/Alaska Native, Asian/Pacific Islander, or of another race and non-Hispanic. Marginal effects of

predicted probabilities of change and predicted numbers of changes estimated from the robust models (Model 3) are presented in Table 5. Non-Hispanic Black children in the analytic sample were six to seven percentage points (36%) more likely to ever be observed in foster care than White (29%) or Hispanic (30%) children. Unadjusted baseline probabilities and average numbers of changes are relatively similar across racial/ethnic groups for all three descriptors of children's care and living arrangements.

Within racial/ethnic groups, the overarching trends in associations between foster care placement and change and instability in children's care and living arrangement are consistent with conclusions drawn from the pooled analysis (Tables 2.3 and 2.4). Estimates using all transition measures again indicate that foster care is associated with higher likelihoods and greater numbers of changes in children's primary caregiver and secondary caregiver availability for children of all three racial/ethnic groups. There is no foster care differential for Hispanic children's experiences of change and instability in their living arrangement, but this association remains statistically distinguishable for children who are White and Black. Using the excess transition measures, foster care placement continues to be associated with a higher likelihood of primary caregiver change and more primary caregiver changes. However, just as in the pooled analyses, foster placement is now associated with a lower likelihood of change and fewer changes in secondary caregiver availability and living arrangement.

These results speak to whether the general associations between foster care placement and children's care and living arrangement experiences holds *within* each racial/ethnic group. However, these stratified models also importantly allow for comparisons *across* racial/ethnic groups and identification of potential racial variation in the foster care differential in or "penalty" to the stability or volatility that children experience. Referring again to the results presented in

Table 2.5, statistically distinguishable differences between racial/ethnic groups in the marginal effect of foster care on predicted probabilities of change or predicted numbers of changes experienced can be identified by comparing the 95% confidence intervals of the estimated marginal effects. Three key findings emerge.

**Table 2.5. Estimated Associations between Foster Care Placement and Likelihood and Number of Care and Living Arrangement Change(s), by Race/Ethnicity, NSCAW I**

	White (N = 716)		Black (N = 515)		Hispanic (N = 303)	
	Mean or %	ME	Mean or %	ME	Mean or %	ME
<b>All Transitions</b>						
Primary Caregiver Change	0.36	0.45*** (0.43, 0.48)	0.35	0.43*** (0.39, 0.47)	0.33	0.47*** (0.42, 0.52)
Number of Changes	0.54 (0.83)	1.59*** (1.31, 1.87)	0.51 (0.80)	2.52*** (2.18, 2.87)	0.53 (0.88)	1.96*** (1.39, 2.52)
SCG Availability Change	0.54	0.13** (0.04, 0.21)	0.55	0.12* (0.02, 0.23)	0.61	0.06 (-0.04, 0.17)
Number of Changes	0.81 (0.91)	0.19* (0.02, 0.36)	0.89 (1.00)	0.25* (0.01, 0.50)	0.94 (0.94)	0.13 (-0.12, 0.38)
Living Arrangement Change	0.73	0.20*** (0.13, 0.28)	0.71	0.23*** (0.14, 0.33)	0.75	0.29*** (0.16, 0.41)
Number of Changes	1.36 (1.15)	0.31*** (0.17, 0.47)	1.36 (1.17)	0.49*** (0.33, 0.65)	1.40 (1.11)	0.41*** (0.19, 0.63)
<b>Excess Transitions</b>						
Primary Caregiver Change	0.21	0.08* (0.01, 0.15)	0.19	0.22*** (0.16, 0.28)	0.20	0.27*** (0.17, 0.38)
Number of Changes	0.24 (0.54)	0.16 (-0.19, 0.52)	0.22 (0.47)	1.27*** (0.88, 1.66)	0.25 (0.54)	1.00** (0.37, 1.63)
SCG Availability Change	0.45	-0.19*** (-0.28, -0.11)	0.46	-0.16** (-0.27, -0.06)	0.53	-0.21*** (-0.31, -0.11)
Number of Changes	0.64 (0.83)	-0.77*** (-1.03, -0.50)	0.70 (0.93)	-0.66*** (-1.01, -0.30)	0.77 (0.88)	-0.73*** (-1.06, -0.40)
Living Arrangement Change	0.63	-0.15** (-0.23, -0.07)	0.60	-0.12* (-0.23, -0.01)	0.65	-0.00 (-0.12, 0.12)
Number of Changes	1.06 (1.06)	-0.61*** (-0.82, -0.39)	1.03 (1.07)	-0.39** (-0.62, -0.15)	1.10 (1.03)	-0.34* (-0.60, -0.07)
Foster Care (%)		0.29		0.36		0.30

Note: "SCG" stands for secondary caregiver. Asterisks denote statistically significant differences at the following significance levels: \*\*\*p<0.001 \*\*p<0.01 \*p<0.05. Estimates are reported as

marginal effects from robust inverse probability-weighted regression models, to be interpreted as percentage-point difference in likelihood of change and difference in number of changes. All estimates draw on multiply-imputed data ( $n=5$ ) and have 95% confidence intervals reported in parentheses. Small cell sizes do not allow for separate analysis of non-Hispanic children of other racial/ethnic groups.

First, estimates using all-transition measures of primary caregiver instability indicate that the foster-non-foster differential in primary caregiver instability is statistically distinguishably larger for Black children than for White children. While Black children ever placed in foster care are likely to experience 2.5 more primary caregiver changes than those never observed in foster care, among White children, foster youth are likely to experience 1.6 more such transitions than White non-foster children. Second, using excess transition measures of change, Hispanic children have a larger foster care differential in the likelihood of experiencing a primary caregiver change than White children; while Hispanic children ever in foster care are estimated to experience one more change in their primary caregiver than co-ethnic children never observed in foster care, this differential is much more modest for non-Hispanic White children (0.08). Finally, Black children, like those who are Hispanic, have a statistically distinguishably larger foster care differential in the likelihood of experiencing a primary caregiver change than White children (0.22 vs. 0.08). However, Black children have a substantially and statistically distinguishably larger foster care differential in the number of primary caregiver changes they are likely to experience compared to that estimated among White children. In fact, while Black foster youth, on average, experienced 1.27 more primary caregiver changes than Black children never placed in foster care, among White children, this differential was statistically indistinguishable from zero (0.16, 95% CI = -0.19, 0.52).

**Table 2.6. Estimated Associations between Foster Care Placement and Likelihood of Care and Living Arrangement Change(s), Restricted and NSCAW II Samples**

	NSCAW I			NSCAW II
	Married Biological Parent	Other Care Arrangement	Observed All Waves	
<b>All Transitions</b>				
Primary Caregiver (PCG) Change	0.44*** (0.40, 0.49)	0.47*** (0.44, 0.49)	0.47*** (0.43, 0.50)	0.57*** (0.52, 0.62)
Number of Changes	1.96*** (1.45, 2.47)	1.84*** (1.56, 2.13)	1.87*** (1.57, 2.17)	na
Secondary Caregiver (SCG) Presence Change	0.24*** (0.13, 0.33)	0.08** (0.02, 0.14)	0.10** (0.03, 0.16)	0.16*** (0.09, 0.22)
Number of Changes	0.56** (0.25, 0.86)	0.10 (-0.04, 0.24)	0.15* (0.20, 0.27)	na
Living Arrangement Change	0.23*** (0.11, 0.34)	0.24*** (0.17, 0.30)	0.21*** (0.15, 0.27)	0.20*** (0.15, 0.25)
Number of Changes	0.37** (0.16, 0.57)	0.38*** (0.27, 0.49)	0.38*** (0.27, 0.48)	na
<b>Excess Transitions</b>				
Primary Caregiver (PCG) Change	0.09 (-0.02, 0.20)	0.18*** (0.13, 0.22)	0.18*** (0.13, 0.23)	0.09*** (0.05, 0.13)
Number of Changes	0.29 (-0.42, 1.00)	0.64*** (0.38, 0.89)	0.67*** (0.38, 0.95)	na
Secondary Caregiver (SCG) Presence Change	-0.02 (-0.14, 0.10)	-0.23*** (-0.28, -0.17)	-0.19*** (-0.25, -0.13)	-0.02 (-0.07, 0.02)
Number of Changes	-0.36 (-0.73, 0.02)	-0.79*** (-0.98, -0.60)	-0.73*** (-0.92, -0.54)	na

*(Continued on next page)*

**Table 2.6, continued.**

Living Arrangement Change	-0.14* (-0.25, 0.03)	-0.11*** (-0.18, -0.04)	-0.11** (-0.18, -0.05)	-0.25*** (-0.30, -0.19)
Number of Changes	-0.59*** (-0.87, -0.31)	-0.46*** (-0.60, -0.32)	-0.42*** (-0.56, -0.28)	na
Covariates	X	X	X	X
Inverse Probability Weights	X	X	X	X
<i>N</i>	388	1,185	1,192	2,661

Note: Asterisks denote statistically significant differences at the following significance levels: \*\*\* $p < 0.001$  \*\* $p < 0.01$  \* $p < 0.05$ .

Estimates of change are reported as marginal effects (percentage-point difference in likelihood of change). Estimates of numbers of changes are reported as marginal effects (difference in number of changes). All estimates draw on multiply-imputed data ( $n=5$ ) and have 95% confidence intervals reported in parentheses. “na” indicates that the model was not estimated in the NSCAW II as the shorter study period (3 waves) precluded estimation of a count model of number of changes.

### *Supplementary Analyses*

These analyses face some important limitations, which I describe here and then attempt to address with additional analyses, the results of which are presented in Table 2.6. First, it is possible that the patterns of change and instability estimated to be associated with foster care placement are ultimately explained by children's baseline living arrangements, which we observed to be statistically distinguishably associated with their foster care entry and are known predictors of family and household instability (e.g. McLanahan and Percheski 2008). Second, my ability to capture instability in children's care and living arrangements is in part a function of the number of times or the duration for which I observe their care and living arrangements in the NSCAW I. If children are observed for unequal lengths of time in ways that are correlated with their entry into foster care placement and/or changes in their care and living arrangements, my estimates may be biased upwardly by the experiences of children observed for comparatively longer periods of time and thus with more points of observation and potential changes. Third, much of the information I use to account for children's and caregivers' characteristics and experiences is self-reported. Given the sensitivity of the experiences that made families eligible for participation in the NSCAW I (i.e. investigation of child maltreatment investigations) and some of the survey content (e.g. caregivers' victimization and substance use), there is reason to be concerned about the accuracy of some of this information.

To test whether the associations I find in the pooled and racially stratified models are simply reflecting the experiences of children who were in baseline care and living arrangements at higher risk of instability, independent of the children's subsequent entry into foster care, I estimate additional models stratified by children's baseline living arrangements. For these models, the analytic sample is split into two groups: children who were observed at baseline in-

home in a married two-parent household with at least one biological parent and children who were observed in any other care and living arrangement. This sample stratification for supplementary analysis is motivated by family demographic research that finds that children in married two-parent households are less likely to experience family and household instability (e.g. Aquilino 1996; Cherlin and Seltzer 2014). As shown by the results shown in Table 2.6 (columns 1 and 2), the general patterns observed here are consistent with those estimated in the main analyses, using both the all-transition and excess transition measures. This suggests that the foster care differentials I estimated are not artifacts of children's earlier care and living arrangement trajectories.

In the main analyses, I account explicitly for varying duration of children's observation periods in the NSCAW I in my logistic and Poisson regression models using control and offset terms, respectively. Further, children ever and never in foster care in my analytic sample are not statistically different in the number of waves for which they are observed. However, as a supplementary assessment to address potential unobserved selection associated with attrition or different lengths of observation periods, I estimate the same models using the subset of children in the analytic sample who are observed for all five waves. I find that these supplementary estimates of children's likelihoods of experiencing change in their primary caregiver, secondary caregiver availability, and living arrangement using these data corroborate findings in the main analyses for the current study (see Table 2.6, column 3). The fully adjusted and inverse probability weighted models using all-transition measures of change are associated with heightened probabilities of change across all three descriptors of children's care and living arrangements. When using excess transition measures, foster care remains positively associated with the likelihood of primary caregiver change, not statistically associated to any change in the

secondary caregiver availability, and negatively associated with the likelihood of change in living arrangements (Table 2.6, column 3).

To explore concerns about the reliability and accuracy of the self-reported items in the NSCAW I, I estimate supplementary models of any change using logistic regression analyses of data from the NSCAW II (Dowd et al. 2008). In addition to following a different group of CPS-investigated children and families for a shorter more recent period (three waves collected between 2008 and 2012), the NSCAW II differs from the NSCAW I in that it draws maltreatment case and foster care history information from linkages to administrative data from the National Child Abuse and Neglect Data System and Adoption and Foster Care Reporting System, respectively. These data sources are much less likely to be affected by respondent error due to recall error or social desirability biases, for example, and thus provide a useful comparison to the NSCAW I analyses. If respondent error is systematic and thus threatens the conclusions I draw from the NSCAW I, I would expect findings to be inconsistent across my analyses of the NSCAW I and II. However, I find that that is not the case and that the estimates are, in fact, confirmatory (Table 2.6, column 4). This does not rule out the possibility that self-reported measures are imperfect in capturing some aspects of living conditions, wellbeing, and system involvement, but does address concerns regarding the degree to which the self-reported measures are problematic in ways that may bias the main findings.

## **Discussion**

Family instability is a key dimension of social inequality, linked to disparities in children's wellbeing in early life and over the life course (e.g. Crosnoe et al. 2014; Fomby and Cherlin 2007). Further, institutional contact—such as involvement with the child welfare

system—plays an important role in shaping inequality in children’s risks of experiencing family change and volatility in familial relationships and structures. In this study, I examine the association between foster care placement and instability in children’s care and living arrangements by leveraging data from the National Survey of Child and Adolescent Wellbeing and inverse probability-weighted regression analyses that account for the fact that entry into foster care is a non-randomly distributed experience. In addition to answering an important question about the consequences of foster care placement for children’s family instability and wellbeing, this investigation aims to situate the experiences of foster youth in the broader context of children’s family instability in the U.S.

Taken together, my analyses point to four key conclusions about the relationship between foster care placement and family instability. First, although the focus of this study was to situate the social instability of foster youth, the high unadjusted rates of change and instability across the entire sample of youth ever investigated by child protective services are notable. Second, youth ever placed in foster care are more likely to experience changes in both the structures of their care and living arrangements as well as key components of the relationships that comprise them, even after accounting comprehensively for their differential risks of foster care entry. However, children’s care arrangements are multidimensional: when exclusively counting changes other than initial entries into foster care and subsequent transitions to permanency, foster care is associated with greater stability in the structures of children’s care and living arrangements, namely the availability of a regular secondary caregiver and their living arrangement. Children ever placed in foster care remain, however, at higher risk of experiencing instability in their primary caregiver, a key relationship likely to be related to the quality of children’s care and family life. Fourth, and finally, although foster care differentials in likelihoods of change and

instability are consistent for White, Black, and Hispanic children when examined separately, the foster differential or “effect” in likelihoods and rates of primary caregiver change are notably larger for Black children, particularly when compared with White children, using both all transition and excess transition measures.

These analyses are constrained in a couple of ways that limit the degree to which these results can speak to more general conclusions about the child welfare-involved population. First, although the NSCAW data are nationally representative of children who were subjects of child welfare investigations during the sampling time frame, due to the sample inclusion criteria necessary for this study, the results presented here are not fully generalizable to that target population. Second, these analyses may miss changes in living arrangements that occur between waves of data collection. Although supplementary analyses with the NSCAW II, which differs in the length of the observation period, largely confirm the results of this study, providing some suggesting evidence that my findings are not likely to be biased by the timing of the care and living arrangement information used in these analysis, this remains a shortcoming of this study. Finally, although the inverse probability weighting strategy that I employ in these analyses do achieve covariate balance on observed characteristics across the comparison groups, the modest explanatory power of the treatment model does not allow me to draw stronger conclusions about the direct effects of foster care placement on family instability. Propensity-based methods are part of the array of quasi-experimental strategies potentially useful for estimation of causal effects (Morgan and Harding 2006; Wooldridge 2010). However, in this study, I use this method to provide a more robust comparison of non-foster and foster youth and their exposure to social instability, rather than to estimate a causal or “treatment” effect of foster care placement. As

such, these results should be interpreted as important to our understanding of causal processes with contact with the child welfare and foster care systems, but descriptive in nature.

Nonetheless, this study builds upon prior family demographic and child welfare research on instability caregiving and household arrangements in three important ways. First, the NSCAW data, the use of which has been near-exclusive to the fields of social work and child development, is the only nationally representative data set that allows for a comparison of foster and non-foster youth who are not confined to a specific child welfare agency/system (e.g. child welfare records from a single state or county) or geographic region of the United States. Second, I define caregiver and household arrangements and characteristics using typologies that can describe both non-foster and foster youths' experiences to allow for comparison across two groups that are often studied separately. Finally, leveraging the sample design, longitudinal structure, and rich detail of the NSCAW data, I am able to account for elements of selection into foster care placement to better isolate the relationship between out-of-home care placement and the likelihood of experiencing caregiver and household change. In so doing, this study brings the experiences of child welfare-involved youth and the contours of their exposure to family change in dialogue with a growing body of work that examines the consequences of institutional contact for family diversity, instability, and inequality.

## Appendix 2

**Appendix Table 2.1. Unweighted Comparison of Baseline and Analytic Samples, NSCAW I**

	Overall	Analytic Sample	
		Excluded	Included
<b>Child and Caregiver Demographics</b>			
<b>Child Demographics</b>			
Age (%)			
< 1 Year	0.20	0.20	0.21
1-4 Years	0.26	0.26	0.27
5-9 Years	0.27	0.27	0.26
10+ Years	0.27	0.27	0.25
Female	0.50	0.51	0.49
Race/Ethnicity			
White	0.43	0.43	0.44
Black	0.32	0.33	0.31
Hispanic	0.17	0.17	0.18
Other	0.07	0.08	0.07
<b>Caregiver Demographics</b>			
Age (%)			
< 25 Years	0.22	0.19***	0.31
25-35 Years	0.35	0.33***	0.40
35+ Years	0.42	0.48***	0.29
Female	0.92	0.91*	0.93
Race/Ethnicity (% Non-White)	0.50	0.50	0.51
<b>Socioeconomic Context</b>			
Caregiver Education (% Some Postsecondary)	0.26	0.28	0.21
Unemployment Status	0.11	0.09***	0.14
Family: Economic Hardship	0.36	0.35	0.37
Family: Low Social Support	0.41	0.40**	0.44
<b>Child Welfare Services Contact</b>			
Alleged Maltreatment Type(s) (%)			
Physical Abuse	0.30	0.30	0.31
Sexual Abuse	0.13	0.13	0.14
Neglect	0.62	0.61	0.62
Number of Types of Maltreatment (2+)	0.34	0.34	0.33
Alleged Perpetrator(s) (%)			
Parent	0.67	0.66**	0.70
Other Family	0.09	0.10	0.08
Substantiated/Indicated Maltreatment	0.61	0.60**	0.64
Criminal Investigation	0.28	0.29	0.27
Prior CPS Investigation	0.49	0.49	0.48
Prior CPS Report Substantiation	0.32	0.34**	0.30
Prior Child Welfare Services Receipt	0.39	0.40**	0.36
Other Child Placed in Foster Care	0.12	0.09***	0.16
Own Maltreatment in Childhood	0.32	0.32	0.33

(Continued on next page)

**Appendix Table 2.1, continued.**

<b>Child and Caregiver Health</b>			
<b>Child Health</b>			
Cognitive Disability	0.10	0.10	0.10
General Health	4.05	4.05	4.04
	(1.02)	(1.01)	(1.03)
Chronic Health Condition	0.29	0.28	0.30
Behavioral Problem	0.25	0.24	0.27
<b>Caregiver Health</b>			
Cognitive Disability	0.10	0.11	0.10
Physical Disability	0.03	0.03**	0.02
Chronic Health Condition	0.20	0.20	0.19
Mental Health	0.26	0.27**	0.23
Emotional Difficulty	0.32	0.31	0.34
Alcohol Dependency	0.02	0.02**	0.03
Drug Dependency	0.03	0.02***	0.05
Domestic Violence Victimization	0.37	0.37	0.37
Family Stress	0.65	0.64**	0.68
<b>Family/Household and Sociospatial Context</b>			
Poor Parenting (Caseworker Report)	0.52	0.53	0.50
<b>Caregiver Time with Child</b>			
<6 Months	0.26	0.32***	0.12
6-24 Months	0.28	0.28	0.30
24-120 Months	0.30	0.27***	0.38
120+ Months	0.16	0.14***	0.20
Caregiver Ever Arrested	0.27	0.25***	0.33
<b>Stratum</b>			
California	0.13	0.13	0.11
Florida	0.05	0.05	0.06
Illinois	0.05	0.05**	0.07
Michigan	0.06	0.06	0.06
New York	0.07	0.07	0.07
Ohio	0.06	0.05	0.06
Pennsylvania	0.05	0.05	0.06
Texas	0.09	0.08**	0.11
Remainder	0.43	0.45***	0.40
Number of Waves Observed	4.37	4.26***	4.63
	(0.97)	(1.05)	(0.67)

*(Continued on next page)*

**Appendix Table 2.1, continued**

<b>Baseline Care and Living Arrangement</b>			
<b>Primary Caregiver (PCG)</b>			
Type: Biological Parent	0.68	0.58***	0.92
Cohabiting w/Partner	0.14	0.13***	0.17
Married	0.37	0.42***	0.27
<b>Secondary Caregiver Present</b>	0.59	0.60**	0.56
<b>Living Arrangement</b>			
Single Parent	0.48	0.43***	0.61
Extended Family Members	0.29	0.30*	0.27
Non-Family Members	0.16	0.17***	0.13
<i>N</i>	1,644	1,129	515

Notes: \*\*\*p<0.001 \*\*p<0.01 \*p<0.05 indicate statistically significance differences from the analytic sample. Estimates are unweighted and based one imputation of multiply-imputed data (n=5) to allow for reporting of standard deviations, which are reported in parentheses.

Notes: \*\*\*p<0.001 \*\*p<0.01 \*p<0.05 indicate statistically significance differences from the analytic sample. Estimates are unweighted and based one imputation of multiply-imputed data (n=5) to allow for reporting of standard deviations, which are reported in parentheses.

**Appendix Table 2.2. Comparison of Inverse Probability Weight Specifications, NSCAW I**

<b>Weight Summary</b>	<b>(1)</b>	<b>(2)</b>	<b>(3)</b>	<b>(4)</b>	<b>(5)</b>	<b>(6)</b>
Mean	1.97	0.99	1.00	1.98	0.99	1.00
(SD)	(1.61)	(0.58)	(0.58)	(1.58)	(0.55)	(0.55)
Variance	2.59	0.33	0.34	2.50	0.30	0.30
Minimum	1.01	0.32	0.32	1.02	0.32	0.32
Maximum	17.75	8.61	8.72	19.48	7.86	7.95
<b>Predictor Specification</b>						
Theoretical predictors	X	X	X			
Correlated subset				X	X	X
<b>Transformations</b>						
Stabilized		X	X		X	X
Scaled			X			X

Note: The main analyses use weight specification (5).

The “theoretical predictors” included in the probit model to calculate weights (1), (2), and (3) are: child’s baseline characteristics (race/ethnicity, sex, age); baseline caregiver characteristics (race/ethnicity, sex, age); baseline socioeconomic context (caregiver education, caregiver unemployment status, family financial hardship, family low social support, caregiver history of arrest); baseline child and caregiver health and health behaviors (child cognitive disability, child health, child and caregiver chronic health conditions, child behavioral problem, caregiver cognitive problem, caregiver physical disability, caregiver mental health issue, caregiver emotional difficulty, family high stress, caregiver alcohol dependency, caregiver drug dependency, caregiver domestic violence victimization); child welfare system contact history and baseline maltreatment investigation characteristics (alleged maltreatment types, number of types of maltreatment alleged, criminal investigation, prior child protective services (CPS) investigation, prior CPS substantiation, prior child welfare services, other child’s foster care placement, caregiver history of child maltreatment, baseline maltreatment report substantiation status); sociospatial context characteristics (caregiver poor parenting, baseline care and living arrangement, caregiver cohabitation and marital status, total number of waves observed, sampling stratum).

The “correlated subset” used in (4), (5), and (6) includes: child age, child race/ethnicity, caregiver age, caregiver race/ethnicity, caregiver educational attainment, caregiver unemployment, family financial hardship, family low social support, alleged maltreatment type, number of types of maltreatment alleged, baseline maltreatment report substantiation status, prior child welfare services, caregiver history of maltreatment victimization, child health, child and caregiver chronic health condition, child behavioral problem, caregiver physical disability, caregiver mental health condition, caregiver emotional difficulty, caregiver domestic violence victimization, family high stress, caregiver poor parenting, caregiver relationship to child, baseline living arrangement, caregiver cohabitation and marital status, total number of waves observed, sampling stratum.

**Appendix Table 2.3. Marginal Effects of Independent Variables in Probit Model of Foster Care Placement, NSCAW I**

	<b>Marginal Effect</b>	<b>Standard Error</b>
<b>Child Age (Ref: &lt;1 Year)</b>		
1-4 Years	-0.02	0.03
5-9 Years	-0.04	0.04
10+ Years	-0.08*	0.04
<b>Child Race/Ethnicity (Ref: White, Non-Hispanic)</b>		
Black	0.07	0.04
Hispanic	0.08	0.04
American Indian/Native American, Other	0.05	0.05
<b>Caregiver Age (Ref: &lt;25 Years)</b>		
25-35 Years	-0.06*	0.03
35+ Years	-0.03	0.04
<b>Caregiver Race/Ethnicity (1 = Non-White, Non-Hispanic)</b>		
<b>Caregiver Education (1 = Some Postsecondary Education)</b>		
<b>Caregiver Unemployment Status</b>		
<b>Family Financial Hardship</b>		
<b>Family Low Social Support</b>		
<b>Alleged Maltreatment Type(s) (%)</b>		
Sexual Abuse	-0.11***	0.03
Neglect	-0.04	0.02
<b>Number of Types of Maltreatment (1 = 2+ Types)</b>		
<b>Substantiated/Indicated Maltreatment</b>		
<b>Prior CPS Investigation</b>		
<b>Prior CPS Report Substantiation</b>		
<b>Prior Child Welfare Services Receipt</b>		
<b>Own Maltreatment in Childhood</b>		
<b>Child General Health (1-5)</b>		
<b>Child Chronic Health Condition</b>		
<b>Child Behavioral Problem</b>		
<b>Caregiver Cognitive Disability</b>		
<b>Caregiver Physical Disability</b>		
<b>Caregiver Chronic Health Condition</b>		
<b>Caregiver Mental Health Condition</b>		
<b>Caregiver Emotional Difficulty</b>		
<b>Caregiver Domestic Violence Victimization</b>		
<b>Family Stress</b>		
<b>Caregiver Poor Parenting (Caseworker Report)</b>		
<b>Caregiver Type (Biological Parent)</b>		
	-0.55***	0.04

*(Continued on next page)*

**Appendix Table 2.3, continued**

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<b>Living Arrangement</b>		
Single Parent	-0.02	0.04
Extended Family	0.07**	0.03
Non-Family	-0.04	0.03
<b>Caregiver Cohabiting</b>	-0.00	0.04
<b>Caregiver Married</b>	-0.03	0.04
<b>Sampling Stratum</b>		
Florida	-0.01	0.05
Illinois	-0.00	0.05
Ohio	0.07	0.05
Pennsylvania	0.03	0.05
Texas	0.03	0.04
Remainder	0.02	0.03
<b>Total Number of Waves Observed</b>	0.01	0.02
<b>Constant</b>	0.31***	0.01

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Notes: N=1,644. Asterisks note statistical significance at the \*\*\*p<0.001 \*\*p<0.01 \*p<0.05 levels. Estimates are unweighted and based on one imputation of multiply-imputed data (n=5) to allow for reporting of standard deviations, which are reported in parentheses. The final inverse probability weights use the averaged predicted probabilities from probit models estimated on each of the five imputed data sets.

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# CHAPTER III: ESTIMATING CAUSAL EFFECTS OF PATERNAL INCARCERATION ON INFANT BIRTH OUTCOMES IN NEW YORK CITY

## **Abstract**

Research has consistently found paternal incarceration to be negatively associated with an array of outcomes, with consequences for the children of the (formerly) incarcerated that reverberate across the life course. However, the unequal distribution of incarceration across the population and challenges of measurement and identification in existing data on child wellbeing and criminal justice system involvement in the United States make it difficult to determine whether these associations reflect the role of incarceration as an engine of inequality or as an indicator of correlated but independent disadvantages tied to health disparities in early life. In this study, I leverage a novel linked administrative data source on all live births and jail spells between 2010 and 2016 in New York City to explore the effect of paternal incarceration on the likelihood that a birth is preterm, low birthweight, requires admission to a neonatal intensive care unit upon delivery, and results in infant death. Although baseline models confirm prior descriptive studies finding strong ties between paternal incarceration and the adverse birth outcomes I examine, more stringent comparisons between births exposed and not exposed to gestational paternal incarceration yield estimated null or negative effects.

## **Introduction**

The United States population is the most incarcerated in the world, with an incarceration rate of approximately 655 persons per 10,000 and over 2 million in jail or prison at any point in time (Walmsley 2018). These levels of own incarceration in the U.S. are accompanied by high levels of exposure to family incarceration, with recent figures reporting that 45 percent of non-institutionalized adults have ever had an immediate family member incarcerated (Enns et al. 2019). For those concerned with social inequality and the intergenerational transmission of disadvantage, documenting the prevalence, distribution, and consequences of parental incarceration has been especially important (Hagan and Foster 2012; Western and Pettit 2010). Evidence indicates that the experience of having ever had a parent incarcerated in childhood is common (Enns et al. 2019; Muller and Wildeman 2016; Wildeman 2009).

Incarceration and broader involvement with the criminal legal system are risk factors for public health (Dumont et al. 2012; Wildeman, Goldman, and Turney 2018; Wildeman and Wang 2017). There is extensive evidence identifying negative associations of incarceration with the health of those incarcerated as well as those to whom they are tied, including romantic partners, parents, and local community members (e.g. Goldman 2020; Sealey-Jefferson et al. 2020; Wakefield and Wildeman 2014; Wildeman and Muller 2012). The impact of incarceration on the health and wellbeing of children, in particular, has received increased attention in recent years (e.g. Center for Child Well-Being 2020; Christian 2009; Wong 2015), in large part due to the implications of this social process for the perpetuation and exacerbation of social inequality and health disparities (Turney and Goodsell 2018; Wakefield and Wildeman 2009).

From a population health perspective, the prenatal period and infancy may be especially important stages of the life course during which parental incarceration may play a role in shaping

health outcomes and inequality (Bruckner and Catalano 2018; Nobles and Hamoudi 2019; Torche 2011; Waggoner 2017). However, systematic investigation of the effects of parental incarceration on health in early life are challenged by the lack of precise measures of incarceration and health and the nature and timing of paternal incarceration (Geller, Jaeger, and Pace 2016; Wildeman, Goldman, and Turney 2018). It is further limited by its focus on parental imprisonment to the exclusion of parental incarceration in jails, which is substantially more common (Turney and Connor 2019; Wildeman, Turney, and Yi 2016).

In this article, I estimate plausibly causal effects of paternal incarceration on health in early life using research designs from both the associational and quasi-experimental methodological traditions. I analyze a novel database of linked administrative records on all births, deaths, and jail incarcerations in New York City between 2010 and 2016 to estimate the effects of paternal incarceration in gestation on the likelihood that a birth is preterm, low birthweight, requires admission to a neonatal intensive care unit, or results in infant death from non-congenital causes. My findings support prior work establishing negative descriptive associations between paternal incarceration and health in early life. However, in a stricter comparison of births with statistically similar risks of experiencing paternal incarceration, the results confront the possibility of negative causal effects, estimating null or negative effects of gestational paternal incarceration on likelihoods of adverse birth outcomes.

## **Background**

### ***Paternal Incarceration and Health in Early Life***

Studies of the consequences and correlates of paternal incarceration on children's wellbeing find that children who have had a father incarcerated have poorer childhood health,

academic, and socioemotional outcomes and are at higher risk of a wide range of adverse later-life experiences (e.g. Geller et al. 2009; Hagan and Foster 2012; Haskins 2014; Turney and Goodsell 2018; Wakefield and Wildeman 2014; Wildeman 2012, 2014). Much of this work has focused on consequences of paternal incarceration in prisons in childhood and adulthood. However, the relationship between parental incarceration and outcomes specifically in the prenatal and natal periods of life may be especially important to our understanding of the role of mass incarceration and the criminal legal system in shaping population health and health disparities (e.g. McCormick 1985; Wildeman 2012).

There are both theoretical perspectives and empirical evidence that point towards social processes and pathways through which a father's incarceration during a mother's pregnancy might affect infant health, even before a face-to-face paternal relationship has had time to develop. Although the characteristics and experiences of individuals and families exposed to the criminal legal system relative to those who have not may encourage an individualistic account of the relationship between incarceration and health, in considering the spillover consequences of an individual's incarceration, a contextual social determinants framework is appropriate (Barker 1990; Link and Phelan 1995). A father's incarceration during the gestation period may affect infant birth outcomes through financial or economic channels, introducing material hardship and limited access to critical care and resources to support maternal health and a healthy pregnancy (e.g. Dumont et al. 2014). Paternal incarceration may also subject a birth mother to stress, stigma, uncertainty, and diminished social support (Comfort 2016; Foster and Hagan 2015; Wildeman, Turney, and Yi 2016), all of which might also ultimately affect maternal and infant health (Pearlin 1989; Thoits 2010; Torche 2011).

However, these pathways are difficult to identify empirically, as children at risk of experiencing paternal incarceration are also likely to be disadvantaged in many other ways. It is plausible that the shock of having a father incarcerated could either compound pre-existing hardships (e.g. Arditti, Lambert-Shute, and Joest 2003; Dwyer Emory 2018) or, for some, ameliorate hardships through the removal of an abusive parent or source of socioemotional or economic stress for a child, their caregiver, or others in their close social network (Hagan and Dinovitzer 1999; Turney and Goodsell 2018; Wildeman 2012). The common co-occurrence of these risk factors and disadvantages makes it difficult to assess the degree to which the paternal incarceration itself is the driver of any disparities and negative associations rather than the conditions, behaviors, and experiences that place a family at higher risk of experiencing paternal incarceration in the first place (e.g. Turney and Wildeman 2015; Wildeman and Turney 2014).

### ***Estimating Effects of Paternal Incarceration***

Some studies have explored potential effects of paternal incarceration on child wellbeing using a range of quasi-experimental research designs and attention to the systematic differences between children and families experiencing paternal incarceration and those who do not (e.g. Haskins 2014; Wildeman, Wakefield, and Turney 2013). However, the extant literature is limited by several important data and analytic challenges that make it difficult to approximate, much less estimate, these effects. First, the data leveraged for this research largely rely on self-reported and/or retrospective measures of children's health and wellbeing as well as correlated or predictive parental health and health behaviors, which may be subject to desirability bias and/or recall error (Baker, Stabile, and Deri 2004; Johnston, Propper, and Shields 2009).

Second, even some of the strongest studies of the consequences of parental incarceration are limited due to constraints in their ability to speak to variation in the nature of parental

incarceration. To date, they have often been unable to separate out the incarceration of different parent types (e.g. biological maternal or paternal; e.g. Roettger and Boardman 2011, Wildeman 2012) and incarceration in different correctional facilities (i.e. jail vs. prison; Comfort 2016; Hagan and Foster 2012; Turney and Connor 2019; Wildeman, Fitzpatrick, and Goldman 2018; Yi, Turney, and Wildeman 2017). Prior work is also often unable to speak to the precise timing, frequency, or duration of parental incarceration in relation to that child's own life events and experiences (see, for example, the National Survey of Adolescent to Adult Health; Harris et al. 2009 and Roettger and Boardman 2012).<sup>1</sup>

Finally, much of this work, though focusing on a relatively disadvantaged and hard-to-reach population, has relied on survey data, which has known challenges with under-representation and systematic exclusion of institutionalized persons and their families and local communities (Pettit 2012). This aspect of studying marginalized experiences raises concerns about statistical power and attrition and how those challenges may affect researchers' ability to correctly estimate exposure to paternal incarceration or observe these children and their families in secondary data at all (Geller, Jaeger, and Pace 2016; Pettit 2012; Sabol 2016). Existing research on the consequences of parental incarceration that draws upon administrative records, which are less likely to be affected by attrition, are concentrated on the Danish and Swedish policy and social contexts, making implications for the differently and exceptionally highly incarcerated U.S. population less clear (e.g. Wildeman and Andersen 2016; Dobbie et al. 2018).

This study attempts to address these challenges directly by analyzing linked administrative records of all jail incarceration spells, births, and deaths in New York City between 2010 and 2016, a data source that is substantially less likely to be impacted by the

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<sup>1</sup> For exceptions, see, for example, Andersen 2016 and Haskins 2014.

statistical power and measurement error challenges described above. These data also allow me to precisely identify the timing of exposure to paternal incarceration relative to an individual's (infant's) life course, more accurately and objectively identify incarceration and infant health, and draw upon a population-level data set that allows me to use multiple research designs capable of accounting for an array of observed child and parent characteristics (Geller, Jaeger, and Pace 2016; Sabol 2016)

## **Data, Measures, and Methods**

### ***2010-2016 New York City Birth, Death, and Jail Records***

This study employs a novel data set of person-spell-level jail records from the New York City Department of Correction (DOC) and person-level birth and death records from the New York City Office of Vital Statistics (OVS). Jail records document all jail intakes that took place between January 1, 2010 and December 31, 2016 and include basic information about the individual incarcerated, admission and discharge dates, and some information about the nature of the jail spell (e.g. charge and sentence codes, discharge type if transferred or released from jail). Birth records include basic birth certificate information such as parental identifiers and the infant's date of birth, but also include a comprehensive array of measures of maternal health during and before pregnancy and the condition of the infant at and immediately following delivery. Death records are more parsimonious and include basic information about the identity of the deceased and the location (borough) and cause of death.

These sets of records were linked to create a child-level data set that includes all live births in the city of New York between January 1, 2010 and December 31, 2016. To create this data set, parents listed on the birth records were linked to adult jail incarceration spell records

using a deterministic matching procedure that leverages identifying information about parents and persons incarcerated in jails available in the OVS and DOC records, respectively. The linkage was conducted using the following identifiers: adults' first name, last name, sex, date of birth, the last four digits of their Social Security Number and, in cases when the information was available, middle name, last name before marriage, and address.<sup>2</sup> Retaining only the strongest matches, of the 855,632 births between 2010 and 2016, three percent (29,051 births) linked to at least one adult, or parental, jail admission between January 1, 2010 and December 31, 2016. Infant death information was then appended using death records previously linked to birth records by OVS.

As discussed above, my ability to estimate plausibly causal estimates of paternal incarceration on children's wellbeing require: (1) precise and accurate identification of the timing of exposure to paternal incarceration, (2) reliability of health and wellbeing measures, and (3) ability to account for differences between parents or soon-to-be parents who are or will be incarcerated. To produce plausibly causal estimates as well as associational estimates to place this study in dialogue with prior research, I restrict the population of births to define the analytic sample in four ways. First, I restrict the analysis to singleton births, as multiple births (e.g. twins, triplets) are strongly associated with the outcomes of interest, and, with the spread of assistive reproductive technologies, increasingly linked to socioeconomic advantage and likely other differences from births exposed to paternal incarceration in gestation unobserved in these data (Adamson and Baker 2004; Goldenberg et al. 2008). This results in the exclusion of 32,547

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<sup>1</sup> A paper introducing the match procedure was presented by Joseph Kennedy, a collaborator on the broader research effort using linked administrative records from NYC and lead in the Data Match Unit in OVS of NYC DOHMH, at the 2018 meetings of the American Public Health Association (Kennedy et al. 2018).

births. Second, I use listwise deletion to exclude birth records that are missing information on the outcome measures of interest (birth weight/height and gestational age for identification of preterm and low birthweight births; NICU admission; infant death; N=7), or any of the covariates and controls included in the regression models described below (N=2,304).

Third, the analysis is constrained in two important ways. For the “associational” analysis, the sample is restricted to births for which I can observe the full gestational period (9-month window prior to date of birth) in the 2010-2016 data, excluding births before October 2010. For the “lagged” analysis, I apply an additional constraint related to timing. I only include those births for whom I am also able to observe the full 9-month period after the birth to allow for a strict comparison group: children whose fathers were incarcerated in close temporal proximity to the time of birth but following the birth instead of during the gestational period.

Finally, to more precisely isolate associations between paternal incarceration and infant birth outcomes, I restrict the analysis further in ways related to exposure to maternal incarceration. For the associational sample, I only include infants who meet the above criteria and whose birth mothers were not incarcerated while pregnant. For the lagged sample, I apply this additional criterion and restrict the sample even further, only retaining births not exposed to any maternal incarceration in the 18 months spanning the prenatal, birth, or post-natal period. Our final associational and lagged analytic samples thus include 632,315 and 8,145 births, respectively, or 74 percent and one percent of the full population of births that took place between 2010 and 2016 in New York City. A statistical description of these births is presented in Table 3.1, with comparison of the full sample of 2010-2016 births and associational and lagged samples presented in Appendix Tables 3.1 and 3.2.

**Table 3.1. Associational and Lagged Analytic Sample Characteristics**

	Associational Sample			Lagged Sample		
	Overall	Paternal Incarceration		Overall	Paternal Incarceration	
		Not in Gestation	In Gestation		Postpartum	In Gestation
<b>Parental Characteristics at Birth</b>						
Father's Age (Years)	33.06*** (7.24)	33.10 (7.23)	28.14 (7.30)	28.16 (7.39)	28.00 (7.47)	28.39 (7.26)
Father's Race/Ethnicity						
White	0.36***	0.36	0.04	0.04	0.04	0.04
Black	0.18***	0.18	0.56	0.55	0.56	0.54
Hispanic	0.28***	0.28	0.37	0.38	0.37	0.39
Asian/Pacific Islander	0.17***	0.17	0.02	0.02	0.02	0.02
Other	0.01***	0.01	0.01	0.01**	0.01	0.01
Father's Education						
<High School Degree	0.21***	0.21	0.41	0.41*	0.42	0.39
High School Degree/GED	0.24***	0.23	0.43	0.42	0.42	0.42
Postsecondary Education	0.56***	0.56	0.17	0.17*	0.16	0.18
Mother's Age (Years)	30.10*** (5.89)	30.14 (5.88)	25.48 (5.93)	25.44** (5.92)	25.26 (5.91)	25.70 (5.92)
Mother's Race/Ethnicity						
White	0.35***	0.36	0.06	0.06	0.06	0.06
Black	0.17***	0.16	0.49	0.49	0.49	0.48
Hispanic	0.29***	0.29	0.42	0.42	0.41	0.43
Asian/Pacific Islander	0.19***	0.19	0.02	0.02	0.02	0.02
Other	0.06*	0.01	0.01	0.01	0.01	0.01
Mother's Education						
<High School Degree	0.18***	0.18	0.36	0.37	0.37	0.35
High School Degree/GED	0.21***	0.21	0.30	0.30	0.30	0.29
Postsecondary Education	0.61***	0.61	0.34	0.34**	0.33	0.35
Parents Married at Birth	0.67***	0.68	0.16	0.15***	0.13	0.18

*(Continued on next page)*

**Table 3.1, continued.**

<b>Maternal Health</b>						
Maternal Body Mass Index	24.82*** (5.47)	24.80 (5.46)	26.70 (6.45)	26.68 (6.42)	26.74 (6.45)	26.60 (6.38)
Cigarette Use during Pregnancy	0.01***	0.01	0.04	0.04	0.04	0.04
Alcohol Use during Pregnancy	0.01	0.01	0.01	0.01	0.01	0.01
Trimester of 1 <sup>st</sup> Prenatal Care						
1 <sup>st</sup> Trimester	0.75***	0.75	0.62	0.62	0.61	0.63
2 <sup>nd</sup> Trimester	0.19***	0.19	0.29	0.29	0.29	0.28
3 <sup>rd</sup> Trimester or None	0.06***	0.06	0.09	0.10	0.10	0.09
Health Insurance: Medicaid	0.55***	0.55	0.84	0.84**	0.86	0.83
Prior Low Birthweight Birth	0.09**	0.09	0.10	0.10	0.10	0.10
Prior Preterm Birth	0.09***	0.09	0.10	0.10	0.10	0.10
<i>N</i>	632,315	627,220	5,095	8,162	4,830	3,332

Notes: Asterisks indicate differences between births exposed and not exposed to gestational paternal incarceration that are statistically distinguishable at the \*\*\* $p < 0.001$ , \*\* $p < 0.01$ , and \* $p < 0.05$  levels. Standard deviations are reported in parentheses.

### ***Paternal Incarceration***

Exposure to gestational or postnatal paternal incarceration is identified by temporally situating the jail spells of birth fathers who match to jail inmates admitted between January 2010 and December 2016 relative to birth dates of the linked births. If a linked birth father had a jail spell for which the admission and discharge dates span the precise nine-month period prior to the date of birth, the birth is identified as having been exposed to gestational paternal incarceration. If a linked birth father had a jail spell that spanned the precise nine-month period immediate following the date of birth, the birth is identified as having been exposed to postnatal paternal incarceration. Information about postnatal paternal incarceration is relevant for the construction of the lagged analytic sample and comparisons leveraged in Stage B of the analytic strategy described below.

As there may be heterogeneity in the effect of paternal incarceration on a mother, pregnancy, and infant with respect to the timing, duration, and other details of the incarceration, the analysis also explores categorical measures of paternal incarceration that disaggregates paternal incarceration further. Paternal incarceration is described and categorized in the analysis with respect to its timing (1<sup>st</sup>, 2<sup>nd</sup>, or 3<sup>rd</sup> trimester of the pregnancy), longest single incarceration spell (1 day or fewer, 2-7 days, 7-30 days, 30 days or more), total duration of incarceration (7 days or fewer, more than 7 days), the criminal processing stage of incarceration (pre-trial detention vs. post-trial/sentencing), and facility type(s) in which the father was incarcerated during the time period of interest (jail vs. jail and transfer to prison).

### ***Birth Outcomes***

This study focuses on a key subset of infant birth outcomes available in the birth records that are well-documented predictors of wellbeing (e.g. Behrman and Butler 2007; Saigal and

Doyle 2008), and, at the population level, health disparities and social inequality (e.g. Currie 2009; Palloni 2006). The analysis focuses on the likelihood of a birth being preterm (younger than 37 weeks in gestational age upon delivery), being low birthweight (less than 2,500 grams in weight upon delivery), resulting in admission to a neonatal intensive care unit (NICU) following delivery, and infant death from non-congenital cause (death at less than 1 year of age). Death data are not yet available for 2016; as such, analyses of infant death are restricted to births that took place before January 2016, which includes 531,094 births in the associational sample and 7,935 births in the lagged sample.

### ***Covariates***

I account for an array of sociodemographic and health characteristics that are associated with risks of paternal incarceration as well as with variation in infant pregnancy outcomes. The analyses incorporate categorical measures of paternal and maternal race/ethnicity (White, Black, Hispanic, Asian/Pacific Islander, and Other race/ethnicity), continuous measures of paternal and maternal age at the time of birth in years, and categorical measures of paternal and maternal educational attainment (less than high school; a high school degree or equivalent; and some postsecondary education). The covariates also include an indicator of parental marital status, identifying whether the birth mother was married at the time of birth. The analyses also account for maternal health and health behaviors known to be predictors of birth outcomes and infant health: continuous body mass index (BMI); indicators of whether the birth mother used cigarettes or alcohol during the pregnancy;<sup>1</sup> and indicators of whether the birth mother had any previous low birthweight or preterm births.

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<sup>1</sup> Although the OVS records also include an indicator of maternal drug use, I was advised not to use that measure due to concerns about inconsistent data collection of that particular item across birth facilities (Office of Vital Statistics 2018).

The next set of controls captures aspects of mothers' access to health care in gestation: the trimester of the birth mother's first prenatal care visit for this birth (1<sup>st</sup> trimester, 2<sup>nd</sup> trimester, and 3<sup>rd</sup> trimester or no prenatal care) and whether the birth was insured by Medicaid.<sup>2</sup> The indicator of trimester of first prenatal care is important not only as an indicator of health care access but also to account for the fact that initiation of prenatal care is a predictor of the accuracy of measurement of gestational age, with evidence suggesting that later initiation of care is associated with discrepancies in calculation of gestational age (Qin, Hsia, and Berg 2007). The measure of health insurance coverage serves not only as an indicator of access to quality health care, but also as a proxy for and known correlate of economic security in the absence of additional standard measures of socioeconomic background—such as income or poverty status (Becker and Newsom 2003; Gazmararian, Adams, and Pamuk 1996; Marcin et al. 2003).

### *Analytical Strategy*

The analysis uses a three-stage approach to explore potential effects of paternal incarceration on infant birth outcomes. In Stage A, I use the associational analytic sample to estimate a series of four nested logistic regression models of the likelihood that a live birth is preterm, low birthweight, requires NICU admission, or results in infant death, as function of an increasingly comprehensive set of key characteristics. The models account for measures of paternal characteristics (Model A1), adding measures of maternal characteristics (Model A2), maternal health and health behaviors (Model A3), and health care access (Model A4) in subsequent models. These measures are factors known to be independent correlates of adverse

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<sup>2</sup> This group also includes those who were enrolled in Family/Child Health Plus, a public New York State health insurance option for those whose incomes were too high to qualify for Medicaid. As of January 1, 2014, Family Health Plus was folded into the state Medicaid expansion as part of the Affordable Care Act and both Family and Child Health Plus became insurance options through the state ACA Marketplace.

birth outcomes (e.g. maternal smoking; Abraham et al. 2017; Anderson et al. 2019) and/or higher risks of paternal incarceration (e.g. educational attainment, race/ethnicity; Pettit and Western 2004; Wildeman 2009). The results of the Stage A models establish a baseline descriptive examination of the focal relationships in these analyses. The models describe the relationship between gestational paternal incarceration  $\beta_{incarc}$  and the probability,  $p$ , of a birth being preterm, low birthweight, requiring NICU admission, or resulting in infant death using the following mathematical expression, adjusting for  $n$  covariates in Model A4:

**Equation 3.1.**

$$\ln\left(\frac{p}{1-p}\right) = \beta_0 + \beta_{incarc}X_{incarc} + \beta_1X_1 + \beta_2X_2 + \dots + \beta_nX_n$$

In Stage B, I leverage the more conservative analytic sample for a stricter comparison. Stage B consists of four models leveraging this lagged analytic sample, the construction of which is described above. First, I estimate two regression models that use the same specification as in Stage A and Equation 3.1, but leverage variation in the timing of paternal incarceration to construct a stronger comparison, one unadjusted model (Model B1) and one fully-adjusted model that includes the full set of covariates incorporated in Model A4 in Stage A of the analysis (Model B2). These two models compare the birth outcomes of births with paternal incarceration in gestation to those with paternal incarceration in the nine-month postpartum period. For the latter group, paternal incarceration exposure is lagged, such that it would be temporally impossible for a reverse causal process to link the paternal incarceration to health outcomes at the point of delivery (Bellemare et al. 2015; Elwert and Pfeffer 2019).

The next two models go beyond accounting for characteristics and experiences observed in the data as in Stage A and Models B1 and B2 by modeling the risk of exposure to gestational paternal incarceration and using predicted risks of gestational paternal incarceration to estimate a

propensity score-matched model (Model B3) and an inverse probability-weighted model (Model B4). The propensity score-matched model estimates each birth's probability of gestational paternal incarceration exposure as a function of  $X$ , the comprehensive set of pre-delivery conditions and characteristics plausibly associated with gestational paternal incarceration included in Models A4 and B2 (Morgan and Harding 2006; Robins 1986; Thoemmes and Ong 2016). The probit expression of each birth's exposure to gestational paternal incarceration  $T_i$ , then, is:

**Equation 3.2.**

$$probit(T = 1 | X) = \phi(\gamma_0 + \gamma_1 X + \varepsilon)$$

For Model B3, the propensity score-matched model, an effect is then estimated by comparing birth outcomes within matched pairs of births that are comprised of one birth with gestational paternal incarceration matched to a birth without gestational paternal incarceration that has the same or similar predicted probability of paternal incarceration in gestation based on Equation 3.1 (Rosenbaum and Rubin 1983; Winship and Morgan 1999). The estimated effect is the adjusted difference in likelihoods of each of the four adverse birth outcomes examined. To avoid inflating the success of our matches of “treatment” and “control” births in the data simply via the volume of the sample, I use a one-to-one matching strategy (Model B3).

Model B4 again uses the estimated predicted probabilities of gestational paternal incarceration. However, rather than matching exposed and non-exposed births, this inverse probability-weighted model uses these predicted probabilities to reweight the comparison. The estimated predicted probabilities are converted into stabilized inverse probability weights (IPWs). For each individual  $i$ , where  $P(T = 1)$  is the proportion of births exposed to paternal incarceration in gestation and  $T_i$  is the estimated predicted probability of gestational paternal

incarceration from Equation 1 (Morgan and Harding 2006; Robins 1986; Wooldridge 2010), the stabilized inverse probability weight  $w_i$  is:

**Equation 3.3.**

$$w_i = \frac{P(T = 1)}{P(T_i = 1 | X)} + \frac{1 - P(T = 1)}{1 - (P(T_i = 1 | X))}$$

I then apply the IPWs to the model specified in Equation 3.1 using the lagged analytic sample (Model B4). The IPWs balance the comparison groups of births by more heavily weighting births to fathers estimated to be less likely to be incarcerated in gestation ( $T_i$  closer to 0) and down-weighting births to fathers estimated to be more likely to be incarcerated during the gestation period ( $T_i$  closer to 1). Both Models B3 and B4 are multivariate models that adjust fully for all covariates in Model A4.

Finally, to the degree possible, I explore potential heterogeneity in the impact of that exposure on infant birth outcomes. To do so, I again rely on the lagged analytic sample and estimate models that adjust fully for all covariates in Model A4 but also include interaction terms of gestational paternal incarceration with measures of the trimester of incarceration, duration of the longest single incarceration spell, cumulative duration of incarceration spells overlapping with the gestation period, whether the gestational paternal incarceration involved pretrial detention (as opposed to tried and awaiting sentencing or sentenced), and whether any gestational paternal jail incarceration ended with a discharge to prison incarceration. To allow for comparison across models, I report all results as marginal effects, or differences in predicted probabilities of each of the adverse outcomes examined, estimated with covariates set at observed values (Allison 1999; Long and Mustillo 2017; Mood 2010).

## Results

The summary statistics presented in Table 3.1 provide a detailed sociodemographic description of the population of New York City births that form the basis of my analysis. As shown, the associational and lagged samples differ substantially with respect to every observed characteristic except for the probability that the birth was to a mother who consumed alcohol during their pregnancy (1%, Table 3.1). In the associational sample, births exposed to gestational paternal incarceration were to younger parents (33 v. 28 years old for fathers; 30 vs. 25 years old for mothers) and less likely to have completed a high school or equivalent level of education (21% vs. 41% for fathers; 18% vs. 36% for mothers). Births exposed to gestational paternal incarceration were more likely to have mothers who used cigarettes while pregnant (1% vs. 4%), as well. The two groups of births also had different levels of health care access. Mothers of non-exposed births were much more likely to have initiated prenatal care visits in the first trimester of pregnancy (75% vs. 62%) and substantially less likely to have had health coverage for the birth through Medicaid as opposed to another source (e.g. employer-sponsored; 55% vs. 84%).

The descriptive statistics in Table 3.2 provide additional detail about the timing, frequency, duration, and conditions of paternal incarceration in gestation and, for the lagged analytic sample, during the nine-month postpartum period, for comparison. The “exposed” group in both samples is more likely to have experienced gestational paternal jail incarceration in the later trimesters than in the first trimester. For “exposed,” or treatment, groups in both the associational and lagged samples, births were exposed to an average of just over one jail incarceration spell during the gestational period (1.25 and 1.18 spells, respectively), with the longest spell overlapping with the pregnancy lasting 24 days, on average (Table 3.2). Approximately 40 percent of births exposed to gestational paternal incarceration had fathers who

were held in pretrial detention during gestation, and just over one in ten had fathers who were transferred to prison following a jail incarceration spell that coincided with the pregnancy (11% and 12% in the associational and lagged samples, respectively; Table 3.2).

**Table 3.2. Details of Paternal Incarceration Exposure, by Gestational Paternal Incarceration Exposure**

	Associational Sample		Lagged Sample		
	Overall	Paternal Incarceration	Overall	Paternal Incarceration	
		In Gestation		Postpartum	In Gestation
<b>Incarceration in Gestation</b>					
Observed	0.01	1.00	0.41	0.00	1.00
Predicted	-	-	0.41	0.40	0.41
			(0.49)	(0.04)	(0.05)
Stabilized Inverse Probability Weight	-	-	1.00	1.00	1.00
			(0.09)	(0.08)	(0.11)
<b>Incarceration in Trimester</b>					
Trimester 1	0.00	0.37	0.15	0.00	0.36
Trimester 2	0.00	0.47	0.18	0.00	0.45
Trimester 3	0.00	0.49	0.19	0.00	0.48
Full Gestational Period	0.00	0.05	0.02	0.00	0.04
<b>Number of Spells</b>					
	0.01	1.23	1.22***	1.25	1.18
	(0.12)	(0.56)	(0.55)	(0.60)	(0.46)
<b>Longest Single Spell (Days)</b>					
	0.19	23.88	29.66***	33.80	23.66
	(4.38)	(42.62)	(48.96)	(51.84)	(43.77)
<b>Cumulative Duration (Days)</b>					
	0.23	28.12	34.24***	39.41	26.73
	(5.10)	(49.45)	(56.14)	(60.00)	(49.07)
<b>Pretrial Detention</b>					
	0.00	0.40	0.41	0.41	0.41
<b>Discharge/Transfer to Prison</b>					
	0.00	0.11	0.16***	0.19	0.12
<i>N</i>	632,315	5,095	8,162	4,830	3,332

Notes: Asterisks indicate differences between births exposed and not exposed to gestational paternal incarceration that are statistically distinguishable at the \*\*\*p<0.001, \*\*p<0.01, and \*p<0.05 levels. Standard deviations are reported in parentheses

### *Associations between Paternal Incarceration and Infant Pregnancy Outcomes*

The analysis begins with an estimation of the descriptive associations between paternal incarceration in gestation or at birth and the likelihoods of a birth being preterm, low birthweight, requiring NICU admission upon delivery, or resulting in infant death. These estimates draw upon the associational analytic sample, a bivariate logistic regression model, and a multivariate logistic regression model that adjusts for maternal and paternal sociodemographic characteristics, maternal health and health behaviors, and health care access. The results of these models are presented in the first two columns of Table 3.3.<sup>1</sup>

The bivariate model estimated using the associational analytic sample suggests strong associations between gestational paternal incarceration and the likelihood that a birth is preterm, low birthweight, requires NICU admission, or ends in infant death (Table 3.3, column 1). These estimates are also substantial in magnitude and statistically distinguishable from zero at a high confidence level ( $p < 0.001$ ). Births to fathers who were incarcerated at some point during the gestational period are four percentage points more likely to be born preterm, low birthweight, or requiring NICU admission. These translate to effect sizes of 67 percent, 81 percent, and 51 percent relative to unadjusted baseline proportions of births with each one of these conditions.

The fully adjusted models estimated using the associational sample, presented in Table 3.3 (column 2), suggest that paternal incarceration in gestation is still consequential for explaining elevated likelihoods of low birthweight and preterm birth, and more modestly, the likelihood that a birth requires admission to a NICU upon delivery. Births to fathers who were incarcerated at some point during gestation are approximately two percentage points more likely

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<sup>1</sup> The results of the full set of nested bivariate and multivariate regression models estimated using the associational sample are presented in Appendix 3, Table 2.

to be preterm or low birthweight relative to births to fathers who were not incarcerated during the gestational period. These marginal effects translate to effect sizes of 29 percent and 34 percent greater probabilities of preterm and low birthweight birth, respectively (Table 3.3, column 1). The estimated association for NICU admission is substantially more modest; exposed births are estimated to be 0.9 percentage points more likely to require NICU admission, an effect size of approximately 10 percent. In the fully adjusted model, the association between paternal incarceration in gestation and likelihood of infant death is statistically distinguishable from zero.

These descriptive estimates indicate that although parental characteristics, maternal health and health behaviors, and measures of health care access explain part of the relationship between paternal incarceration in gestation and key indicators of infant health, some non-negligible differences in the birth outcomes of exposed and non-exposed births remain. However, given the substantial and multidimensional differences in the *observed* characteristics of these groups of birth (Table 3.1), it is possible that there are other factors driving these health disparities, either by placing certain fathers at higher risk of incarceration or by affecting birth outcomes through pathways that do not involve paternal incarceration. The remaining analyses use additional strategies and a more conservative analytic sample to answer this question.

**Table 3.3. Estimated Associations between Gestational Paternal Incarceration and Infant Birth Outcomes**

	Associational Sample		Lagged Sample		
	(1) Bivariate Regression	(2) Multivariate Regression	(3) Multivariate Regression	(4) Propensity Matched Regression (ATET)	(5) Inverse Probability Weighted Regression
<b>Preterm Birth</b>					
Proportion (Unadjusted)	0.066	0.066	0.112	0.112	0.112
Marginal Effect	0.044*** [0.035, 0.052]	0.019*** [0.012, 0.026]	-0.014* [-0.028, 0.000]	-0.012 [-0.031, 0.007]	-0.014 [-0.028, 0.000]
<b>Low Birthweight</b>					
Proportion (Unadjusted)	0.059	0.059	0.106	0.106	0.106
Marginal Effect	0.048*** [0.040, 0.057]	0.020*** [0.013, 0.026]	-0.012 [-0.025, 0.002]	-0.005 [-0.023, 0.014]	-0.012 [-0.026, 0.002]
<b>NICU Admission</b>					
Proportion (Unadjusted)	0.087	0.087	0.132	0.132	0.132
Marginal Effect	0.044*** [0.035, 0.053]	0.009** [0.002, 0.016]	-0.019* [-0.033, -0.004]	-0.014 [-0.034, 0.005]	-0.019* [-0.034, -0.004]
<b>Infant Death</b>					
Proportion (Unadjusted)	0.002	0.002	0.006	0.006	0.006
Marginal Effect	0.003*** [0.001, 0.005]	0.001 [-0.001, 0.002]	0.001 [-0.003, 0.004]	0.002 [-0.003, 0.006]	0.001 [-0.003, 0.004]
<i>N</i>	632,315	632,315	8,162	8,158	8,162

Notes: Results are reported as marginal effects to allow for comparison across models, estimated with covariates set at observed values. Asterisks denote statistically distinguishable differences between exposed and non-exposed births at the \*\*\*p<0.001 \*\*p<0.01 \*p<0.05 levels. 95% confidence intervals are reported in brackets. All models presented here adjust for paternal characteristics, maternal characteristics, maternal health/health behaviors, and health care access. ATET indicates that the estimator presented for reporting of results from the propensity-matched model are estimated average treatment effects on the treated. Four observations were dropped from estimation of Model 4 due to violation of the overlap assumption.

### *Towards an Estimated Effect of Paternal Incarceration on Infant Birth Outcomes*

To move towards a comparison of births and families that are likely to be more similar, both on observed and unobserved characteristics, I first re-estimate the associational models using the “lagged sample,” described above. The lagged sample allows for a more restrictive test by comparing only births that have been exposed to paternal incarceration within a nine-month proximity to delivery, either in gestation or the nine-month postpartum period. This lagged analytic sample is notably different from the associational sample, as shown in Table 3.1. In many ways, these differences parallel those observed between exposed and non-exposed births in the associational sample. Compared to births in the associational sample, births in the lagged sample are substantially more likely to be to Black (55% vs. 18%) or Hispanic fathers (38% vs. 28%), less likely to have been born to parents who had attained high school-level educations (41% vs. 21%) or were married at the time of birth (15% vs. 67%), and more likely to have been born to mothers who used cigarettes while pregnant (4% vs. 1%).

In comparing birth groups in the lagged sample differentiated only by the timing of paternal incarceration (gestation vs. postpartum), however, the comparison groups in the lagged analytic sample are statistically indistinguishable on most observed characteristics. They do differ statistically and somewhat substantially in the proportion of births to parents who were married at birth (18% vs. 13%), however. The two groups also differ statistically in the proportions of births to fathers who identify as being of another race/ethnicity, parental educational attainment, maternal age at birth, and their source of health insurance coverage, although these differences are much more modest in magnitude, ranging in size from less than one to three percentage points (Table 3.1). This indicates that estimated differences in birth outcomes between the two groups in the lagged sample are less likely than those in a broader

comparison to be driven by observed differences in characteristics and experiences predictive of ever having a father incarcerated. Differences in health outcomes among these births, then, are more likely to be directly linked to their exposure to gestational paternal incarceration.

This discussion focuses on results from the fully adjusted model of the lagged sample comprised exclusively of births exposed to paternal incarceration in gestation or in the postpartum period. The results of these models are reported in the third column of Table 3.3.<sup>1</sup> Perhaps surprisingly, these models estimate null and negative effects of gestational paternal incarceration on adverse infant birth outcomes. The estimated effects of gestational paternal incarceration on likelihoods of a birth being low birthweight or resulting in infant death are statistically indistinguishable from zero and negative in direction. For likelihoods of preterm birth and NICU admission, the models yield statistically distinguishable negative effects of paternal incarceration in gestation. Births to fathers incarcerated at some point during their mothers' pregnancy are estimated to be 1.4 and 1.9 percentage points less likely to be born preterm or to require NICU admission following delivery, respectively.

I next leverage alternative model specifications from the toolkit of propensity-based causal inferential methods to see if these results hold. I estimate (1) propensity score-matched models and (2) inverse probability-weighted models. In both sets of models, I also include the full vector of covariates used in the multivariate regression models presented in columns 2 and 3 of Table 3.3, which serve to tighten the comparisons between births above and beyond what propensity score-matching and inverse probability weighting may be able to accomplish. This combination of propensity-based strategies with covariate adjustment is sometimes described as

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<sup>1</sup> The results of the full set of nested bivariate and multivariate regression models estimated using the lagged sample are presented in Appendix Table 3.3.

the specification of “robust” models (e.g. Thoemmes and Ong 2016). The results of these models are presented in Table 3.3, as well.

The propensity score-matched model estimates the effects of paternal incarceration in gestation on likelihoods of adverse birth outcomes by comparing exposed and non-exposed births matched on the previously predicted probabilities of paternal incarceration in gestation (e.g. Angrist and Pischke 2008; Morgan and Harding 2006). As noted in Table 3.2, the estimated propensity scores of the two comparison groups are statistically indistinguishable, suggesting that there is sufficient common support for estimation.<sup>2</sup> The propensity-matched regression model finds no statistically distinguishable effect of paternal incarceration in gestation on the likelihood that a birth is preterm, low birthweight, requires NICU admission, or results in infant death from non-congenital cause. These results also align with the results of the previous model discussed (multivariate regression) in that the estimated coefficients are all negative, or close to zero, as is the case for the estimated effect of gestational paternal incarceration on infant death.

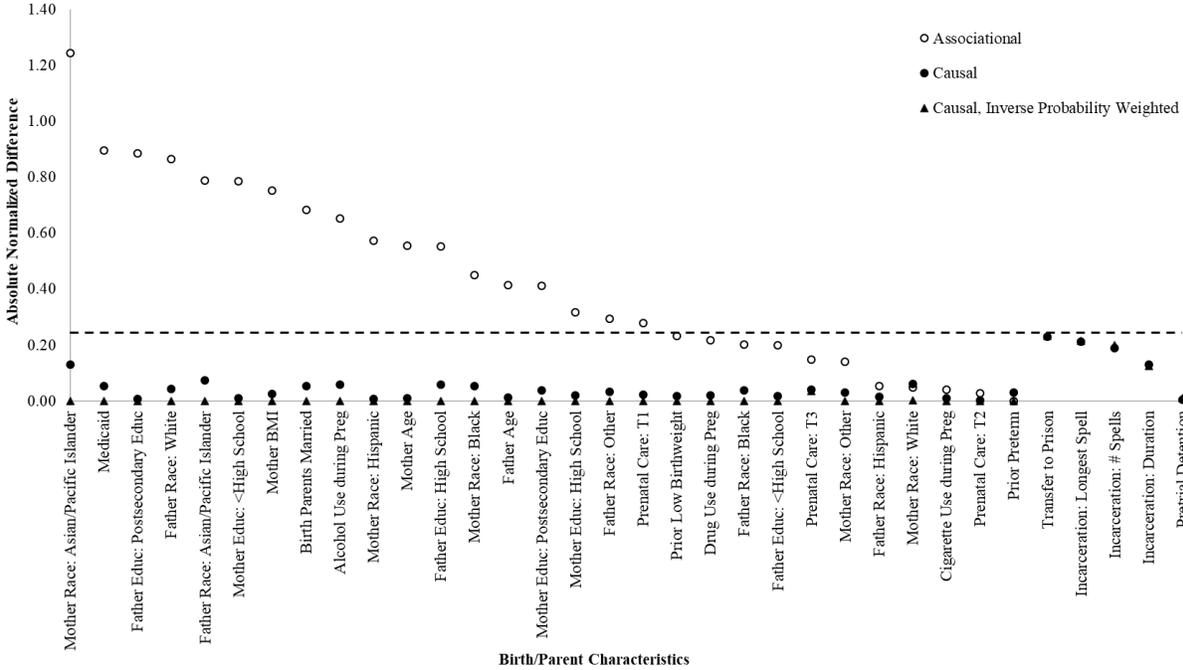
Finally, rather than matching directly on the estimated propensities as in the last model, the next model weights the births in the analysis inversely to their predicted probability of exposure (Model B3). Figure 3.1 compares unweighted and stabilized inverse probability-weighted absolute normalized differences between paternal incarceration-exposed and non-exposed births in the lagged sample, illustrating the efficacy of the IPWs in diminishing already-minimal imbalances on observed characteristics across the two groups of births. As shown in Table 3.3, again, a more stringent comparison and specification of the effect of gestational paternal incarceration furthers what I observe in Models B1 and B2, essentially duplicating the

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<sup>2</sup> Four observations were trimmed, or dropped, from the propensity-matched regression analysis due to violation of the overlap assumption, meaning that they had propensity scores that were too extreme for comparison to a counterfactual birth.

associations and effects estimated in the multivariate regression model without propensity matching or inverse probability weighting.

**Figure 3.1. Normalized Differences, Comparison of Analytic Samples**



Notes: N=632,315 (associational sample), N=8,162 (lagged sample). Data points note the absolute values of normalized differences in characteristics between exposed and non-exposed groups (differentiated by exposure to gestational paternal incarceration) for the two different sample specifications. The dotted line at 0.25 indicates the threshold below which absolute normalized differences indicate balance between the two groups on that characteristic (Wooldridge 2010).

**Exploring Potential Heterogeneity**

Up until this point, the evidence points towards potentially null or even protective effects of paternal incarceration in gestation on infant birth outcomes in New York City. It is possible, however, that these analyses of all births are masking underlying heterogeneity, either with respect to births’ sociodemographic characteristics or with respect to differences in the nature of the birth fathers’ incarceration. Prior work finds differential risks and outcomes of criminal legal system involvement, particularly across racial/ethnic groups (e.g. Pager 2003) and outlines the potential importance of the nature and characteristics of incarceration in shaping its correlates

and consequences (e.g. Turney and Conner 2019; Wildeman, Turney, and Yi 2016). Therefore, I re-estimate the fully adjusted multivariate regression model (Table 3.3, column 3) in two ways.

First, I explore possible racial/ethnic heterogeneity by estimating fully interacted or stratified models by broad racial/ethnic group. These results are presented in Table 4 alongside the pooled results, for reference. The stratified models again point towards negative associations between paternal incarceration in gestation and the likelihood of the four adverse birth outcomes examined. Nearly all estimated effects are statistically indistinguishable from zero, although, the relatively large estimated 95% confidence intervals, relative to the baseline proportions and estimated effects, suggest that these are not precisely estimated null effects. Among births to non-Hispanic White fathers, however, the estimated negative effect of gestational paternal incarceration on likelihood of NICU admission following delivery is notable: the racially stratified model estimates that gestational paternal incarceration lowers the likelihood of this adverse birth outcome by 2.6 percentage points, an estimated effect size of -23%.

**Table 3.4. Estimated Associations between Gestational Paternal Incarceration and Infant Birth Outcomes, Lagged Sample, by Race/Ethnicity**

	Lagged Sample	Lagged Sample, by Race/Ethnicity			
		White	Black	Hispanic	Other
<b>Preterm Birth</b>					
Proportion (Unadjusted)	0.112	0.117	0.123	0.096	0.095
Marginal Effect	-0.014*	-0.010	-0.015	-0.018	0.036
	[-0.028, 0.000]	[-0.082, 0.062]	[-0.035, 0.005]	[-0.039, 0.004]	[-0.039, 0.111]
<b>Low Birthweight</b>					
Proportion (Unadjusted)	0.106	0.108	0.118	0.087	0.116
Marginal Effect	-0.012	-0.061	-0.012	-0.008	0.003
	[-0.025, 0.002]	[-0.013, 0.013]	[-0.032, 0.007]	[-0.029, 0.013]	[-0.081, 0.087]
<b>NICU Admission</b>					
Proportion (Unadjusted)	0.132	0.157	0.139	0.115	0.182
Marginal Effect	-0.019*	0.013	-0.015	-0.026*	-0.053
	[-0.033, -0.004]	[-0.067, 0.094]	[-0.037, 0.006]	[-0.049, -0.002]	[-0.152, 0.046]
<b>Infant Death</b>					
Proportion (Unadjusted)	0.006	0.006	0.007	0.004	0.008
Marginal Effect	0.001	na	na	na	na
	[-0.003, 0.004]				
<i>N</i>	8,162	343	4,510	3,067	242

Notes: Results are reported as marginal effects to allow for comparison across models, estimated with covariates set at observed values. Asterisks denote statistically distinguishable differences between exposed and non-exposed births at the \*\*\*p<0.001 \*\*p<0.01 \*p<0.05 levels. 95% confidence intervals are reported in brackets. All models presented here adjust for paternal characteristics, maternal characteristics, maternal health/health behaviors, and health care access. “na” indicates that cell sizes were prohibitively small for estimation.

I also explore potential heterogeneity in the effect of gestational paternal incarceration on infant birth outcomes with respect to the timing, frequency, duration, and conditions of the jail incarceration. Descriptive statistics of these measures are in Table 3.2, for the lagged analytic sample overall and disaggregated by comparison groups. The regression models include measures that identify whether the birth was exposed to paternal incarceration in gestation, measures that describe the aforementioned characteristics of paternal incarceration—either in gestation or in the nine-month postpartum period (for the comparison group), as well as an interaction term between them. For the model disaggregating gestational paternal incarceration by the trimester of exposure, interaction terms are not included, as those would be perfectly collinear with the gestational paternal incarceration indicator.

Table 3.5 reports the estimated effects of gestational paternal incarceration from each of these models, across different categorical measures of timing, frequency, longest single spell, cumulative duration, and conditions of confinement (held in pretrial detention, transfer to prison). Marginal effects for the base measure of paternal incarceration in gestation from these same models are presented in Appendix Table 3.4. The results presented in Table 3.5 should be interpreted as the estimated effect of paternal incarceration in gestation across different types of paternal jail incarceration exposure within that critical period, compared to births with the same categorical timing, frequency, duration, or conditions of paternal jail incarceration exposure, but in the nine-month postpartum period.

These models do not immediately lead to a clean or conclusive picture of how the effect of paternal jail incarceration in gestation might vary with respect to the nature of incarceration. I focus here on estimated effects that are statistically distinguishable from a null effect. For models of preterm birth, paternal incarceration in gestation is negatively and statistically distinguishably

associated with a births' predicted probability of being preterm among births exposed to two or more paternal jail spells in gestation ( $dy/dx=-0.04$ , where  $dy/dx$  is the marginal effect of gestational paternal incarceration) and births to fathers whose jail spells in gestation did not conclude with transfers to prison ( $dy/dx=-0.02$ ). The models also yielded statistically distinguishable effects of gestational paternal incarceration on the predicted probability of being born low birthweight among births with gestational paternal incarceration in the first trimester of pregnancy ( $dy/dx=-0.02$ ) and gestational paternal incarceration that was not for pretrial detention ( $dy/dx=-0.03$ ; i.e. awaiting or after sentencing).

For models of NICU admission, gestational paternal incarceration was estimated to be negatively and statistically distinguishably associated with likelihood of NICU admission after delivery for births to fathers incarcerated: during the third trimester ( $dy/dx=-0.02$ ), once rather than two or more times during the gestational period ( $dy/dx=-0.02$ ), for between one day and one week for their longest single jail spell ( $dy/dx=0.04$ ), for a cumulative duration of one week or less during the gestational period ( $dy/dx=-0.03$ ), post-trial as opposed to pretrial detention ( $dy/dx=-0.03$ ), and without an observed transfer to prison following a gestational jail spell ( $dy/dx=-0.02$ ). There were no statistically distinguishable estimated effects of gestational paternal incarceration on the probability of infant death.

**Table 3.5. Estimated Associations between Gestational Paternal Incarceration and Birth Outcomes, Lagged Sample**

	<b>Preterm Birth</b>	<b>Low Birthweight</b>	<b>NICU Admission</b>	<b>Infant Death</b>
<b>Trimester</b>				
1 <sup>st</sup> Trimester	-0.017 [-0.036, 0.001]	-0.022* [-0.040, -0.004]	-0.006 [-0.026, 0.015]	-0.001 [-0.005, 0.003]
2 <sup>nd</sup> Trimester	-0.011 [-0.029, 0.006]	-0.016 [-0.033, 0.000]	-0.014 [-0.033, 0.005]	0.003 [-0.002, 0.008]
3 <sup>rd</sup> Trimester	-0.012 [-0.029, 0.005]	-0.005 [-0.022, 0.012]	-0.021* [-0.039, -0.003]	-0.001 [-0.005, 0.003]
<b># Spells</b>				
1 Spell	-0.009 [-0.024, 0.006]	-0.012 [-0.026, 0.003]	-0.021* [-0.037, -0.004]	0.001 [-0.003, 0.004]
2+ Spells	-0.040* [-0.072, -0.007]	-0.013 [-0.046, 0.021]	-0.010 [-0.047, 0.027]	0.001 [-0.008, 0.010]
<b>Longest Spell</b>				
1 Day or Fewer	-0.007 [-0.035, 0.021]	-0.005 [-0.031, 0.022]	-0.020 [-0.050, 0.010]	0.002 [-0.004, 0.009]
2-7 Days	-0.021 [-0.045, 0.004]	-0.015 [-0.040, 0.010]	-0.035* [-0.062, -0.009]	0.001 [-0.005, 0.007]
7-30 Days	-0.008 [-0.039, 0.023]	0.002 [-0.029, 0.033]	0.003 [-0.031, 0.037]	-0.002 [-0.010, 0.007]
>30 Days	-0.020 [-0.048, 0.007]	-0.026 [-0.052, 0.000]	-0.015 [-0.045, 0.015]	0.001 [-0.006, 0.008]
<b>Total Duration</b>				
1 Week or Less	-0.012 [-0.032, 0.008]	-0.013 [-0.032, 0.007]	-0.028* [-0.050, -0.006]	0.003 [-0.002, 0.007]
>1 Week	-0.017 [-0.037, 0.003]	-0.012 [-0.032, 0.008]	-0.007 [-0.029, 0.015]	-0.000 [-0.006, 0.005]
<b>Pretrial Detention</b>				
Post-trial	-0.015 [-0.033, 0.002]	-0.029** [-0.046, 0.012]	-0.025* [-0.044, -0.006]	0.001 [-0.003, 0.004]
Pretrial	-0.012 [-0.034, 0.009]	0.013 [-0.008, 0.035]	-0.010 [-0.033, 0.014]	0.001 [-0.006, 0.008]
<b>Facility Type</b>				
Jail Only	-0.017* [-0.032, -0.002]	-0.011 [-0.025, 0.004]	-0.023** [-0.039, -0.007]	0.001 [-0.002, 0.005]
Jail and Prison	-0.003 [-0.039, 0.032]	-0.018 [-0.053, 0.018]	0.008 [-0.032, 0.048]	-0.000 [-0.012, 0.012]

Notes: N=8,162. Results are reported as estimated marginal effects of paternal incarceration in gestation on predicted probabilities of each birth outcome, with covariates set at observed values. Reference group is those with paternal incarceration in the nine months following the birth. Asterisks note statistically distinguishable differences between exposed and non-exposed births at \*\*\*p<0.001 \*\*p<0.01 \*p<0.05 levels. 95% confidence intervals are reported in brackets. Models adjust for parental characteristics, maternal health and health behaviors, and health care access, and also include interaction terms of gestational paternal incarceration with measures disaggregating exposure to paternal incarceration (e.g. total number of spells).

## Discussion

This study contributes leverages the large population-level “sample” and detailed and objective indicators of parental incarceration and health found in administrative records to examine potentially causal effects of paternal incarceration on birth outcomes in the United States. The detailed incarceration spell-level information and hospital record information on infant and maternal health and characteristics in this novel data set allow us to revisit estimates of these important associations and explore the possibility of causal processes with the advantage of new administrative data. Using linked birth, death, and jail records from the city of New York between the years of 2010 and 2016, I find that, counter to what is suggested in extensive prior research, paternal incarceration in gestation may not be independently positively associated with the likelihood that a birth is preterm, low birthweight, requires NICU admission upon delivery, or results in infant death from non-congenital cause.

Taken together, this analysis leads to four key conclusions. First, the descriptive “associational” analysis confirms prior work identifying strong negative associations between gestational paternal incarceration and health in early life. Second, exploiting variation in the timing of paternal incarceration in close temporal proximity to birth, I do not find evidence of negative effects of paternal incarceration in gestation on infant health as measured by birth outcomes. In fact, I find suggestive evidence of null or even protective effects of paternal incarceration. This discrepancy between these sets of results suggest experiences and disparities predictive of paternal incarceration may underlie linkages between paternal incarceration and infant health. Third, I do not find consistent evidence of heterogeneity in these effects across racial/ethnic groups or with respect to variation in the frequency, duration, timing, and facility type of paternal incarceration during the gestational period. Finally, descriptive differences in the

details of paternal jail incarceration between fathers incarcerated in the gestational and postpartum periods suggest that there may be important variation in family process and these types of paternal incarceration that merit further investigation.

Administrative data hold many strong empirical advantages for examining the association between parental incarceration and infant outcomes. However, these results are faced with some important limitations, two of which are due to the fact that information on jail incarceration come from NYC DOC records. These data apply exclusively to incarceration in jails within the city of New York, which constrain the completeness of available information about parental incarceration, both in terms of geography and type of contact with the criminal justice system. First, because the data only capture jail incarceration spells, I am not able to account for parental incarceration that occurs in prison (rather than jails) at the time of birth or during pregnancy. Although I estimate models that examine heterogeneity across births to fathers who are only observed incarcerated in jails and to fathers who are incarcerated in jails and are then transferred to prisons, I miss any births in NYC to mothers who were incarcerated in prisons at the time of delivery. Second, because these data are for institutions within the NYC jail system, I am unable to capture paternal incarceration that occurs entirely outside the city's system (i.e. in prisons or local jails outside of NYC) and cannot be recovered using information about discharge type.

A third limitation arises from the fact that 10 percent of the birth records I analyze are missing information on birth fathers, which is likely to represent a non-random subsample of births and families in NYC. Finally, I am limited by the characteristics of the vital records, the source of birth and health information in these analyses. One challenge here is that available measures of health behaviors and health care are likely to be impacted by underreporting and inconsistent measurement, as in other research examining the consequences of marginalization.

Further, these NYC birth records do not include any measures of paternal health behaviors potentially associated with infant birth outcomes. Additionally, my reliance on vital records means that the conclusions drawn here are exclusively relevant to pregnancies that culminated in live births. In doing so, this analysis misses miscarriages and stillbirths, an important subgroup of pregnancy outcomes that also serve as important indicators of population health and health inequality (Bruckner and Catalano 2018; Nobles and Hamoudi 2019)

These results complicate the existing understanding of the relationship between paternal incarceration and infant health. Future work could further elucidate these dynamics and the role of parental incarceration as a risk factor, cause, or protective factor for infant health by delving further into the detailed information available in administrative records. This work could explore potential heterogeneity by the types of charges and offenses linked to birth fathers' incarceration as well as variation within families for those who appear in these data multiple times (i.e. biological siblings that vary in their exposure to gestational paternal incarceration). These findings support prior evidence documenting negative descriptive associations between paternal incarceration in gestation and child wellbeing but also indicate that children exposed to this adverse event in gestation may be faring better than those who go on to experience paternal incarceration in the early months of life. Although these analyses face some limitations, analysis of a near-population-level database constructed through record linkage provides a unique and insightful complement to the wealth of knowledge that social scientific research has developed in recent years on the consequences and correlates of parental incarceration.

### Appendix 3

**Appendix Table 3.1. Comparison of Full All Births and Associational and Lagged Samples**

	All Births	Associational Sample	Lagged Sample
<b>Parental Characteristics at Birth</b>			
Father's Age (Years)	33.07 (7.32)	33.06 (7.24)	28.16 (7.39)
Father's Race/Ethnicity			
White	0.36	0.36	0.04
Black	0.18	0.18	0.55
Hispanic	0.28	0.28	0.38
Asian/Pacific Islander	0.17	0.17	0.02
Other	0.01	0.01	0.01
Father's Education			
<High School Degree	0.18	0.21	0.41
High School Degree/GED	0.21	0.24	0.42
Postsecondary Education	0.49	0.56	0.17
Mother's Age (Years)	29.81 (6.11)	30.10 (5.89)	25.44 (5.92)
Mother's Race/Ethnicity			
White	0.32	0.35	0.06
Black	0.20	0.17	0.49
Hispanic	0.30	0.29	0.42
Asian/Pacific Islander	0.17	0.19	0.02
Other	0.01	0.06	0.01
Mother's Education			
<High School Degree	0.20	0.18	0.37
High School Degree/GED	0.22	0.21	0.30
Postsecondary Education	0.58	0.61	0.34
Parents Married at Birth	0.60	0.67	0.15
Father Missing on Birth Certificate			
<b>Maternal Health</b>			
Maternal Body Mass Index	25.04 (5.65)	24.82 (5.47)	26.68 (6.42)
Cigarette Use during Pregnancy	0.01	0.01	0.04
Alcohol Use during Pregnancy	0.01	0.01	0.01
Trimester of 1 <sup>st</sup> Prenatal Care			
1 <sup>st</sup> Trimester	0.73	0.75	0.62
2 <sup>nd</sup> Trimester	0.20	0.19	0.29
3 <sup>rd</sup> Trimester or None	0.07	0.06	0.10
Health Insurance: Medicaid	0.58	0.55	0.84
Prior Low Birthweight Birth	0.09	0.09	0.10
Prior Preterm Birth	0.08	0.09	0.10
<i>N</i>	855,632	632,315	8,162

Notes: Standard deviations are reported in parentheses.

**Appendix Table 3.2. Estimated Associations between Gestational Paternal Incarceration and Infant Birth Outcomes, Associational Sample**

	<b>Overall Proportion</b>	<b>Model 1</b>	<b>Model 2</b>	<b>Model 3</b>	<b>Model 4</b>
<b>Birth Outcomes</b>					
Preterm Birth (<37 weeks)	0.066	0.044*** [0.035, 0.052]	0.017*** [0.011, 0.022]	0.019*** [0.012, 0.026]	0.019*** [0.012, 0.026]
Low Birthweight (<32 weeks)	0.059	0.048*** [0.040, 0.057]	0.019*** [0.014, 0.024]	0.020*** [0.013, 0.027]	0.020*** [0.013, 0.026]
NICU Admission	0.087	0.044*** [0.035, 0.053]	0.010** [0.003, 0.017]	0.009** [0.002, 0.016]	0.009** [0.002, 0.016]
Infant Death (<1 year, non-congenital cause)	0.002	0.003*** [0.001, 0.005]	0.001 [-0.001, 0.002]	0.001 [-0.001, 0.002]	0.001 [-0.001, 0.002]
<b>Father Characteristics</b>			X	X	X
<b>Maternal Health</b>				X	X
<b>Health Care Access</b>					X

Notes: N=632,315. Results are reported as marginal effects to allow for comparison across models, estimated with covariates set at observed values. Asterisks denote statistically distinguishable differences between exposed and non-exposed births at the \*\*\*p<0.001 \*\*p<0.01 \*p<0.05 levels. 95% confidence intervals are reported in brackets.

**Appendix Table 3.3. Estimated Associations between Gestational Paternal Incarceration and Birth Outcomes, Lagged Sample**

	<u>Overall Proportion</u>	<u>Model 1</u>	<u>Model 2</u>	<u>Model 3</u>	<u>Model 4</u>
<b>Infant Outcomes</b>					
Preterm Birth	0.112	-0.013 [-0.027, 0.000]	-0.014 [-0.028, 0.001]	-0.014* [-0.028, 0.000]	-0.014* [-0.028, 0.000]
Low Birthweight	0.106	-0.012 [-0.025, 0.001]	-0.011 [-0.025, 0.002]	-0.012 [-0.025, 0.002]	-0.012 [-0.025, 0.002]
NICU Admission	0.132	-0.020* [-0.034, -0.005]	-0.019* [-0.034, -0.004]	-0.019* [-0.033, -0.004]	-0.019* [-0.033, -0.004]
Infant Death	0.006	0.001 [-0.003, 0.004]	0.001 [-0.003, 0.004]	0.001 [-0.003, 0.004]	0.001 [-0.003, 0.004]
<b>Father Characteristics</b>			X	X	X
<b>Maternal Health</b>				X	X
<b>Health Care Access</b>					X

Notes: N=8,162. Results are reported as marginal effects to allow for comparison across models, estimated with covariates set at observed values. Asterisks denote statistically distinguishable differences between exposed and non-exposed births at the \*\*\*p<0.001 \*\*p<0.01 \*p<0.05 levels. 95% confidence intervals are reported in brackets. All models presented here adjust for paternal characteristics, maternal characteristics, maternal health/health behaviors, and health care access, as in Models 4 presented in Table 2.

**Appendix Table 3.4. Estimated Associations between Gestational Paternal Incarceration and Birth Outcomes, Lagged Sample**

	<u>Preterm Birth</u>	<u>Low Birthweight</u>	<u>NICU Admission</u>	<u>Infant Death</u>
<b>Trimester</b>				
1 <sup>st</sup> Trimester	-0.017 [-0.036, 0.001]	-0.022* [-0.040, -0.004]	-0.006 [-0.026, 0.015]	-0.001 [-0.005, 0.003]
2 <sup>nd</sup> Trimester	-0.011 [-0.029, 0.006]	-0.016 [-0.033, 0.000]	-0.014 [-0.033, 0.005]	0.003 [-0.002, 0.008]
3 <sup>rd</sup> Trimester	-0.012 [-0.029, 0.005]	-0.005 [-0.022, 0.012]	-0.021* [-0.039, -0.003]	-0.001 [-0.005, 0.003]
<b>Number of Spells</b>	-0.015* [-0.028, -0.001]	-0.012 [-0.025, 0.002]	-0.019* [-0.034, -0.004]	0.001 [-0.003, 0.004]
<b>Longest Single Spell</b>	-0.015* [-0.028, -0.001]	-0.012 [-0.025, 0.001]	-0.019* [-0.034, -0.004]	0.001 [-0.003, 0.004]
<b>Cumulative Duration</b>	-0.014 [-0.029, 0.001]	-0.013 [-0.028, 0.002]	-0.016 [-0.032, 0.001]	0.001 [-0.003, 0.004]
<b>Pretrial Detention</b>	-0.014* [-0.028, 0.000]	-0.012 [-0.025, 0.002]	-0.019* [-0.033, -0.004]	0.001 [-0.003, 0.004]
<b>Facility Type</b>	-0.015* [-0.029, -0.001]	-0.012 [-0.025, 0.002]	-0.018* [-0.033, -0.003]	0.001 [-0.002, 0.005]

Notes: N=8,162. Results are reported as estimated marginal effects of paternal incarceration in gestation on predicted probabilities of each birth outcome, with covariates set at observed values. Asterisks note statistically distinguishable differences between exposed and non-exposed births at \*\*\*p<0.001 \*\*p<0.01 \*p<0.05 levels. 95% confidence intervals are reported in brackets. Models adjust for parental characteristics, maternal health/health behaviors, and health care access, and also include interaction terms of gestational paternal incarceration with measures disaggregating exposure to paternal incarceration (e.g. total number of spells).

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