"HIDDEN CURRICULUM" REVISITED: HIGH SCHOOL SOCIOECONOMIC COMPOSITION AND LABOR MARKET OUTCOMES IN ADULTHOOD

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ABSTRACT

How do neighborhoods and schools experienced in adolescence shape labor market outcomes in adulthood? Research on both early life neighborhood and school conditions finds effects for earnings later in life, but it is not clear whether these associations are dependent on educational attainment. Drawing on the National Longitudinal Study of Adolescent to Adult Health and marginal mean weighting through stratification (MMWS) techniques for multi-category treatment effects with observational data, I estimate differences in self-reported wages and workplace autonomy in adulthood among non-college goers who were exposed to, in adolescence 1. neither high-poverty neighborhoods or high-poverty schools, 2. high-poverty neighborhoods but not high-poverty schools, 3. high-poverty schools but not high-poverty neighborhoods, and 4. both high-poverty neighborhoods *and* high-poverty schools. Most in line with institutional resource perspectives of neighborhood effects, consequences are consistently observed only among respondents who were exposed to both high-poverty neighborhoods *and* high-poverty schools.

How do schools shape inequality across generations? While differences between schools play a peripheral role in explaining academic achievement, school-level socioeconomic composition is robustly associated with educational attainment outcomes such as high school graduation and college attendance (Coleman et al. 1966; Ainsworth 2002; Lauen and Gaddis 2013; Palardy 2013; Jennings et al. 2015; Morgan and Jung 2016; von Hippel, Workman, and Downey 2018). Thus, through the attainment of educational credentials, there is evidence that schools experienced early in life contribute to inequalities in eventual labor market outcomes (Lleras 2008). Additionally, evidence from randomized experiments of exposure to high-quality teachers illustrate clear effects on earnings in adulthood, but to what extent these benefits are dependent on high school or college completion has yet to be fully investigated (Chetty et al. 2011; Chetty, Friedman, and Rockoff 2014). Taken together, then, whether schools directly affect labor market outcomes independent of educational attainment remains unknown.

While long-term effects of school quality on labor market outcomes may be dependent on the application of academic skills (e.g. college completion), theories of how schools differentially socialize youth for the labor market hypothesize effects for youth who directly enter the labor market. Specifically, the *correspondence* perspective on the relationship between schools and the labor market contends that schools condition students for jobs that reflect their social background (Bowles and Gintis 1976, 2002). High-poverty schools are argued to emphasize supervision, self-control, and rule compliance, whereas schools serving more affluent students are thought to emphasize autonomy, collaboration, and engagement with ideas. Thus, through differential conditioning of non-cognitive skills, school socioeconomic composition is hypothesized to be directly associated with labor market outcomes (Deming 2017).

Still, youth spend most of their waking hours exposed to contexts beyond the school walls, with a substantial literature corroborating consequences of concentrated neighborhood poverty for academic achievement, attainment, and earnings in adulthood (Sharkey and Elwert 2011; Wodtke, Harding, and Elwert 2011; Chetty, Hendren, and Katz 2016; Wodtke and Parbst 2017; Hicks et al. 2018; Levy 2018). As is the case with school effects, however, whether effects of neighborhood poverty on labor market outcomes are conditional on educational attainment remains to be known. Even so, cultural isolation and social disorganization theories of neighborhood effects hypothesize processes that affect labor market outcomes independent of educational progress (Wilson 1987; Jencks and Mayer 1990; Sampson, Raudenbush, and Earls 1997; Harding and Hepburn 2014).

Though neighborhood and school socioeconomic factors are hypothesized to have independent effects on labor market outcomes, resources between these contexts are deeply interrelated. For most students in the U.S., public school attendance assignments are largely based on residential proximity. As a result, for example, youth residing in high-poverty neighborhoods tend to go to school with students from neighborhoods of similar compositions. There is thus considerable overlap in socioeconomic resources in the home and school environments for most public school students, with a substantial proportion being exposed to both concentrated poverty in their neighborhood and at school (Owens and Candipan 2019). Institutional resource theory suggests that these students are further likely to reside in areas lacking in access to child and health care institutions, hypothesizing especially detrimental socioeconomic attainment outcomes for residents of these doubly disadvantaged areas (Wilson 1987; Jencks and Mayer 1990).

Taken together, direct effects of exposure to both high-poverty neighborhoods and schools are motivated for investigations labor market outcomes among students who do not attend or attain college credentials. Both the correspondence perspective and institutional resource theory predict additive effects of concentrated neighborhood and school poverty, while prevailing findings from studies of neighborhood effects suggests similar effects for youth residing in high-poverty neighborhoods independent of their school context. I test these competing hypotheses using data from the National Longitudinal Study of Adolescent to Adult Health (Add Health). Family background, neighborhood, and school poverty are all measured at Wave I when respondents are largely enrolled in high school, while labor market and educational attainment outcomes are measured at Wave IV when respondents are between the ages of 24 and 32. The analytic sample consists of respondents who are currently working at least 10 hours a week or report being temporarily laid off or on medical leave and who have not completed a two-year college degree or higher, thus also considering respondents who have completed only some college and who hold vocational training certifications.

Compared to youth who reside in neighborhoods with less than forty percent of residents in poverty and who attend schools in which less than forty percent of students are eligible for free or reduced priced lunch (FRPL), nominal treatment categories are constructed for adolescents who 1. grew up in high-poverty neighborhoods but did not attend high-poverty schools, 2. attended high-poverty schools but did reside in high-poverty neighborhoods, and 3. both attended high-poverty schools and resided in high-poverty neighborhoods. My analyses pay special attention to selection into concentrated neighborhood and school poverty, attempting to be conservative while also minimizing the risk of over controlling for contextual effects (Sampson, Sharkey, and Raudenbush 2008; Hong 2010, 2012; Linden 2014). Labor market outcome measures include hourly wages and perceived workplace autonomy. In line with the institutional resource perspective on neighborhood effects, neither concentrated neighborhood poverty nor concentrated school poverty are independently associated with the outcome measures, while respondents who grew up in a high-poverty neighborhood *and* attended a high-poverty school have lower hourly wages and less autonomy to make important decisions at work. In contrast with findings regarding academic achievement, this research suggests that school poverty represents a key dimension of concentrated neighborhood poverty, heretofore unconsidered in investigations of non-academic outcomes (Wodtke and Parbst 2017).

SCHOOLS AND INEQUALITY

Research examining between-school effects on learning and achievement have enlightened our understanding of both the power and powerlessness of schools to resolve American inequality (Bryk and Driscoll 1988; Palardy 2008, 2013). On the one hand, scholars have shown compellingly that academic success is hardly the product of academic aptitude and effort alone. Socioeconomic resource differences existing between schools are vast, and undoubtedly fall short of the promise of "Equality of Educational Opportunity" (Coleman et al. 1966; Orfield and Lee 2005). And even if school differences were equalized, it is evident that inequalities between families would continue to have profound influences on how students experience and navigate educational environments (Roscigno and Ainsworth-Darnell 1999; Morris 2005; Crosnoe 2009; Crosnoe and Muller 2014). Another group of scholars, however, contrasts with this critical view, pointing out that the racial achievement gap grows only in the summer and in fact reduces when school is in session (Alexander, Entwisle, and Olson 2001; Downey, von Hippel, and Broh 2004; von Hippel et al. 2018). In this view, schools are argued to be "great equalizers" by comparison with a counterfactual in which neither advantaged nor disadvantaged families have access to public schooling. Much of this work focuses on academic achievement outcomes, however, leaving open the question of to what extent schools shape inequality in ways more directly tied to labor market success.

Scholars have also considered the link between school socioeconomic resources and high school graduation and college attendance (Palardy 2013; Jennings et al. 2015). How schools may play a role in the stratification for students who do not go on to college, however, remains widely unknown. Most central to the present study is Bowles and Gintis's correspondence principle and Jean Anyon's extension to a *"hidden curriculum of work"* (Bowles and Gintis 1976, 2002; Anyon 1980). Specifically, Bowles and Gintis argued:

> Different levels of education feed workers into different levels within the occupational structure and, correspondingly, tend toward an internal organization comparable to levels in the hierarchical division of labor. … predominantly working-class schools tend to emphasize behavioral control and rule-following, while schools in well-to-do suburbs employ relatively open systems that favor greater student participation, less direct supervision, more student electives, and, in general, a value system stressing internalized standards of control (Bowles and Gintis 1976:132).

Beyond sorting students academically by socioeconomic background, the argued mechanism relating educational experiences to positions in the occupational structure is the conditioning of non-cognitive skills (Levy and Richard J. 2005), or social skills (Deming 2017). Students from lower socioeconomic backgrounds are thought to be exposed to schools more focused on control, obedience, and rule-following. At the other end of the spectrum, students from higher socioeconomic backgrounds attend schools in which they are deeply involved in classroom activities, offered electives and general choice, and socialized to be self-regulating. For Anyon

(1980), this process extends beyond conditioning via interactions between students and teachers to the hidden curriculum of work. Whereas students in low-resource schools were found to be working on tasks which reward accuracy in repetition, students attending middle class schools were rewarded for their originality and ability to self-express in a variety of forms. Evidence for the general form of correspondence largely validates the presence of these conditioning exercises, but to what extent they are consequential in the labor remains understudied. At the individual-level, for example, associations between high school GPA, interest in school, and truancy have all been found to be associated with income among non-college goers in early adulthood (Rosenbaum 2001). The first goal of this study is to test the school-level hypothesis of the correspondence principle.

NEIGHBROHOODS AND INEQUALITY

Neighborhoods have a precedent over schools in the literature on contextual effects for a wide variety of individual-level outcomes. Since the release of the Coleman Report, scholars of education have largely concluded that the key to understanding achievement disparities lies beyond the school walls, or in out-of-school factors (Coleman et al. 1966; Borman and Dowling 2010; Alexander and Morgan 2016; Downey and Condron 2016). Chief among these is the residential neighborhood context (Wilson 1987; Mayer and Jencks 1989; Sampson, Morenoff, and Gannon-Rowley 2002; Chetty et al. 2016). Extant neighborhood effects research from a variety of study designs finds robust effects of exposure to concentrated neighborhood poverty on a range of development as well as educational and work-related outcomes (Sampson et al. 2008; Sharkey and Elwert 2011). While institutional resource theories of effects of neighborhood poverty emphasize the role of schools and other local institutions (Wilson 1987; Jencks and Mayer 1990), other hypothesized mechanisms include low levels of informal social control coupled with a high degree of social disorganization (Sampson et al. 1997), low prevalence of prosocial adult role models (Wilson 1987), and environmental health hazards such as lead poisoning (Sampson and Winter 2016).

Central to present study are recent evaluations of the Moving to Opportunity randomized housing experiment, which enabled residents of high-poverty neighborhoods to move to low-poverty neighborhoods and affordable public housing options. While early evaluations found little improvement in academic achievement gains between treatment and control groups, (Sanbonmatsu et al. 2006), more recent assessments focusing on outcomes in adulthood find robust effects of residence in high-poverty neighborhoods on both earnings, college attendance rates, and the quality of colleges attended. Notably, these effects are unlikely to be due to related changes in school contexts, as many participants either opted to remain in their pre-treatment assignment school or transferred to schools of similar compositions and quality (Briggs et al. 2008). Thus, two key questions remain. First, results for earnings in adulthood are presented as differences between treatment and control groups without consideration of differences in college attendance, thus leaving open the question what the impact of neighborhood reassignment for earnings is independent of educational attainment. Second, because participants experienced only slight changes in school-based exposures, the MTO experiment also leaves open the question of how differential exposure to school resources shapes labor market outcomes. Further, though randomized experiments of exposure to teacher and school quality find effects on academic achievement, attainment, and earnings in adulthood, these studies are similarly unclear about whether effects on earning are dependent on educational attainment. Thus, how neighborhoods and schools may be directly relevant for labor market success among those who do not go on to college remains an important question.

SUMMARY OF THE PRESENT STUDY

This study examines whether concentrated neighborhood and school poverty experienced in adolescence are independently or jointly associated with labor market outcomes in adulthood among those who have not attained a college credential. The correspondence perspective on the relationship between schools and labor market outcomes predicts a negative effect of concentrated school poverty independent of concentrated neighborhood poverty (Bowles and Gintis 1976). In contrast, extant neighborhood effects research suggests that the effect of concentrated neighborhood poverty will be similar for youth attending both high- and low-poverty schools. Finally, institutional resource theory suggests that concentrated neighborhood and school poverty will have additive effects, expecting that youth who both reside in high-poverty neighborhoods and attend high-poverty schools will be the least successful in the labor market.

To test these hypotheses, I employ regression adjustment and probability of treatment weighting methods for observational studies to account for potential pathways for selection into concentrated neighborhood and school poverty. Specifically, I draw on marginal mean weighting through stratification (MMWS) methods for multi-category treatment effects (Hong 2010; Linden 2014). This probability of treatment weighting method has been found to produce accurate estimates under various model misspecification circumstances, with results tending to be more accurate than estimates based on traditional inverse probability of treatment weight methods (Hong 2012). Provided that the data used to measure neighborhood and school selection are cross-sectional, however, I aim only to produce accurate estimates of the relationships between neighborhood and school concentrated poverty and labor market outcomes, and thus do not make strong causal claims.

DATA AND METHODS

Data are from Waves I and IV of the National Longitudinal Study of Adolescent to Adult Health (Add Health), a nationally representative school-based study of adolescents in the United States (Harris et al. 2009). The sampling frame included 80 high schools and additional feeder middle schools stratified by region, school urbanity, sector, and size. Wave I data (n = 20,745; grades 7-12) were collected from students in grades seven to twelve in 1994-95, and Wave IV data (n = 15,701; ages 24-32) were collected in 2008. School-level data were collected from school administrators, from student in-school surveys, and later by linking data from the National Center for Education Statistics. Data are also available for respondents' block groups of residence and were used to construct the present measure of concentrated neighborhood poverty (Levy 2018). Interested specifically in how neighborhood and high school experiences relate to labor market outcomes, I additionally draw on the Adolescent Health and Academic Achievement (AHAA) supplemental files. Researchers at the University of Texas at Austin linked final high school transcripts to most of the Add Health Wave III participants. This allows for the exclusion of students who are known to have transferred out of Add Health high schools. The analytic sample is constrained to respondents who have dropped out of high school, completed high school, completed any or some vocational training, and who have completed some college but have not attained a college credential, and who are either currently working at least 10 hours per week or who report being only temporarily out of the labor market[2]. Missing data across all variables are handled using multiple imputation with chained equations with 20 imputed data sets.

[2] Including self-reports of being temporarily laid off, on temporary medical or paternity leave, and excluding individuals who identity as being permanently disabled, unemployed and looking for work, and unemployed and not looking for work.

DEPENDENT MEASURE

With the key theoretical mechanism linking schools and labor market outcomes being the conditioning of non-cognitive skills, the present dependent variables are intended not just to tap quality of employment, but the extent to which respondents are in jobs that are socially demanding. ***Wages*** in adulthood was measured in three stages. First, respondents were asked to report the exact total of their wages and salaries, including tips, bonuses, overtime pay, and self-employment before taxes for the previous year. Respondents who were unsure about their total personal income were then presented with a set of income categories and asked to select their "best guess" of which category matched their income. Respondents were also asked about the number of hours they work per week across all their current jobs. Following Sabia (Sabia and Rees 2012), personal earnings is then divided by hours worked multiplied by 50 (out of 52 weeks in a year). To account for outlier wage values, wages were conservatively top coded to $100, respondents reporting less than $1 per hour wages were dropped, and then the measure was log transformed to account for skewness (Sabia and Rees 2012; Sabia 2014; Mize 2016). ***Workplace Autonomy*** is based on responses to the question "Overall, how often (do/did) you have the freedom to make important decisions about what you (do/did) at work and how you (do/did) it [in your current primary/most recent job]? Response categories are: 1 = "none or almost none of the time," 2 = "some of the time," 3 = "most of the time," 4 = "all or almost all of the time," and are left as is[3].

NEIGHBORHOOD AND SCHOOL CONCENTRATED POVERTY CATEGORIES

[3] Other dependent variables considered include a binary indicator of workplace manager status and an ordinal measure of workplace repetitiousness. These measures are not adequately distributed among the focal categories of neighborhood and school concentrated poverty, however.

I use residential block groups to define the neighborhood context, and employ the conventional cutoff of forty percent of residents living below the poverty line to operationalize concentrated neighborhood poverty (Small, Jacobs, and Massengill 2008; Levy 2018). There are drawbacks to imposing cutoffs to potential linear associations, but recent reanalyzes of Moving to Opportunity data suggest threshold effects of neighborhood poverty (Burdick-Will et al. 2011). Concentrated school poverty is similarly measured as attendance at a school with at least forty percent of students receiving free or reduced-price lunch (FRPL). The measure of proportion of students receiving FRPL comes from the NCES and was linked by Add Health and Academic Achievement researchers at the University of Texas—Austin. To protect the confidentiality of the schools, this proportion was rounded to the nearest .05, and ranges from 0 to 1. This information is only available for public schools, and thus students attending private schools are excluded from the present analyses. Though this limits the generalizability of my findings to public schools, it may serve to slightly diminish the role of unobserved heterogeneity (e.g., advantaged parents selecting into private schools).

Mutually exclusive categories of exposure to concentrated neighborhood and school poverty are constructed from these measures. First, the baseline (i.e. “control group”) category consists of respondents who resided in block groups and attended schools at Wave I which are not characterized by concentrated poverty (n = 3,405). The first concentrated poverty category (i.e. treatment group) includes only respondents who resided in concentrated neighborhood poverty but did not attend schools characterized by concentrated poverty (n = 128). The second category includes respondents who attended schools characterized by concentrated poverty but did not reside in neighborhoods characterized by concentrated poverty (n = 847). The third and final category includes respondents who both resided in concentrated neighborhood poverty *and* attended schools characterized by concentrated poverty (n = 234)[4].

SELECTION MEASURES

When examining the influences of school and neighborhoods it is important to consider the role that family selection may play in choosing children's neighborhoods and schools and in monitoring their influence. Drawing on past research examining selection into neighborhood and school contexts, a first task of this paper is to explicitly model the probability of membership in each of the four concentrated poverty categories. Past research drawing on Add Heath data in the estimation of selection into neighborhoods and schools has focused on how family and contextual chrematistics at Wave I predict neighborhood and school environments inhabited 1-2 years later at Wave II (Crosnoe 2009; Levy 2018). However, as Levy (2018) points out, Wave I neighborhood context is highly correlated with Wave II neighborhood context. Additionally, use of Wave II data necessitates the exclusion of respondents who are largely high school seniors at Wave I, and thus who may be important representatives of labor market outcomes years later in adulthood. As such, I proceed drawing on only Wave I data for the purposes of estimating the propensity of membership in the four concentrated poverty categories, foregoing potential advantages of including temporally relevant selection data at Wave II in exchange for a considerable increase in sample size.

Also in contrast to past Add Health research modelling selection into neighborhoods and schools, I do not consider measures drawn of the Add Health Wave I parent survey, section A, including respondent birth weight and parent-reported reasons for choosing their Wave I

[4] While seemingly small proportions of the analytic sample, these n sizes are similar to those reported by Levy (2018), who analyzed heterogeneous effects of concentrated neighborhood poverty among 287 Add Health participants.

neighborhood (e.g. proximity to work, prevalence of drugs, school quality). As important as these items may be to the propensity of membership in the present concentrated poverty categories, use of these items necessitate a considerable loss of Wave I respondents. Additionally, missingness on responses to these questions are systematically missing by family and neighborhood socioeconomic resources, with the most disadvantaged respondents' parents being about 45% more likely to have not be asked these questions than the most advantaged respondents[5,6].

Following past research on neighborhood and school selection, I consider family socioeconomic resources, parental educational involvement, parental college aspirations, parent-child closeness, measured academic ability (Peabody Picture Vocabulary test scores, respondents' years residing at their Wave I residence, gender, race, nativity status, family structure, and age (Crosnoe 2009; Levy 2018). *Family Socioeconomic Status (SES)* is constructed using the Moody and Bearman approach, taking the highest values of parental educational attainment and occupational status reported by parents and adolescents yielding a scale that ranges from 0 to 9 (Bearman, Moody, and Stovel 2004). *Family Income*[7] is based on the maximum amount reported between the respondent and their interviewed parent and log-transformed to account for skewenss. I supplement these indicator of family socioeconomic resources with an indicator of whether any interviewed family member reported *receipt of*

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[5] Estimated from models regressing the present 0-9 family socioeconomic status measure with a squared term on missingness on parent survey section A. The probability of missingness is about .43 among respondents in the lowest family SES category and about .29 among respondents in the highest family SES category.
[6] In supplemental analyses (not shown), I include these items in the section model used to generate the propensity of membership in each of the concentrated poverty categories. Parent-reported reasons for living in their neighborhood at Wave I are predictive of category membership, and do not balance (i.e. become nonsignificant) in models weighted by MMWS. One plausible reason for this is that these items may tap parent evaluations of neighborhood resources and social disorganization rather than parental propensity to select into their present neighborhood contexts.
7 Because the primary driver of missingness in the analytic samples is family income (24%), a squared term for family socioeconomic status was included in MICE equations.

welfare benefits. *Parental college aspirations* is based on parent responses to the question “How disappointed would you be if [child] did not graduate from college?” with response categories included “very disappointed,” somewhat disappointed,” and “not disappointed.” *Parents’ educational involvement* is based on student reports of whether, in the past month, their residential mother talked with them about grades, class projects, or school in general. These same questions were asked about their resident father. Items were summed for each parent and combined (alpha = .84). If one parent is not present, responses for the non-missing parent are used. *Parental closeness* is similarly measured based on student assessments of perceived closeness, care, communication satisfaction, warmth, and overall relationship quality with their residential mother and father (alpha = .85). Measured academic ability is based on student scores on an abridged Add Health version of the Peabody Picture Vocabulary test. *Demographic controls.* Self-reports of race and ethnicity are combined to yield four mutually exclusive categories including non-Hispanic white, non-Hispanic black, any Hispanic origin, and some other race. I additionally include a control for foreign born status (1 = foreign born, 0 = native born). A dichotomous control for living with two biological parents was included as a control for family structure, with all other family situations being set equal to 0. The present measure of gender is based on adolescent self-reports of biological sex where 1 = female, 0 = male. Finally, I include a control for respondents’ age at Wave I. I also include a binary control for *Middle school status* that indicates that at Wave I the respondent was attending a middle but is known to have eventually attended one of the Add Health high schools included in the analytic sample[8].

METHODS

[8] According to linked Add Health and Academic Achievement supplemental transcript files.

Results proceed in three steps. First, a multinomial logic regression model is used to estimate the generalized propensity scores (i.e. predicted probabilities) of membership in each of the four concentrated poverty categories. From these probabilities, a range of common support is defined for each concentrated poverty category using the minimum of the maximum and the maximum of the minimum probabilities of membership within each category as cutoff points (Hong 2012). Next, the probabilities of category membership for the remaining respondents represented within the range of common support are used to calculate marginal mean weights (Hong 2012; Linden 2014). A first step in this process is to stratify each of the four propensity scores (one for each concentrated poverty category) into five quintiles. Though more or fewer strata are optional, use of five strata is conventional and has been shown to remove over 90% of the selection bias resulting in the provided covariates (Rosenbaum and Rubin 1983, 1984). Once the strata are obtained, the weights can be estimated (Linden 2014):

$$\frac{n_{s_t} \times \Pr(T = t)}{n_{T=t,s_t}}$$

Where $\Pr(T = t)$ is the proportion of respondents in treatment category t (i.e. concentrated poverty category), n_s is the number of respondents in stratum s_t of treatment category t, and $n_{T=t,s_t}$ is the number of respondents in stratum s_t who are in treatment category t. Within each treatment category, the weight thus increases the representation of respondents with a relatively low probability of being in that treatment category and decreases the representation of individuals within a strata with a relatively high probability of being in that treatment category.

Finally, these weights are included in regression models of wages and workplace autonomy with indicator variables for the three concentrated poverty categories referencing individuals who did not reside in high-poverty neighborhoods or attend high-poverty schools in adolescence. When models are weighted according to this procedure, average treatment effects can then be estimated by contrasting outcome means between any two treatment categories. Marginal mean weights through stratification have been shown to produce more accurate estimates and less variable weights than those generated when using traditional inverse probability of treatment weighting strategies in cases of binary treatments (Huang et al. 2005; Hong 2010), though recent comparisons of these methods in cases of nominal treatments finds similar results between weighting strategies[9] (Linden et al. 2016).

RESULTS

SELECTION MODEL

Descriptive statistics for all model results are presented in Table 1. The first column set displays descriptive statistics for all respondents used to generate the MMWS weights, including individuals outside the range of common support. The second column represents these respondents when excluding individuals outside the range of common support. A considerable proportion of the sample is lost between these columns. Most notably, the proportion of individuals at Wave I who are exposed to neither concentrated neighborhood nor school poverty has decreased from 3,405 to 2,403 respondents. Individuals exposed to only concentrated neighborhood poverty reduced from 128 to 117 respondents. Individuals exposed to only concentrated school poverty reduced from 847 to 741 respondents, and individuals exposed to both concentrated neighborhood and school poverty reduced from 234 to 217 respondents. Comparisons of average family socioeconomic status, family income, family welfare receipt proportion, and the proportion of respondents who lived with both of their biological parents at Wave I suggests that the individuals outside the range of common support were largely more advantaged individuals. Also notable is that the Wave I middle school students known to have

[9] Indeed, in models not shown, using conventional IPTW methods yielded substantively identical conclusions to those presented here.

gone on to an Add Health high school are entirely outside the range of common support and excluded from all but the first column set. Though 190 fewer individuals are represented in models of hourly wages at Wave IV, comparison of the descriptive statistics suggests minimal differences between the samples.

Table 2 presents multinomial logistic regression models of the likelihood of being in any of the given concentrated poverty categories compared to those exposed to neither concentrated neighborhood nor school poverty in adolescence. The first model set is for all respondents, including those who are outside the range of common support, while the second model set excludes these respondents. Results of these models are largely the same, with only minor differences in the magnitude and statistical significance of coefficients. I thus focus on the models for respondents within the range of common support. Turning first to what predicts membership in concentrated neighborhood poverty alone, years residing at the Wave I address, being of some race other than white, black, or Hispanic, and, most markedly, being black are all are statistically significant and positively associated with membership in concentrated neighborhood poverty. Examining what predicts attendance at high-poverty schools, family income and academic ability (picture vocabulary test results) are statistically significant and negatively associated with the outcome category. Family welfare receipt, years at the Wave I address, and being black are all positively associated with attendance at a high-poverty high school. Interesting, parental closeness is also positively associated with attending a high-poverty school. Turning lastly to the comparison between respondents in the “control” group and respondents who were exposed to both concentrated neighborhood and school poverty, family income, academic ability, and now residence with two-biological parents at Wave I are all statistically significant and negatively associated with membership in this doubly disadvantaged category. Family welfare receipt and black and "other" racial categories and the only items positively associated with membership in both concentrated neighborhood and school poverty. Notably, the magnitude of the associations between race and the probability of being in a given category is much larger for membership in both of the high-poverty neighborhood categories than for membership in a high-poverty school alone.

The final model set includes the same respondents and covariates as those in the unweighted common support models, but now weighted with the marginal mean through stratification weights. All the covariates which were statistically significant in the previous models are now nonsignificant with substantially smaller effect sizes, indicating that the weighing procedure successfully balances the covariates. Thus, when the weights are applied, the "control" group participants are similar to participants in the concentrated poverty categories on all the observed covariates.

LABOR MARKET OUTCOMES

Table 3 presents OLS regression models of wages (log transformed) and ordinal logistic regression models of workplace autonomy. OLS regression coefficients are shown with accompanying 95% confidence intervals, while ordinal logistic regression results are shown with exponentiated coefficients. Confidence intervals are based cluster robust standard errors to account for clustering of students at Wave I within Add Health primary sampling units. Models proceed in three steps. First, "naïve" models show the association between the concentrated neighborhood poverty categories and the dependent variable referencing individuals who did not reside in a high-poverty neighborhood or attend a high-poverty school in adolescence. These models include controls only for educational attainment and age at Wave IV. Second, regression "adjusted" models are the same as naïve models but now control for all the selection items (coefficients not shown) included in Table 2. These models run the risk of “over controlling” for contextual effects, but also perform best in cases where the outcome and selection models are missspecified (Sampson et al. 2008; Linden et al. 2016). Finally, marginal mean weighting through stratification, or “MMWS,” models are the same as naïve models but are now weighted by the MMWS weight. These models are a cross between the naïve and regression adjusted models in that steps are taken to explicitly account for selection pathways into concentrated poverty categories without also risking washing out pathways through which concentrated poverty may affect the outcome variables (e.g. effects on parental resources).

Turning first to results for the naïve models of wages in adulthood, respondents exposed to both concentrated neighborhood and school poverty in adolescence experience about ((1 – exp(-0.26))*100= -22.9, $p < .001$) 23 percent lower hourly wages than respondents exposed to neither concentrated neighborhood nor school poverty in adolescence. Respondents who attended high-poverty schools also report reduced wages ($b = -.16$, $p < .01$), while concentrated neighborhood poverty is not associated with wages in adulthood. Moving the regression adjusted model, all concentrated poverty coefficients have reduced notably, and with only respondents who attended a high-poverty school reporting statistically significant lower wages (b = -.09, < .05) net of all family controls. Still, the coefficient associated adolescent exposure to concentrated neighborhood and school poverty is similar in magnitude and direction to the coefficient for exposure to only concentrated school poverty.

Last are models for wages adjusted with the marginal mean through stratification weights. Here the independent association for concentrated school poverty has become only marginally significant, while we again observe that adolescent exposure to both high-poverty neighborhoods and schools is associated with reduced wages in adulthood. Specifically, adolescent exposure to concentrated neighborhood and school poverty in adolescence is associated with about ((1 – exp(-0.20))*100= -18, $p < .05$) 18 percent lower self-reported hourly wages in adulthood compared to respondents exposed to neither concentrated neighborhood nor school poverty in adolescence.

Turning attention to ordinal logistic regression models of workplace autonomy, a much consistent story is evident. Across the naïve, regression adjusted, and propensity weighted models, there is a sizable and statistically significant association between adolescent exposure to both concentrated neighborhood and school poverty and workplace autonomy in adulthood. Specifically, these respondents experience about 27 percent lower odds (OR 0.73, $p < .05$) of being in a higher workplace autonomy category compared to adolescents who were not exposed to either high-poverty neighborhoods or schools. Taken together, these results present consistent evidence for a joint effect of adolescent exposure to both neighborhood and school concentrated poverty on adulthood labor market outcomes. Lesser evidence is presented for labor market consequences among respondents who attended high-poverty schools, and no evidence is presented for consequences among respondents who only resided in high-neighborhoods.

DISCUSSION

How do schools shape inequality across generations? Both the school and neighborhood effects literatures illustrate linkages to labor market outcomes by way of educational attainment (Chetty et al. 2014, 2016; Jennings et al. 2015). Thus, whether these effects extend to students who do not go on to college and directly shape labor market outcomes remains an unanswered question. The correspondence perspective on the relationship between schools and work suggests a direct effect of school socioeconomic composition on labor market outcomes among non-college goers (Bowles and Gintis 1976; Anyon 1980). In contrast, studies of the potential for school resources to explain neighborhood effects suggest minimal contributions of school experiences.

In this study, I investigate the labor market consequences of adolescent residence in high-poverty neighborhoods, attendance at high-poverty schools, and joint exposure to both high-poverty neighborhoods and schools among adult respondents who have not attained college credentials. Drawing on novel developments in propensity weighting methods, I find consistent evidence only for a joint association of exposure to both high-poverty neighborhoods *and* schools on wages and workplace autonomy in adulthood. Less consistent evidence is found for an independent association of attendance at a high-poverty school on adulthood wages. I find no evidence for an independent association between adolescent residence in a high-poverty neighborhood and labor market outcomes. Taken together, these results are most consistent with theories of neighborhood effects that empathize the importance of local neighborhood institutions, hypothesizing robust consequences for youth exposed to both resource deprived neighborhoods *and* schools (Wilson 1987; Mayer and Jencks 1989; Jencks and Mayer 1990). Though I find little evidence for the correspondence perspective on the relationship between schools and labor market success, these findings suggest important contributions of schools to conceptualization of concentrated neighborhood poverty. These results are in contrast to studies finding that school resource play only a residual role in explaining neighborhood effects on academic achievement and attainment (Wodtke and Parbst 2017; Levy 2018). Conceptualizing schools as potential mediators of neighborhood effects, these approaches may underestimate the potential for school poverty to be a complementary feature of concentrated neighborhood poverty constructs (Owens and Candipan 2019)

This study is not without limitations. Most notably, I draw on cross-section data at the time point in which concentrated poverty is measured to attempt to capture family selection into neighborhood and school contexts. That is, I forego use of the short-term longitudinal design of Waves I and II of Add Health, opting instead for a substantial increase in sample size by considering Wave I high school seniors. Still, past studies using Add Health report high correlations between contexts experienced in early and late adolescence, suggesting little potential for use of these longitudinal measures to yield new insights for neighborhood and school selection. More important, however, is the lack of available data in the Add Health study on childhood family, neighborhood, and school conditions. The lack of these key unobserved measures further warns against a causal interpretation of the present results. Thus, analyses of other data sources such as the PSID or the Moving to Opportunity data are recommended and may additionally enlighten these results. This study also depends on conceptualizations of neighborhood and school poverty threshold effects, using cutoffs at 40% of census block group residents and students within schools living below the poverty line. Though recent reanalyzes of Moving to Opportunity data offer empirical support for neighborhood poverty threshold effects (Burdick-Will et al. 2011), linear measurement strategies of neighborhood poverty are likely to yield new insights, some of which might explain the present results (Hicks et al. 2018). For example, adolescent residents of extremely high-poverty census tracts may also by default be in relatively high-poverty schools, with a linear operationalization of neighborhood poverty likely to detect effects for such doubly disadvantaged youth. Lastly, I conceptualize neighborhoods as census-based administrative units which, though practical, diverge sharply from residents own neighborhood perceptions and activity paths (Coulton et al. 2001; Hipp and Boessen 2013; Zenk et al. 2019). While it is possible that I find no evidence for an independent association of concentrated neighborhood poverty because of offsetting exposure to low-poverty schools, it might also be the case that these residents are exposed to few disadvantaged areas beyond their immediate neighborhood environment (Hipp 2007).

CONCLUSION

How do neighborhoods and schools inhabited in adolescence shape labor market outcomes in adulthood? Focusing specifically on adults who have not attained college credentials, I find little evidence that independent exposure to concentrated neighborhood or school poverty contribute to labor market outcomes. In contrast, youth exposed to *both* concentrated neighborhood poverty *and* high-poverty schools face detrimental consequences in the labor market, as measured by lower hourly wages and lower non-cognitive skills (Deming 2017). These results are in line with hypotheses of neighborhood effects that emphasize both the structural and institutional components of neighborhoods, and contrast with results focused on educational achievement outcomes (Wodtke and Parbst 2017). This study thus echoes the calls of others to direct attention toward a wider consideration of outcomes which may enlighten our understanding of how neighborhoods and schools contribute to inequality (Jennings et al. 2015).

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Table 1. Descriptive Statistics.

	Treatment		Treatment (common)		ln(Wages)		Workplace Autonomy		
	Mean	*SD*	Mean	*SD*	Mean	*SD*	Mean	*SD*	Range
Dependent Variable									
ln(Wages)	-	-	-	-	2.46	*0.01*	-	-	0.1-4.6
Workplace Autonomy	-	-	-	-	-	-	2.85	*0.02*	0-4
Concentrated poverty categories (Wave I)									
Neither concentrated neighborhood nor school poverty	0.74	-	0.69	-	0.70	-	0.69	-	0-1
Concentrated neighborhood poverty	0.03	-	0.03	-	0.03	-	0.03	-	0-1
Concentrated school poverty	0.18	-	0.21	-	0.21	-	0.21	-	0-1
Concentrated neighborhood and school poverty	0.05	-	0.06	-	0.06	-	0.06	-	0-1
Selection controls (Wave I)									
Family socioeconomic status	-0.55	*0.04*	-0.90	*0.04*	-0.89	*0.04*	-0.89	*0.04*	0-9
ln(Family income)	-0.03	*0.02*	-0.21	*0.02*	-0.21	*0.02*	-0.21	*0.02*	-4.22-4.4
Family welfare receipt	0.11	-	0.14	-	0.14	-	0.14	-	0-1
Parental educational involvement	1.15	*0.01*	1.11	*0.01*	1.10	*0.02*	1.11	*0.01*	0-3
Parental closeness	3.23	*0.01*	3.23	*0.01*	3.24	*0.01*	3.23	*0.01*	0-4
Parental college aspirations	1.18	*0.01*	1.20	*0.01*	1.20	*0.01*	1.20	*0.01*	0-2
Picture vocabulary test score	-2.85	*0.20*	-4.31	*0.21*	-4.06	*0.22*	-4.21	*0.21*	-87-31
Years at current residence	7.02	*0.08*	7.18	*0.10*	7.19	*0.10*	7.18	*0.10*	0-20
ln(County density)	-1.89	*0.02*	-1.86	*0.03*	-1.86	*0.03*	-1.88	*0.03*	-6.68-3
Female	0.47	-	0.49	-	0.49	-	0.49	-	0-1
White	0.50	-	0.47	-	0.47	-	0.47	-	0-1
Black	0.22	-	0.28	-	0.28	-	0.28	-	0-1
Hispanic	0.20	-	0.22	-	0.23	-	0.22	-	0-1
Other race	0.05	-	0.05	-	0.05	-	0.05	-	0-1
Foreign born	0.09	-	0.05	-	0.05	-	0.05	-	0-1
Two-biological parents	0.48	-	0.41	-	0.41	-	0.41	-	0-1
Age (Wave I)	16.34	*0.02*	16.43	*0.02*	16.45	*0.02*	16.43	*0.02*	12-21
Middle schooler	0.03	-	-	-	-	-	-	-	0-1
Wave IV controls									
Educational Attainment									
<High school	0.10	-	0.11	-	0.11	*0.01*	0.11	-	0-1
High school degree	0.28	-	0.29	-	0.29	*0.01*	0.29	-	0-1
Some vocational training	0.06	-	0.06	-	0.06	*0.00*	0.06	-	0-1
Vocational degree	0.20	-	0.20	-	0.20	*0.01*	0.20	-	0-1
Some college	0.36	-	0.34	-	0.34	*0.01*	0.34	-	0-1
Age (Wave IV)	29.25	*0.02*	29.34	*0.02*	29.35	*0.02*	29.34	*0.02*	24-34
N	4,615		3,479		3,289		3,479		

Notes: Missing data handled using multiple imputation by chained equations with 20 imputed data sets. Descriptive statistics in column set 1 are for all respondents used to generate probabilities of membership in each of the treatment and control categories (see Table 2). Descriptive statistics in Column set 2 reduce the sample to respondents within the range of common support across the probabilities of membership in each of the treatment and control categories. Column sets 3 and 4 are constrained to respondents within the range of common support and who are not missing on the respective dependent variable.

Table 2. Multinomial Logistic Regression Models of Membership in Concentrated Neighborhood and School Poverty Categories (vs. Control Category)

		Unweighted (All)			Unweighted (Common Support)			MMWS Weighted		
	Neither Concentrated neighborhood nor school poverty (ref.)	Concentrated neighborhood poverty	Concentrated school poverty	Concentrated neighborhood and school poverty	Concentrated neighborhood poverty	Concentrated school poverty	Concentrated neighborhood and school poverty	Concentrated neighborhood poverty	Concentrated school poverty	Concentrated neighborhood and school poverty
Family socioeconomic status		0.93	0.98	1.01	0.94	0.99	1.02	0.90	1.03	0.99
		[0.82,1.05]	[0.93,1.04]	[0.91,1.12]	[0.83,1.06]	[0.94,1.05]	[0.92,1.14]	[0.70,1.16]	[0.97,1.08]	[0.85,1.16]
ln(Family income)		0.63***	0.65***	0.57***	0.59***	0.65***	0.52***	0.85	0.89	0.89
		[0.50,0.79]	[0.55,0.76]	[0.45,0.72]	[0.46,0.75]	[0.56,0.77]	[0.41,0.67]	[0.57,1.28]	[0.75,1.05]	[0.62,1.29]
Family welfare receipt		1.53	1.44*	2.17***	1.38	1.43*	1.95**	0.75	1.39	2.22
		[0.82,2.85]	[1.02,2.04]	[1.39,3.38]	[0.73,2.59]	[1.01,2.04]	[1.21,3.14]	[0.43,1.34]	[0.98,1.98]	[0.99,4.97]
Parental closeness		1.10	1.24**	1.12	1.08	1.24**	1.19	1.81	1.03	1.19
		[0.88,1.38]	[1.09,1.42]	[0.82,1.54]	[0.83,1.40]	[1.07,1.44]	[0.85,1.67]	[0.96,3.44]	[0.88,1.20]	[0.60,2.37]
Parental educational involvement		0.87	0.93	0.84	0.88	0.95	0.83	0.86	1.02	0.98
		[0.65,1.17]	[0.82,1.05]	[0.71,1.01]	[0.67,1.17]	[0.84,1.07]	[0.69,1.01]	[0.60,1.25]	[0.90,1.17]	[0.74,1.29]
Parental college aspirations		1.27	1.17	1.19	1.17	1.15	1.18	0.82	0.98	1.06
		[0.82,1.97]	[1.00,1.36]	[0.93,1.54]	[0.77,1.77]	[0.97,1.36]	[0.91,1.53]	[0.56,1.20]	[0.83,1.16]	[0.69,1.63]
Picture vocabulary test score		1.00	0.99*	0.97***	0.99	0.99**	0.98***	1.00	1.00	1.00
		[0.98,1.01]	[0.98,1.00]	[0.96,0.99]	[0.98,1.01]	[0.98,1.00]	[0.96,0.99]	[0.97,1.03]	[0.99,1.01]	[0.98,1.03]
Years at current residence		1.03	1.03**	1.03	1.03*	1.03*	1.04	0.95	1.00	1.04
		[0.99,1.06]	[1.01,1.05]	[0.99,1.07]	[1.00,1.07]	[1.00,1.05]	[0.99,1.08]	[0.85,1.07]	[0.98,1.02]	[0.98,1.11]
ln(County density)		1.00	1.04	0.74	0.99	1.03	0.75	1.03	1.05	0.88
		[0.72,1.38]	[0.66,1.63]	[0.43,1.29]	[0.72,1.37]	[0.65,1.61]	[0.44,1.29]	[0.73,1.45]	[0.68,1.64]	[0.46,1.70]
Female		0.88	1.04	1.10	0.83	1.03	1.11	1.26	0.91	1.55*
		[0.65,1.20]	[0.90,1.21]	[0.82,1.48]	[0.61,1.13]	[0.88,1.21]	[0.83,1.48]	[0.62,2.56]	[0.77,1.09]	[1.04,2.31]
Black		17.98***	3.57*	11.27***	16.46***	3.08*	10.08***	1.20	0.90	0.90
		[7.15,45.19]	[1.24,10.30]	[3.25,39.09]	[6.60,41.10]	[1.04,9.12]	[2.83,35.96]	[0.44,3.23]	[0.29,2.73]	[0.25,3.30]
Hispanic		3.43	0.62	2.12	3.51	0.53	1.73	0.81	0.78	1.00
		[0.85,13.90]	[0.18,2.06]	[0.46,9.65]	[0.78,15.79]	[0.16,1.82]	[0.37,8.10]	[0.16,3.94]	[0.24,2.56]	[0.17,5.80]
Other race		10.75**	1.78	11.88**	7.56*	1.63	11.71*	0.65	0.84	0.99
		[2.45,47.28]	[0.65,4.84]	[1.95,72.22]	[1.15,49.84]	[0.68,3.93]	[1.78,77.13]	[0.09,4.69]	[0.33,2.11]	[0.11,8.58]
Foreign born		2.64*	0.62	0.25*	2.16	0.47	0.31	1.08	0.83	1.36
		[1.14,6.12]	[0.26,1.45]	[0.08,0.79]	[0.76,6.16]	[0.17,1.30]	[0.09,1.04]	[0.39,2.96]	[0.29,2.36]	[0.44,4.27]
Two-biological parents		0.86	0.90	0.64*	0.82	0.92	0.69*	1.58	1.02	0.98
		[0.42,1.77]	[0.69,1.17]	[0.46,0.90]	[0.41,1.66]	[0.69,1.22]	[0.47,1.00]	[0.48,5.15]	[0.77,1.35]	[0.62,1.54]
Age (Wave I)		1.01	0.96	1.01	0.98	0.94	1.00	0.86*	0.92	1.03
		[0.88,1.17]	[0.87,1.07]	[0.91,1.12]	[0.85,1.13]	[0.85,1.04]	[0.90,1.11]	[0.75,0.98]	[0.83,1.02]	[0.92,1.15]
Middle schooler (Wave I)		0.00***	1.93	1.14	-	-	-	-	-	-
		[0.00,0.00]	[0.61,6.06]	[0.29,4.40]						
N		4,615			3,479			3,479		

* p<0.05, ** p<0.01, *** p<0.001

Notes: Exponentiated coefficients; 95% confidence intervals from cluster robust standard errors in brackets. Intercepts not shown.

Table 3. Linear Regression Models of ln(Wages) and Ordinal Logistic Regression Models of Workplace Autonomy.

	ln(Wages) (Linear)			Workplace Autonomy (Ordinal)		
	Naïve	Adjusted	MMWS	Naïve	Adjusted	MMWS
	b	*b*	*b*	exp(*b*)	exp(*b*)	exp(*b*)
Treatment categories (Wave I)						
Neither concentrated neighborhood nor school poverty	-	-	-	-	-	-
Concentrated neighborhood poverty	-0.12	-0.07	-0.17	0.79	0.85	1.11
	[-0.35,0.11]	[-0.25,0.11]	[-0.43,0.09]	[0.56,1.10]	[0.60,1.20]	[0.55,2.23]
Concentrated school poverty	-0.16**	-0.09*	-0.09+	0.94	0.96	0.96
	[-0.27,-0.05]	[-0.19,-0.00]	[-0.20,0.01]	[0.79,1.13]	[0.79,1.17]	[0.81,1.15]
Concentrated neighborhood and school poverty	-0.26***	-0.09	-0.20*	0.71***	0.70***	0.73*
	[-0.39,-0.13]	[-0.20,0.03]	[-0.36,-0.04]	[0.57,0.87]	[0.56,0.86]	[0.58,0.93]
Wave IV controls						
Educational Attainment						
<High school (ref.)	-	-	-	-	-	-
High school degree	0.19**	0.21***	0.15**	1.11	1.08	1.08
	[0.07,0.30]	[0.11,0.32]	[0.04,0.25]	[0.88,1.39]	[0.86,1.37]	[0.83,1.40]
Some vocational training	0.30***	0.34***	0.32***	1.32+	1.29	1.35+
	[0.14,0.47]	[0.18,0.51]	[0.16,0.49]	[0.97,1.79]	[0.92,1.81]	[0.96,1.92]
Vocational degree	0.36***	0.45***	0.37***	1.76***	1.80***	1.62***
	[0.23,0.50]	[0.31,0.58]	[0.24,0.50]	[1.33,2.33]	[1.36,2.39]	[1.22,2.15]
Some college	0.37***	0.43***	0.34***	1.24+	1.22+	1.27*
	[0.27,0.48]	[0.32,0.53]	[0.23,0.45]	[0.98,1.56]	[0.97,1.55]	[1.01,1.60]
Age (Wave IV)	0.03*	0.07*	0.03*	0.96+	1.03	0.95+
	[0.00,0.05]	[0.01,0.12]	[0.00,0.05]	[0.92,1.00]	[0.89,1.19]	[0.89,1.00]
Constant	1.43***	1.24***	1.43***	-	-	-
	[0.75,2.11]	[0.49,1.98]	[0.73,2.13]			
Cut 1	-	-	-	0.04***	0.15+	0.03***
				[0.01,0.14]	[0.02,1.20]	[0.01,0.15]
Cut 2	-	-	-	0.19*	0.81	0.14*
				[0.05,0.72]	[0.10,6.42]	[0.02,0.77]
Cut 3	-	-	-	0.73	3.14	0.53
				[0.19,2.76]	[0.39,25.03]	[0.09,2.96]
N	3,289	3,289	3,289	3,479	3,479	3,479

* p<0.05, ** p<0.01, *** p<0.001

Notes: Exponentiated coefficients; 95% confidence intervals from cluster robust standard errors in brackets. Regression adjusted models control for all variables displayed in Table 2.