The Effect of Socioeconomic Rank on Psychosocial Health and Educational Attainment

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Abstract

Research on socioeconomic inequality has most often focused on psychosocial processes of status differentiation and resource allocation at the macro level. Less attention, however, has been paid to hierarchies derived among peers in local contexts. Using data from Add Health, I examine the effect of socioeconomic rank among adolescent schoolmates on psychosocial health and educational attainment. Addressing common limitations of past work, I exploit quasi-random variation across cohorts within schools to mitigate selection bias and isolate the effect of ordinal rank itself from peer composition, the magnitude of social distance, and absolute status. I find that higher rank is consistently associated with reduced depression and truancy, increased school attachment, and increased long-term educational attainment. These findings support an integration of the relative material deprivation framework with academic "frog-pond" models of peer comparison and competition. More broadly, these results highlight the salience of local hierarchies in producing durable advantage and disadvantage.

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I. Introduction

A large body of work across sociology and demography argues that relative socioeconomic position is associated with wellbeing, behavior, and attainment above and beyond absolute status. To explain this relationship, social scientists often implicate variations of the psychosocial hypothesis, which suggests the corrosive potential of lower rank and upward comparisons in a social hierarchy. This perspective is most frequently applied at the macro level, particularly in explaining the negative cross-national health-inequality gradient (Pickett and Wilkinson 2015; Wilkinson and Pickett 2009). At this scale, the emphasis is primarily on diffuse measures of inequality such as the Gini coefficient, in which case the measure is assigned equally to all individuals within aggregates such as states or nations, and it is assumed status differentiation, comparison, and competition are most relevant at these levels. In considering the psychosocial processes and effects of stratification, however, this level of analysis appears incomplete – an emerging, interdisciplinary body of work suggests that social comparisons are instead most salient in *local* contexts, among immediate schoolmates, coworkers, and other peers.

Research on socioeconomic inequality usually ascribes *relative deprivation* as a cornerstone of the psychosocial framework. Relative deprivation has been flexibly defined and applied across multiple domains and levels of scale. Often, it is invoked as a latent theoretical mechanism linking inequality and population health or used as a general stand-in for any negative self-appraisals and social strain induced by low status. Similarly, inequality itself has been inferred as a proxy for the degree of relative deprivation, as average deprivation at the societal level increases along with the divide between socioeconomic classes. It is important to emphasize, however, that economic inequality is a *structural* measure, again assigned equally to each individual within the area of interest, while relative deprivation is an *individual* measure (Eibner and Evans 2005). Thus, there is significant variation in levels of relative deprivation for individuals in a society with the same level of inequality, and it is important to avoid conflating the two concepts.

Formally, relative deprivation is most commonly represented by variations of the Yitzhaki index, which is an explicitly individual-level measure of socioeconomic rank and distance from peers within a defined reference group (Yitzhaki 1979). An individual's degree of relative deprivation, then, is contingent on both her *ordinal* position (i.e., simply being higher or lower in the distribution) and her *cardinal* position (i.e., the magnitude of difference between her status and that of her reference group). Reference group status is typically defined as a simple average; many studies employing the Yitzhaki index rely on ecological inference to define such groups, assuming the average socioeconomic status within a particular geographic area is a salient point of comparison that individuals can intuit (Eibner and Evans 2005).

Despite the widespread appeals to relative deprivation and associated psychosocial processes, findings have been decidedly mixed. Such ambiguity has been ascribed to numerous issues, including selection bias, reverse causality, incomplete conceptualizations of status, and the choice of model specification for relative position (Adjaye-Gbewonyo and Kawachi 2012; Balsa et al. 2014; Eibner and Evans 2005; Hounkpatin et al. 2016; Reagan et al. 2007). Defining appropriate reference groups has also proved especially challenging (Mangyo and Park 2011).

In contrast to such work on economic inequality incorporating the psychosocial perspective, a growing body of research on peer effects indicates that competition and status comparison processes are locally salient. This is the crux of "frog-pond" or "invidious comparison" hypotheses of academic peer effects, which suggest that the presence of higher achieving school peers may precipitate adverse consequences such as negative self-appraisal, social withdrawal, and even diminished long-run academic performance and investment (Alicke et al. 2009; Crosnoe 2009; Davis 1966; Espenshade et al. 2005; Hoxby and Weingarth 2005). These perspectives are similar to the big-fish-little-pond effect (BFLPE), which has been observed across a variety of different contexts and populations, although the BFLPE is typically more narrowly focused on academic self-concept (Marsh et al. 2008). Work in experimental psychology, furthermore, has lent support to a "local dominance effect," which indicates that self-evaluations of performance are most strongly influenced by immediate school peers, even when objective information of standing in the broader population is available (Zell and Alicke 2010). Finally, several studies indicate that *ordinal* ability rank among school peers is significant in its own right, with higher rank associated with increased educational attainment, reduced engagement in risky behaviors, and increased academic achievement (Elsner and Isphording 2017 & 2018; Murphy and Weinhardt 2018).

Importantly, significant methodological advances have also been made in the school peer effects literature that enable viable causal inference – a large number of studies implement natural experiments or exploit plausibly idiosyncratic variation across groups (e.g., Hoxby 2005; Sacerdote 2011; Sacerdote 2014). By focusing on school peers, moreover, this work also consistently defines explicit, locally salient reference groups. Thus far, however, this work has almost exclusively considered standing among school peers in relation to *academic* characteristics. Despite the similar emphasis on psychosocial mechanisms, relative *socioeconomic* standing has rarely been considered among school peers while also taking advantage of the methodological tools offered by the school environment (but see Balsa et al. 2014).

In this study, I aim to address this gap, integrating perspectives on relative deprivation, status feedback, and academic peer effects by examining the influence of ordinal socioeconomic rank among adolescent schoolmates on several important outcomes. I first test the effect of rank on three representative indicators of psychosocial health: depressive symptoms, perceived school attachment, and truancy. These are intended to sample the range of social, mental, and behavioral elements that psychosocial health encompasses, and that lower relative status has been hypothesized to effect via negative self-evaluation, marginalization, and so forth (e.g., Kearns et al. 2013; Hounkpatin et al. 2015). I expect higher socioeconomic rank to protect against depression and truancy and to increase attachment.

Moving beyond immediate outcomes, I also examine the influence of rank on long-term educational attainment, motivated by the “differential returns” perspective suggested as a potential driver of inequality in the sociology of education (Jennings et al. 2015). Within the school environment, there are intuitively obvious potential benefits from exposure to more economically advantaged peers, including access to social, material, academic, and even cultural resources that promote learning and success (Ackert 2018; Legewie and DiPrete 2012). This intuition, however, is complicated by the psychosocial processes described above. In one of the few studies to extend the frog-pond framework to the socioeconomic domain, for instance, Crosnoe (2009) demonstrates that low-income students’ academic progress may be hindered by higher proportions of advantaged peers.

To my knowledge, however, no work has addressed the effect of local socioeconomic rank itself on long-term educational outcomes. In essence, lower rank within a school, and the corresponding value reductions in social, cultural, and other forms of capital, may restrict a student’s ability to effectively navigate the school environment and compete with more advantaged peers for the resources that encourage academic achievement. Because such resources are necessarily limited, a family’s mere ordinal position in the local distribution might effectively alter the marginal cost of academic investment, thereby generating significant variation in access and subsequent advantage or disadvantage.

The present study extends previous work in several other ways as well. As discussed, much research in this area is prone to confounding due to selection or omitted variable bias, particularly when comparing across broad aggregates. In order to overcome these issues, I use a causal identification strategy developed in the academic peer effects literature that has rarely been incorporated into studies of relative deprivation and socioeconomic inequality. The key identification assumption, as originally developed by Hoxby (2000), is that the distribution of peers within a given grade within a school is effectively random conditional on school-level variation. The idea is that there is some degree of idiosyncratic, essentially unpredictable fluctuation in student characteristics from cohort to cohort within a school on the basis of student birthdate and school entry cutoffs. Leveraging such variation using school and grade fixed effects hedges against bias introduced by systematic self-selection into schools by families along socioeconomic and other lines. A student's rank in a given year, then, is assumed to depend in part on the random component of her cohort's socioeconomic composition. In order to identify a "pure" rank effect that lends itself to causal interpretation, I also employ a school *by* grade fixed effects model that separates rank from average peer effects.

By focusing on grade-level school peers, moreover, I identify an explicit, local reference group within which competition and comparative processes are particularly salient. Unlike most work on relative socioeconomic status, I am able to capture the actual distribution within which individuals are embedded, as opposed to inferring relative status from group or geographic averages. Detailed individual-level data also allows me to construct a more comprehensive socioeconomic index, incorporating both parental education and income. The use of parental measures of status, furthermore, helps address issues of reverse causality, a common source of concern in research relating health and economic inequality.

Finally, I focus on an oft-overlooked specification of relative socioeconomic status: ordinal rank. The rank and distance between individuals in a distribution are often conflated under the purview of deprivation, and both are incorporated in the standard Yitzhaki index. Nonetheless, these are distinct components that might vary in importance depending on the context and outcome in question (Eibner and Evans 2005). Indeed, some evidence suggests that rank may be more appropriate than the Yitzhaki index when measuring the influence of smaller-scale distributions on psychological health (Hounkpatin et al. 2016). Relatively little work has considered the role of ordinal rank while simultaneously accounting for cardinal differences and contextual bias, such as that induced by differences in average peer composition.

Overall, I find that higher rank within a school cohort has a robust, positive influence on psychosocial health and educational attainment. These results highlight that ordinal socioeconomic position within a local reference group matters for important short- and long-term outcomes, complementing recent work finding effects of relative ability and achievement among school peers (Elsner and Isphording 2017 & 2018; Murphy and Weinhardt 2018). This study also suggests that relative deprivation does not operate exclusively through comparisons across broad aggregates, and indicates that ordinal position is influential in its own right, as distinct from absolute differences in socioeconomic resources and group composition.

II. Relative Socioeconomic Status and Peer Effects

The crux of relative deprivation theory is that individuals are negatively influenced by upward social comparisons via the perceived or realized inability to maintain the standards set by a salient frame of reference. Relative deprivation may operate through psychosocial pathways, such as stress and negative self-perceptions, but also material pathways, if lower relative standing restricts access to valued resources. As formalized by Runciman, an individual is relatively deprived if she (1) does not possess a particular good, (2) sees others who possess that good, (3) desires that good, and (4) believes that it is feasible for her to obtain that good (Balsa et al. 2014). This concept has since been expanded to apply to a wide array of sociocultural, material, and other "goods" that might comprise status, which the original definition did not preclude, although the focus has remained on relative income specifically.

At the macro level, relative deprivation is often not measured explicitly. Rather, it is inferred as an inevitable spill-over consequence of societal inequality that degrades population wellbeing. Wilkinson in particular has made the case that relative deprivation is in part responsible for numerous features of societal strain, poor health, and dysfunction (Wilkinson and Pickett 2007). The consistency of the cross-national gradient in inequality and population wellbeing is certainly informative in its own right. Significant methodological challenges, however, hinder causal inference in these cases – in particular, associations in such heterogeneous contexts are especially prone to confounding due to selection, omitted variable, or aggregation bias (e.g., Deaton 2001). At these scales, furthermore, it is challenging to adequately separate the effect of *absolute* differences in socioeconomic resources from *relative* status specifically. Indeed, in several studies that more carefully account for absolute status, population heterogeneity, and other potential confounders, the observed effects of structural inequality are attenuated or reduced to statistical insignificance (Beckfield 2004; Mellor and Milyo 2001; Milyo and Mellor 1999). It is important to reiterate that individual relative deprivation should not be conflated with structural inequality, despite the temptation for this ecological inference.

Other studies have nonetheless measured relative deprivation explicitly and within comparatively smaller geographic contexts. Eibner and Evans (2005), for example, construct reference groups by matching individuals on important demographic characteristics, and find using state fixed effects that relative income deprivation within these groups, measured by the Yitzhaki index, is positively associated with mortality and various measures of morbidity and risky health behaviors. Although there is some promising evidence that relative deprivation is related to health outcomes in particular, overall the findings have been mixed (see Adjaye-Gbewonyo and Kawachi 2012 for a review of work relating the Yitzhaki index and population health). In one review of the "causal evidence" for the relative deprivation perspective on income inequality and health, Pickett and Wilkinson (2015) even go so far as to question the relevance of psychosocial processes at the local level.

Some studies of local contexts, including schools and neighborhoods, suggest the possibility of mechanisms similar to relative deprivation without measuring it explicitly. Work by Odgers and colleagues using nationally representative data on British families, for instance, suggests that greater neighborhood affluence may precipitate risk for antisocial behavior among low-income boys (Odgers 2015; Odgers et al. 2015). Odgers argues that this "shadow of wealth" may produce a form of double-disadvantage for poor children by sharpening the actual feeling of being poor, relating to a broader body of work on subjective social status and so-called "status syndrome" (e.g., Kawachi et al. 2010; Schnittker and McLeod 2005). Similarly, in the Moving to Opportunity (MTO) study, which experimentally assigned housing vouchers to low-income families to encourage relocation to more advantaged neighborhoods, boys in the treatment group initially exhibited more antisocial behavior following relocation. Such negative outcomes in this case, however, appear to have been relatively transient (Sciandra et al. 2013). Considering aggregate social processes, research in the social capital tradition similarly suggests that high within-neighborhood inequality may erode trust and social cohesion (e.g., Browning et al. 2017; Luttmer 2005).

Turning to the school level, Crosnoe (2009) draws on the frog pond framework and finds that low-income students in more affluent high schools may be at risk for diminished psychosocial health and academic achievement outcomes. Ackert (2018) invokes the relative deprivation perspective explicitly, but does not find evidence of differential associations of mean school SES by socioeconomic group as it relates to academic engagement. The evidence from these studies, then, is similarly mixed, although both are only able to adjust for observable confounders.

The inconsistencies in the research discussed thus far, however, may be largely due to methodological choices and limitations. As Balsa and colleagues (2014) argue, most studies of relative deprivation have failed to either 1) identify an appropriate reference group, or 2) adequately account for selection bias and other sources of confounding. On the first issue, much work employs the previously discussed methods of demographic matching, geographic averages, or both to define reference groups, as in Eibner and Evans (2005). Demographic matching, however, particularly along class lines, misses important sources of status difference, and still fails to capture the distributions within which individuals are actually embedded. Perhaps more importantly, many of these studies rely on ecological inference to define reference groups, assuming the average socioeconomic status within a particular geographic area is a salient point of comparison that individuals can intuit.

In order to address such issues, one strategy is to instead look to immediate peers and leverage advances from quasi-experimental work on peer effects, especially in the academic context. In particular, numerous studies have now taken advantage of variation in cohort composition within schools, under the assumption that conditional on the school-level distribution of the outcome of interest, such differences are quasi-random on the basis of student birthdate and entry cutoffs. The bulk of this work has focused almost exclusively on variation in peer ability and individual achievement outcomes. Several studies demonstrate positive effects of average peer ability, but research increasingly seems to suggest that these effects are non-linear, and that the classic "linear-in-means" model may be insufficient to capture the nuanced dynamics at play (see Sacerdote 2014 for a review). Antecol et al. (2016), for example, suggest that average peer achievement may actually have a negative effect on individual achievement in certain contexts.

Employing a regression discontinuity design, Pop-Eleches and Urquiola (2013) similarly find that students at the lower margins in high-achieving schools internalize their diminished status and feel marginalized by peers.

Hoxby and Weingarth (2005) introduce several possible stylized classifications of non-linear peer effects, in which students are differentially influenced by schoolmates depending on their own background or distributional position. Most relevant here, perhaps, is the so-called "invidious comparison" model, in which individual outcomes suffer from the presence of better-achieving peers. Although originally applied to peer ability, the connection to socioeconomic rank is clear.

Indeed, studies are increasingly extending the peer effects framework to other outcomes and contextual features. Black et al. (2013), for instance, exploit quasi-random differences in school cohort composition to examine the effect of the percentage of female grade-mates on a range of long term outcomes, including educational attainment. As another example more relevant to the present study, Bifulco et al. (2011) use a similar within school/across cohort design to find that a higher proportion of classmates with college-educated mothers increases individual educational attainment. Balsa et al. (2014) is the only study I am aware of that exploits the within-school/across cohort design to assess the effect of relative deprivation specifically. Also using data from Add Health and a school fixed-effects model, the authors find that higher relative deprivation among grade-level school peers, as measured by the Yitzhaki index, is associated with increased frequency of engagement in several risky health behaviors among adolescent boys, but not girls.

The Balsa et al. (2014) study takes significant steps towards addressing common methodological concerns, but it still has multiple limitations. First, the authors use a relatively crude measure of socioeconomic status in student-reported (in-school survey) parental education. In much work that considers school peer demographic characteristics, in fact, SES is often captured with imprecise measures such as dichotomized free lunch eligibility (van Ewijk and Sleegers 2010). Furthermore, the authors do not account for unobserved school-grade specific confounders, such as dynamic selection, systematic differences in peer composition, or shocks across grades within schools. Most notably, however, this study cannot separate the effect of relative status itself from the effect of the average socioeconomic composition of a student's peers.

A more appropriate measure of relative socioeconomic status among grade-mates may be ordinal rank itself. Much of the experimental work examining the effects of hierarchy within nonhuman primate groups, for instance, actually measures ordinal rank among primate "peers" as opposed to relative deprivation per se. Such studies have found several ill-effects of subordination in resource-based hierarchies, including increased stress-reactivity and social withdrawal (Sapolsky 2004; Sapolsky 2005; Zizzo 2002).

It is likely that some of the social, psychological, and physiological effects of rank and subordination carry over into human populations. In work on wage differences among peers, for example, findings have supported a form of Parducci's "Range Frequency Theory," suggesting that ordinal position within an organizational salary structure is more important for wellbeing and job satisfaction than a mean reference point (Boyce et a. 2010; Brown et al. 2008; Card et al. 2012; Clark et al. 2010). Indeed, it is unclear if those farther away in the socioeconomic distribution actually should be weighted more, as the Yitzhaki index assumes, because it is possible that more proximate peers are more salient competitors (e.g., Festinger 1954). Other evidence also suggests that ordinal rank may be more influential than the Yitzhaki Index for psychosocial health and forms of psychopathology, including depression (Hounkpatin et al. 2016). It has been suggested this may partly be due to cognitive constraints, if most relevant information can be inferred from mere rank position without the additional demanding task of keeping track of the magnitude of differences (Hounkpatin et al. 2016).

At the school level, a series of causally well-identified studies have found that higher ordinal rank among grade-level peers in terms of ability increases educational attainment, reduces delinquency, and increases academic achievement (Elsner and Isphording 2017; Elsner and Isphording 2018; Murphy and Weinhardt 2018). This specification of relative position, however, has not been applied to socioeconomic status within schools, despite the theoretical support and methodological advantages it offers. Because the present study exploits variation within schools, socioeconomic distance between peers is inevitably compressed, and thus it may be more appropriate to use a measure that does not weight social distance heavily. It is worth reiterating that the effect of ordinal socioeconomic rank is distinct from the effect of average peer composition as well. Indeed, one of the goals here is to isolate the former from the latter.

Overall, previous findings in the relative deprivation and academic peer effects literatures provide only a murky window into the competing costs and benefits of peer socioeconomic advantage. It remains unclear if findings suggested from the most causally well-identified studies for *ability* peer effects maps on to peer *socioeconomic* characteristics in a meaningful way. Issues of selection and contextual bias, reference group identification, socioeconomic scale construction, and choice of model for relative status all remain. A relatively narrow range of outcomes has also been considered.

In the present study, I choose three representative psychosocial outcomes suggested to be influenced by relative status – depression, to capture mental health/psychopathology; truancy, to capture an "objective" measure of delinquency and antisocial behavior; and perceived school attachment, to capture subjective feelings of social integration and belonging. Extending past work further, I also examine the effect of relative status on educational attainment, as academic resources represent particularly salient "goods" that students and families compete for, with potentially differential ability to do so on the basis of ordinal socioeconomic rank.

III. Data and Measures

Add Health

The National Longitudinal Study of Adolescent to Adult Health (Add Health) follows a nationally representative sample of US adolescents who were in grades 7-12 during the 1994-1995 school year. In the first stage of the sampling procedure, a stratified random sample of 88 high schools was drawn from the frame of all US high schools with more than 30 students. For each high school that did not have a 7th grade, a "feeder" middle or junior high school was selected with probability proportional to its student contribution to the high school, yielding a total of 132 schools (including 44 school pairs). An initial "in-school" survey was distributed to the majority of students in each school. Subsequently, random samples of 17 boys and 17 girls were drawn from each grade in each school for wave 1 "in-home" interviews. These include highly detailed information about each respondent's individual, social, and environmental characteristics. For students selected for in-home interviews, a questionnaire was also administered to a designated parent or guardian, with preference given to the resident mother. In this form, each selected parent provided further information about herself, her spouse or partner if applicable, as well as her children. Approximately 85% of children in the wave 1 in-home sample have a completed parental questionnaire. Subsequent waves of interviews following up with the 1994-95 cohort were completed in 1996 (wave 2), 2000-2001 (wave 3), and 2008-2009 (wave 4) (see Harris 2013 for more details on the Add Health study design).

Such design features of Add Health are critical to the empirical strategy employed here. Because Add Health draws from multiple grades within each school, I am able to leverage the plausibly random component between each cohort (i.e., each grade) within a school to identify the effect of rank among a relatively large sampling of immediate peers. Detailed indicators of socioeconomic status, peer characteristics, and demographic factors also enable more precise specifications of rank and can be used to hedge against further potential threats to identification. Few datasets provide the contextual measures, within-school multi-grade design, and longitudinal features necessary to answer the research questions in the present study.[1] Educational attainment here is measured at wave 4, while all other variables are derived from the wave 1 in-home sample and associated parental questionnaires. For the analyses of psychosocial health outcomes, I drop observations with missing data on wave 1 variables only in order to conserve sample size. For educational attainment, I use a separate subset that additionally excludes cases with missing information at wave 4 due to respondent attrition. In order to minimize error in the rank variable, within each subset I also drop school-grades with fewer than 20 observations, and subsequently those schools without at least two cohorts of adequate size to compare. The final psychosocial health sample contains 11,873 students from 110 schools and 338 unique school-grades. The educational attainment sample contains 7,131 students from 77 schools and 221 school-grades. Table 1 provides summary statistics for all variables used; apart from sample size, there appears to be very little compositional differences between the two subsets.

Dependent Variables I: Psychosocial Health

In order to capture several elements of the broadly defined "psychosocial health," I focus on three separate variables: depression, school attachment, and truancy. Each is a continuous, self-reported indicator measured at wave 1.

I measure depression using Add Health's modified version of the Center for Epidemiologic Studies Depression (CES-D) scale. The CES-D scale is a generally well-validated instrument commonly used to assess depressive symptoms among non-psychiatric populations (Hounkpatin et al. 2015; Jacobson and Newman 2014). Add Health contains 19 self-report items from the

[1] The only other potential dataset I am aware of is High School and Beyond (HS&B), a nationally representative study of 10^{th} and 12^{th} graders in 1980, which features four follow-up waves. This is the only longitudinal study administered by NCES to sample multiple grades within each school, which is required for the identification strategy used here.

original 20-item scale. Each is a likert-type question asking respondents to report how frequently they experienced each symptom or feeling over the last week (e.g., "You were hopeful about the future") from 0 ("never or rarely") to 3 ("most of the time or all of the time"). I sum scores across all questions to form a continuous index ranging from 0 to 57. Depressive symptomology within the Add Health population is relatively low, with an 11.2-point average for the sample used here.

I construct perceived school attachment as a composite of three questions asking students how much they agree or disagree with the following: 1) you feel close to people at your school, 2) you feel like you are part of your school, and 3) you are happy to be at your school. Each question is on a likert-scale from 1 (strongly agree) to 5 (strongly disagree). These were reverse coded and summed to form a continuous index ranging from 3-15, with higher scores indicating greater attachment and sense of belonging to the school environment.

Finally, I measure truancy as the number of days each student reported skipping school in the past year without an excuse. Although related to school attachment, this is intended to capture a more behaviorally-oriented component of psychosocial health.

Dependent Variables II: Educational Attainment

I measure educational attainment as education level reported at wave 4. The original index ranges from "8th grade or less" to "completed post baccalaureate professional education," which I convert to total education in years. Even the youngest respondents were in their mid-twenties at the time of wave 4 interviews, so it is plausible the vast majority of the sample had completed their intended education by that time.[2]

Explanatory Variable of Interest: Socioeconomic Rank

I am primarily interested in the effect of socioeconomic rank among grade-mates at wave 1. I construct absolute socioeconomic status as a composite of parental education and income. Similar to student educational attainment, I convert parental education to a continuous measure in years using the midpoint of each category. In order to minimize reporting error, I also substitute

[2] As another strategy to conserve sample size, I also tried complementing the sample with respondents who were missing at wave 4 but reported education at wave 3. The median age at wave 3 is around 22, however, so it is likely that the education of many respondents at this time was still in progress. Nonetheless, results with this alternative sample are highly similar.

parent-reported education when available. I then standardize (z-score) the highest educational level of either parent. Household income for the past year, measured in thousands of dollars, is reported on the parental questionnaire. I log-transform these values, setting the minimum to 0 for those that reported no income, and then standardize. Finally, I average the standardized education and income measures to form a continuous index, which I re-center to have a minimum value of 0.

Using this index, I calculate each student's school-grade level ordinal rank, which I standardize as a percentile to allow comparisons across grades of different size (Murphy and Weinhardt 2018):

$$R_{isg} = \frac{\mathrm{n}_{isg} - 1}{\mathrm{N}_{sg} - 1}, \; R_{isg} \in \{0,1\}$$

In the above, n_{isg} indicates the ordinal (i.e., absolute) rank for individual i in school s and grade g, and N_{sg} indicates the number of students in the individual's particular school-grade. As a percentile, the standardized rank R_{isg} is bounded between 0 and 1. In the rare case of ties, both students are given the higher rank.

The psychosocial framework presented in part rests on the assumption that socioeconomic rank is both meaningful and distinguishable among peers. There are many possible factors to incorporate into this measure, and more research is needed to understand all viable axes of differentiation and the contexts in which they are most salient.[3] Social, cultural, and symbolic capital, for instance, certainly inform students' expectations, valued goals, academic orientations, and engagement with peers and teachers in the school environment. However, such complex features are difficult to capture quantitatively, or even place on an axis from "low" to "high" that transcends different contexts. In terms of relative deprivation, there may be significant variation across groups and social environments in terms of what comprises "status", including its associated valued goods and knowledge, the signaling processes that demonstrate their possession, and how they can be attained (Sweet 2011; Sweeting and Hunt 2015). Thus, in the present study I focus on

[3] Two measures I also considered were neighborhood income and parental occupation. The former is included in robustness checks. I decided against parental occupation, however, as the categories provided in Add Health wave 1 can only be roughly converted to standard continuous indices of occupational prestige, such as the Hauser-Warren index (See Hauser and Warren 1997)

two measures of status that are theoretically well-supported, empirically tractable, and fairly universal in the hierarchical advantages they can be expected to confer (Cowan et al. 2012).

Table 1. Summary Statistics

	A. Psychosocial Health Subset				B. Educational Attainment Subset			
	Mean	SD	Min	Max	Mean	SD	Min	Max
Individual attributes								
Male	0.5	0.5	0	1	0.48	0.5	0	1
Age (months)	191.27	19.68	136	248	190.86	19.27	138	248
Hispanic	0.16	0.37	0	1	0.16	0.37	0	1
Black	0.23	0.42	0	1	0.22	0.42	0	1
Asian	0.06	0.24	0	1	0.06	0.24	0	1
Foreign born	0.08	0.26	0	1	0.07	0.26	0	1
Two-parent family	0.7	0.46	0	1	0.72	0.45	0	1
Household size	4.59	1.57	2	16	4.59	1.55	2	16
Cognitive ability	100.83	14.27	13	139	101.71	13.79	17	138
Parent edu (years)	13.89	3.26	5	20	13.99	3.24	5	20
Parent inc (thousands)	45.39	50.82	0	999	45.83	48.64	0	999
SES composite	3.25	0.83	0	6.06	3.28	0.81	0	6.06
Outcomes								
Depression	11.23	7.55	0	54	11.2	7.52	0	54
School attachment	11.2	2.58	3	15	11.18	2.58	3	15
Truancy	2.2	7.68	0	99	2.1	7.37	0	99
Edu attainment (years)	-	-	-	-	14.44	2.6	7	20
Sample								
Grade size	62.02	76.17	20	333	56.49	61.88	20	249
Grades per school	3.53	1.16	2	6	3.3	1.02	2	6
	N				N			
Students	11,873				7,131			
Schools	110				77			
School-grades	338				221			

Due to the within-school identification strategy, one issue might be that socioeconomic variation is too restricted for local rank to be distinguishable. Figure 1 helps assuage this concern. First, for all students in each decile of the full-sample socioeconomic distribution, it shows the distribution of corresponding ranks at the grade level. Students who rank in the sixth decile in the full sample, for example, rank everywhere from about the 18th to the 87th percentile among school-specific peers. Most relevant to this study, Figure 1 also displays the distribution of grade-level ranks for students in each decile of their corresponding school. Students who rank in the sixth decile among their peers at the school level rank everywhere from below the 40th to above the 70th

percentile among their grade-specific school peers. The within-school standard deviation in rank, conditional on grade and absolute SES, is about 0.11. It is clear that an individual's socioeconomic rank can vary dramatically depending on the level of context and choice of relevant comparison group. This evidence suggests that even students in the same school with equivalent SES may end up at significantly different ranks by dint of the grades and corresponding peer distributions they end up in.

Figure 1. Rank in Grade vs. School and Full Sample

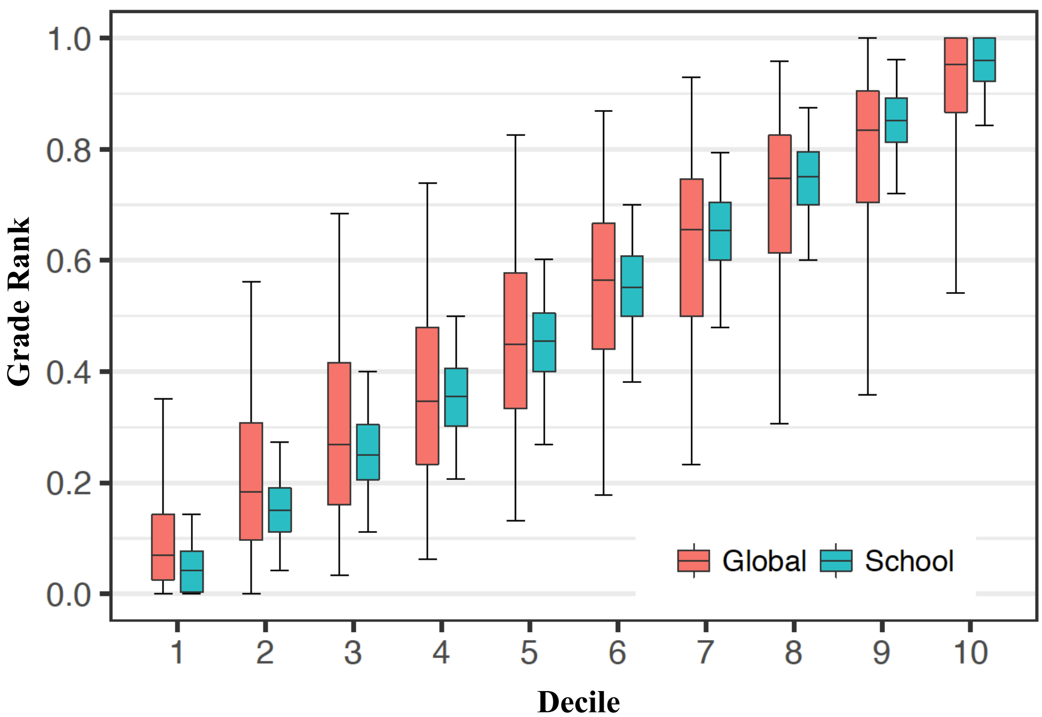

Figure 1: This figure displays the range of grade-level percentile ranks for students in each decile of the global (full sample) and school socioeconomic distributions. The plots indicate the 5th, 25th, 50th, 75th, and 95th percentiles of each grade-level rank distribution for the decile and level indicated.

It appears there is enough variation for students to infer their ranking within a reasonable margin of error. Ordinal rank, in fact, might be easier to intuit than relative deprivation as commonly defined, if the greater cognitive and signaling demands required of students to additionally calculate cardinal differences among themselves are not feasible (Hounkpatin et al. 2015). Some work in experimental psychology, furthermore, suggests that individuals are relatively adept at inferring their "status," broadly defined, but it is unclear if this applies to socioeconomic status per se (Anderson et al. 2006). It is important to emphasize, however, that rank need not operate exclusively through psychosocial processes, particularly in terms of its effect on educational attainment. Again, because academic resources within schools are necessarily limited, a family's mere position in the local distribution may confer greater ability to compete for access, in effect lowering the marginal cost of academic investment.

IV. Analytic Strategy

Identifying Variation

The analytic strategy I use here largely follows that of Elsner and Isphording (2017 & 2018). Whereas they examine the effect of rank in terms of cognitive ability, however, I look at the effect of socioeconomic rank. Identifying variation is derived from differences in grade-level socioeconomic distributions within schools. Again, it is assumed that differences in the distribution of students across grades within a school are quasi-random, and thus each student's particular rank in a cohort has an idiosyncratic component. As discussed by Hoxby (2000), even the most thorough and invested parent cannot determine with precision the distribution of peers her child can expect. In terms of socioeconomic status, even within the population of families selecting into each school, there are random yearly fluctuations in the numbers of children born into more or less advantaged families. Relatedly, a parent cannot simply assign her child to a different birth year – it is possible the parent might delay entry of her child into a particular cohort based on perceptions of that cohort's characteristics, but evidence suggests this is a relatively recent phenomenon, to the extent that it occurs (Balsa et al. 2014). Because I construct rank using data from 1994-1995, cohort-specific selection by parents is unlikely to be a concern.

Variation in student rank can first result from *mean* differences in grade distributions within a school (i.e., if there are more or less advantaged students in the cohort a student happens to end up in). This is demonstrated in figure 2A – holding individual SES constant for student *i*, her rank would be lower if she ended up in Grade B vs. Grade A, due to the higher average peer SES in the former. Leveraging this variation corresponds to a model with separate school and grade fixed effects, ruling out bias induced by static selection into schools.

Beyond differences in means, however, rank can also vary through differences in the *variance* of grade-level socioeconomic distributions within schools. As illustrated in figure 2B, a student's rank would be higher if she ended up in Grade B due to its lower variance, even holding individual SES and the grade-level means constant. This corresponds to a model with school *by* grade fixed effects, which removes mean differences across school-grades but still allows for identification of the rank effect through remaining differences in variance.

Figure 2: Identifying Variation

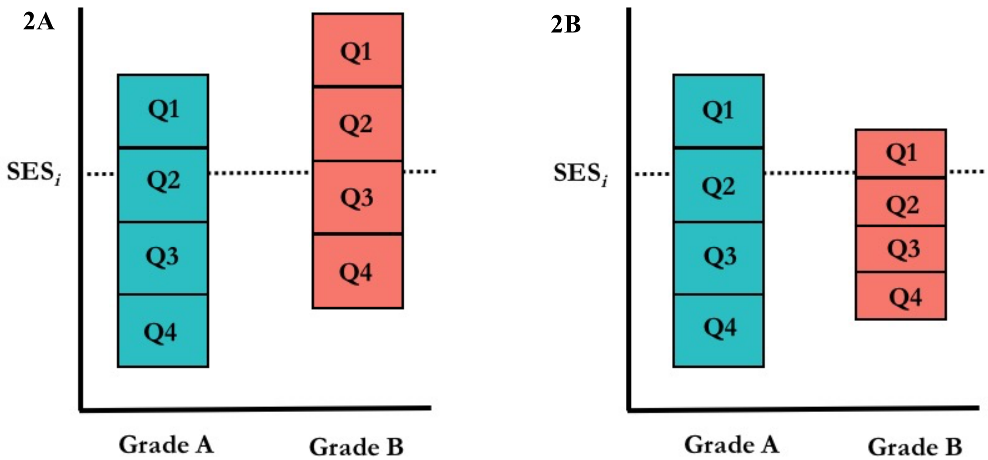

Figure 2: 2A demonstrates that socioeconomic rank can vary as a result of differences in grade-level mean SES, holding individual SES constant. 2B demonstrates that rank can vary as a result of differences in the grade-level variance of SES, even holding grade-level mean and individual SES constant. Boxes represent levels of each grade-level socioeconomic distribution. Figure is used from Elsner and Isphording (2017)

Empirical Models

Model 1: School and Grade Fixed Effects

The first empirical specification is a model with separate school and grade fixed effects:

$$y_{isg} = \beta_0 + \beta_1 R_{isg} + \beta_2 SES_{isg} + X_{isg} \beta_3 + \mathrm{FE}_s + \mathrm{FE}_g + \varepsilon_{isg}$$

A given outcome variable is indicated by y_{isg}, representing either depression, school attachment, truancy, or educational attainment for individual *i* in school *s* and grade *g*. The primary explanatory variable of interest, socioeconomic rank, is measured as within school-grade percentile, and thus bounded between 0 and 1. Along with absolute SES, an additional vector of individual-level controls is represented by X_{isg}. Control variables include age and age squared in months, sex, race or ethnicity (dummies for black, Asian, and Hispanic, with white as the reference category), cognitive ability (continuous indicator measured by Add Health's modified version of the Peabody Picture Vocabulary Test), immigrant status (dummy indicating if born in the US), family structure (dummy for married couple versus other), and a continuous indicator of household size. FE_s, school fixed effects, indicates a set of dummy variables for all schools, and FE_g, grade fixed effects, indicates a set of dummy variables for all grades. Finally, the error term ε_{isg} indicates unobserved determinants of the outcome in question. Standard errors are clustered at the school level for all analyses.

The use of these fixed effects terms eliminates many sources of confounding, particularly bias introduced by self-selection into schools. However, a causal interpretation of the effect of rank in model 1 is hindered by two primary issues. First, it assumes there are no grade-specific effects *within* schools, as the unnested grade fixed effects simply remove average differences and are not school specific. In other words, endogeneity concerns about the effect of rank arise if there are systematic differences across grades within schools that are correlated with both socioeconomic rank and either educational attainment or psychosocial health. Because each grade is observed in a different year, for instance, the model must assume that selection into schools is static. This could easily be violated, however, if resource distribution policies or related factors had changed between entry cohorts within a given school. Furthermore, peers themselves may vary systematically across grades in terms of race and other characteristics that are also associated with socioeconomic status, thus making it difficult to distinguish the effects of changes in peer composition from changes in rank per se (Elsner and Isphording 2017 & 2018).

This relates to the second main threat to causal interpretation: socioeconomic *rank* varies mechanically with mean socioeconomic *composition* (i.e., an individual student's rank will be lower in a cohort with higher average SES, and vice versa). Thus, resulting "rank" effects may be biased by both the direct socioeconomic effects of peers themselves, as well as the indirect effects these peers elicit, such as responses from teachers or involvement in school activities by parents. Subsequent models attempt to address such issues related to dynamic selection, changes in school quality over time, and systematic compositional effects.

Model 2: School and Grade Fixed Effects with School-Grade Compositional Controls

The second specification includes several additional parameters to address some of the endogeneity concerns described above:

$$y_{isg} = \beta_0 + \beta_1 R_{isg} + \beta_2 SES_{isg} + X_{isg}\beta_3 + \boldsymbol{\beta_4} \boldsymbol{\overline{Y}_{-isg}} + \boldsymbol{\beta_5} \boldsymbol{\overline{SES}_{-isg}} + \boldsymbol{\overline{X}_{-isg}} \boldsymbol{\beta_6} + \text{FE}_s + \text{FE}_g + \varepsilon_{isg}$$

First, in order to separate the effect of peer socioeconomic composition from the effect of rank itself, grade-level average (excluding the student in question) is included as a control, as represented by $\beta_5\, \overline{SES}_{-isg}$. In order to account for other unobserved compositional factors that might confound the relationship between rank and each outcome, the peer means of all other controls, $\bar{X}_{-isg}\,\beta_6$, are also included. Finally, because the three psychosocial outcomes are also measured at wave 1, peer mean for each of these variables is included in the corresponding model to separate peer from rank effects. The peer mean for educational attainment is not included in its corresponding model, as this outcome is measured at wave 4.

Model 3: School by Grade Fixed Effects

Although model 2 accounts for several compositional effects, there are many potential unobserved or unknown confounders. In order to try and isolate a "pure" rank effect, model 3 alternatively includes school by grade fixed effects:

$$y_{isg} = \beta_0 + \beta_1 R_{isg} + \beta_2 SES_{isg} + X_{isg}\beta_3 + \mathbf{FE}_{\boldsymbol{s}} \times \mathbf{FE}_{\boldsymbol{g}} + \varepsilon_{isg}$$

This eliminates mean differences in school-grade specific variables, including systematic compositional factors, dynamic selection, and exogenous shocks to specific school-grades. This strategy is adopted from Elsner and Isphording (2017 & 2018), who applied a variation of this model to cognitive ability rank. As they discuss, even after removing all mean differences across cohorts, differences in the shape of each grade distribution remain, because rank varies at the level of the individual student. Thus, this model leverages differences in the variance of the socioeconomic distributions across school-grades, after accounting for observed and unobserved confounders by eliminating mean differences. By focusing on the grade mean-independent effect of rank itself, furthermore, this model hedges against issues related to contextual bias that are rampant in much work on peer effects (Angrist 2014).

V. Results

Table 2 shows the results for each of the models detailed above: school and grade fixed effects (model 1), school and grade fixed effects with compositional controls (model 2), and school by grade fixed effects (model 3). As a further robustness check, an additional model is included with school by grade fixed effects and a squared socioeconomic status term in order to control for potential nonlinearities (model 4). Full model results are provided in the appendix. Because rank is bounded between 0 and 1, the coefficients on their own essentially indicate the effect of moving from the bottom to the top of the distribution. As this is typically not the case, however, the coefficients are more reasonably evaluated at the within-school standard deviation in rank, which is approximately 0.11 across models. Models 3 and 4 are the preferred specifications, although the latter may be overly restrictive.

Main Results

For depression, the rank coefficient is in the expected negative direction across all models, and statistically significant at the 5% level in models 2 through 4. In the preferred specification in model 3, a 0.11 (within-school SD) increase in rank is associated with a reduction of 0.14 points on the 57 point CES-D scale. Given that the average depression score in the sample is only 11.2, this effect size is not necessarily trivial. Even in the most restrictive model, the effect size and significance remain approximately the same.

For school attachment, the rank coefficient is in the expected positive direction and statistically significant across all models. In model 3, a 0.11 increase in rank is associated with a 0.055-point increase on the 15-point school attachment scale. Evaluated at the sample mean of 11.2, this effect size is rather small; in the even more restrictive model 4, however, it remains statistically significant at the 5% level.

Table 2.
The Effect of Socioeconomic Rank: OLS Regression Results

	Model			
	(1)	(2)	(3)	(4)
		Depressive Symptoms		
Rank coefficient	−0.589	−1.387*	−1.421*	−1.579*
	(0.588)	(0.673)	(0.686)	(0.680)
Observations	11,873	11,873	11,873	11,873
R^2	0.105	0.113	0.124	0.124
		School Attachment		
Rank coefficient	0.380**	0.541***	0.552***	0.398*
	(0.180)	(0.205)	(0.210)	(0.233)
Observations	11,873	11,873	11,873	11,873
R^2	0.041	0.045	0.062	0.062
		Truancy		
Rank coefficient	−1.120*	−1.490**	−1.494**	−0.783
	(0.661)	(0.659)	(0.669)	(0.648)
Observations	11,873	11,873	11,873	11,873
R^2	0.076	0.078	0.094	0.095
		Educational Attainment		
Rank coefficient	1.493***	1.808***	1.839***	1.148***
	(0.263)	(0.314)	(0.324)	(0.301)
Observations	7,131	7,131	7,131	7,131
R^2	0.286	0.289	0.302	0.307
School FE	Yes	Yes	No	No
Grade FE	Yes	Yes	No	No
Individual SES	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes
Grade means	No	Yes	No	No
School x Grade FE	No	No	Yes	Yes
SES^2	No	No	No	Yes

Note: *p<0.05; **p<0.01; ***p<0.001
Standard errors clustered at school level
Samples restricted to grade size >= 20

The rank coefficient for truancy is also in the expected direction across all models, and generally statistically significant. In model 3, a 0.11 increase in rank is associated with a 0.15-day reduction in school days missed without an excuse. Given that the average student in the sample only reports missing 2.2 school days, this effect size may also be non-trivial; upon inclusion of the squared term in model 4, however, the coefficient is cut in half and reduced to statistical insignificance.

For educational attainment, the rank coefficient is highly significant across all models and again in the direction expected. In the preferred specification of model 3, a 0.11 increase in rank is associated with a 0.184 increase in years of education attained by wave 4. This effect size is reduced to 0.115 years upon inclusion of the squared socioeconomic term in model 4, but remains highly significant.

Overall, results for the effect of rank on psychosocial health and educational attainment are largely consistent. Although the effect for each individual outcome appears rather small, the consistency of the direction and general statistical significance of the coefficients are highly suggestive. The reductions in effect size in model 4, moreover, may be misleading given that the variance explained is virtually unchanged from model 3 for each outcome. Importantly, if rank reductions induce many detriments at once, as appears to be the case, it is possible they could aggregate into a more robust form of disadvantage.

It is also important to emphasize that these effect sizes may be more economically significant than they appear at first glance. Given that identifying variation is derived exclusively within schools, with contextual effects eliminated, the findings here are likely conservative. Moreover, although I discussed the effects primarily in relation to the within-school standard deviation in rank of 0.11, significantly larger rank differences across grades within schools are common for students with equivalent absolute status, as illustrated in figure 2.

Further Robustness Checks

In a series of robustness checks, I further explore issues related to the measurement of socioeconomic status, the chosen grade-size threshold, reference-group salience, and the stability of rank. Regarding socioeconomic status, I rerun all models using various combinations of parental education, log and unlogged income, and median neighborhood income. Neighborhood income is measured at the block level and linked from the 1989 census. To ensure comparability, I convert the values provided to 1994 dollars using the standard Personal Consumption Expenditures (PCE) deflator. Although this provides a cruder measure of household income, it comes with the benefit of low missingness. Results are broadly similar across SES composites.

I also reconduct all analyses with several different SES specifications while additionally varying the minimum grade size threshold for the sample. Along with the baseline cutoff of 20 students, I test grade size minimums of 10, 15, 25, and 30. Moreover, I test several of these varying thresholds using the full sample, the sample restricted to high schools (grades 9-12) only, and the sample restricted to respondents who at least graduated high school. These restriction strategies are intended to help address concerns about the stability of socioeconomic rank across time, and also to hedge against dynamic attrition.

Finally, it is important to note that I conducted a preliminary replication of the results presented here using the longitudinal High School and Beyond (HS&B) study, which was administered by NCES beginning in 1980 with four follow-up waves. Findings with this alternative dataset are largely insignificant, but likely unreliable for several reasons. Namely, the within-grade sample size is much smaller than in Add Health, only two grades maximum from each school are sampled, and the socioeconomic indicators are fairly coarse (e.g., household income is categorical rather than continuous, and only available via student report). Thus, there is good reason to view the findings from Add Health with far more confidence.

VI. Conclusion

The findings in this paper indicate that socioeconomic rank among school peers has a significant effect on both psychosocial health and educational attainment. More broadly, they highlight that local status matters, which at the very least complicates (or complements) the common assumption in previous work that relative deprivation operates primarily across broad, aggregate, or distant groups. Relatedly, this paper suggests the need to more carefully specify the connection between socioeconomic inequality and psychosocial outcomes – rank and distance within a hierarchy both appear important, but it may not be justified to conflate them, and their relative salience may be context dependent. Given recent trends in economic segregation between school districts, for example, it is possible that compressed socioeconomic distributions within schools will sharpen the effect of ordinal position, particularly in low-income schools in which students may have to compete for increasingly scarce resources (Owens 2018).

This study also complements a growing body of causally well-identified work suggesting that peer effects are frequently non-linear and highly context dependent. Most work in this area has examined the effect of peer ability composition on academic outcomes, and it is still unclear how proposed frameworks for peer ability translate to socioeconomic status. Nonetheless, it appears there may be common mechanisms at play, and it would be valuable to further integrate frog pond, BFLP, and relative deprivation perspectives.

Complementing work by Elsner and Isphording (2017 & 2018) in particular, this paper sheds further light on how rank effects might be working behind the scenes to diminish expected positive outcomes - if rank tradeoffs are important, as suggested, mere exposure to more socioeconomically advantaged peers may not produce straightforward benefits. Especially when considering policies like school transfer programs, it could be important to mitigate vulnerabilities induced by dramatic rank reductions, so that relatively disadvantaged students can reap the full rewards of their academic environments.

VII. Next Steps

Moving forward, I first plan to examine the salience of rank within gender and race-specific reference groups within grades. Indices constructed from Add Health's network variables indicate that social ties are highly segregated by grade, and that there is little difference across grades in terms of degree of separation, but it is possible that rank may be more influential within particular subgroups. I also plan to assess potential non-linear effects of rank, to see if there are differential effects by rank position (e.g., above and below the grade mean). I also intend to further interrogate the assumptions of the causal identification presented by examining changes in peer composition and rank stability across waves, and leveraging the saturated school samples to get a sense of the margin of error for rank that results from the sampling procedure in other schools. In addition, I will more thoroughly examine the empirical distributions of grades within schools to provide a more concrete sense of how differences in variance are being exploited to identify the rank effect (i.e., empirical demonstrations of the stylized model in figure 2B). Furthermore, I intend to conduct an analysis incorporating the Yitzhaki index for purposes of comparison, and will attempt to decompose the contributing components of rank and distance. Finally, I plan to conduct a more thorough replication of the High School and Beyond data, using various strategies to conserve school- and grade-level sample size.

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Appendix

OLS Regression Results

	Dependent Variable			
	Depressive Symptoms			
	(1)	(2)	(3)	(4)
SES Percentile	−0.589	−1.387*	−1.421*	−1.579*
	(0.588)	(0.673)	(0.686)	(0.680)
SES	−0.325	−0.040	0.026	−0.205
	(0.246)	(0.272)	(0.288)	(0.545)
SES^2				0.047
				(0.079)
Ability	−0.077***	−0.076***	−0.077***	−0.077***
	(0.006)	(0.006)	(0.006)	(0.006)
Male	−2.077***	−2.102***	−2.066***	−2.065***
	(0.169)	(0.170)	(0.174)	(0.174)
Age (months)	0.334***	0.332***	0.292**	0.291**
	(0.094)	(0.093)	(0.102)	(0.102)
Age^2	−0.001**	−0.001**	−0.001*	−0.001*
	(0.0002)	(0.0002)	(0.0003)	(0.0003)
Hispanic	0.925***	0.944***	0.980***	0.979***
	(0.250)	(0.259)	(0.249)	(0.249)
Black	−0.046	−0.062	−0.027	−0.023
	(0.322)	(0.321)	(0.326)	(0.324)
Asian	2.197***	2.139***	2.314***	2.315***
	(0.453)	(0.465)	(0.428)	(0.430)
Foreign born	−0.801**	−0.854**	−0.693*	−0.704*
	(0.299)	(0.285)	(0.311)	(0.309)
Two parent family	−0.868***	−0.858***	−0.834***	−0.830***
	(0.179)	(0.178)	(0.184)	(0.185)
Household size	0.088	0.092	0.081	0.080
	(0.047)	(0.048)	(0.048)	(0.048)
Constant	−16.699	−59.244**	−15.803	−15.337
	(8.745)	(18.055)	(9.851)	(9.840)
School FE	Yes	Yes	No	No
Grade FE	Yes	Yes	No	No
Grade means	No	Yes	No	No
School x Grade FE	No	No	Yes	Yes
Observations	11,873	11,873	11,873	11,873
R^2	0.105	0.113	0.124	0.124

Note: *p<0.05; **p<0.01; ***p<0.001
Standard errors clustered at school level
Samples restricted to grade size >= 20

	Dependent Variable			
	Perceived School Attachment			
	(1)	(2)	(3)	(4)
SES Percentile	0.380*	0.541**	0.552**	0.398
	(0.180)	(0.205)	(0.210)	(0.233)
SES	−0.050	−0.107	−0.126	−0.351*
	(0.084)	(0.092)	(0.098)	(0.149)
SES^2				0.046
				(0.024)
Ability	−0.004	−0.004	−0.004	−0.004
	(0.002)	(0.002)	(0.002)	(0.002)
Male	0.094	0.091	0.095	0.096
	(0.058)	(0.059)	(0.059)	(0.059)
Age (months)	−0.046	−0.044	−0.039	−0.040
	(0.038)	(0.038)	(0.038)	(0.038)
Age^2	0.0001	0.0001	0.0001	0.0001
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Hispanic	−0.037	−0.031	−0.057	−0.057
	(0.090)	(0.092)	(0.090)	(0.090)
Black	−0.215*	−0.227*	−0.210*	−0.207*
	(0.104)	(0.107)	(0.104)	(0.104)
Asian	0.070	0.092	0.024	0.024
	(0.161)	(0.162)	(0.178)	(0.179)
Foreign born	0.144	0.140	0.127	0.116
	(0.095)	(0.096)	(0.095)	(0.093)
Two parent family	0.218***	0.200**	0.221***	0.225***
	(0.064)	(0.063)	(0.064)	(0.065)
Household size	0.041**	0.043**	0.041*	0.040*
	(0.016)	(0.016)	(0.016)	(0.016)
Constant	17.840***	29.068***	16.194***	16.649***
	(3.640)	(7.004)	(3.732)	(3.754)
School FE	Yes	Yes	No	No
Grade FE	Yes	Yes	No	No
Grade means	No	Yes	No	No
School x Grade FE	No	No	Yes	Yes
Observations	11,873	11,873	11,873	11,873
R^2	0.041	0.045	0.062	0.062
Note:	*p<0.05; **p<0.01; ***p<0.001			
	Standard errors clustered at school level			
	Samples restricted to grade size >= 20			

	Dependent Variable			
	Truancy			
	(1)	(2)	(3)	(4)
SES Percentile	−1.120	−1.490*	−1.494*	−0.783
	(0.661)	(0.659)	(0.669)	(0.648)
SES	0.069	0.190	0.235	1.271*
	(0.279)	(0.280)	(0.292)	(0.567)
SES^2				−0.212*
				(0.086)
Ability	−0.025***	−0.025***	−0.026***	−0.025***
	(0.006)	(0.007)	(0.007)	(0.007)
Male	0.317*	0.369*	0.284*	0.279*
	(0.138)	(0.144)	(0.141)	(0.142)
Age (months)	−0.063	−0.056	−0.106	−0.101
	(0.096)	(0.097)	(0.098)	(0.098)
Age^2	0.0004	0.0004	0.0005	0.0005
	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Hispanic	0.338	0.347	0.330	0.332
	(0.299)	(0.292)	(0.315)	(0.315)
Black	−1.399***	−1.379***	−1.420***	−1.437***
	(0.240)	(0.244)	(0.239)	(0.242)
Asian	−0.452	−0.421	−0.512	−0.515
	(0.432)	(0.447)	(0.368)	(0.374)
Foreign born	−1.493***	−1.483***	−1.484***	−1.433***
	(0.317)	(0.325)	(0.307)	(0.308)
Two parent family	−1.028***	−1.010***	−1.032***	−1.049***
	(0.280)	(0.279)	(0.290)	(0.295)
Household size	−0.018	−0.022	−0.020	−0.016
	(0.079)	(0.079)	(0.080)	(0.081)
Constant	3.700	−2.266	9.311	7.216
	(8.790)	(16.559)	(8.843)	(9.137)
School FE	Yes	Yes	No	No
Grade FE	Yes	Yes	No	No
Grade means	No	Yes	No	No
School x Grade FE	No	No	Yes	Yes
Observations	11,873	11,873	11,873	11,873
R^2	0.076	0.078	0.094	0.095

Note: *p<0.05; **p<0.01; ***p<0.001
Standard errors clustered at school level
Samples restricted to grade size >= 20

	Dependent Variable			
	Years of Education			
	(1)	(2)	(3)	(4)
SES Percentile	1.493***	1.808***	1.839***	1.148***
	(0.263)	(0.314)	(0.324)	(0.301)
SES	0.242	0.128	0.091	−0.892***
	(0.137)	(0.160)	(0.169)	(0.208)
SES^2				0.201***
				(0.032)
Ability	0.042***	0.042***	0.041***	0.041***
	(0.002)	(0.002)	(0.002)	(0.003)
Male	−0.681***	−0.671***	−0.697***	−0.689***
	(0.054)	(0.053)	(0.057)	(0.057)
Age (months)	−0.032	−0.033	−0.054	−0.059
	(0.035)	(0.035)	(0.035)	(0.035)
Age^2	−0.00003	−0.00003	0.00002	0.00003
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Hispanic	0.174	0.177	0.153	0.149
	(0.153)	(0.153)	(0.155)	(0.149)
Black	0.388***	0.402***	0.370***	0.377***
	(0.111)	(0.112)	(0.111)	(0.109)
Asian	0.519**	0.509***	0.507**	0.506**
	(0.158)	(0.154)	(0.158)	(0.157)
Foreign born	0.742***	0.746***	0.719***	0.675***
	(0.096)	(0.096)	(0.103)	(0.100)
Two parent family	0.166**	0.171**	0.145*	0.157**
	(0.058)	(0.057)	(0.058)	(0.059)
Household size	−0.042	−0.045	−0.044	−0.049
	(0.025)	(0.025)	(0.026)	(0.026)
Constant	13.836***	5.125	20.957***	22.981***
	(3.371)	(5.228)	(3.532)	(3.498)
School FE	Yes	Yes	No	No
Grade FE	Yes	Yes	No	No
Grade means	No	Yes	No	No
School x Grade FE	No	No	Yes	Yes
Observations	7,131	7,131	7,131	7,131
R^2	0.286	0.289	0.302	0.307

Note: *p<0.05; **p<0.01; ***p<0.001
Standard errors clustered at school level
Samples restricted to grade size >= 20