

## Schools and Attitudes Toward Economic Equality

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*Do policies that shape equality in schools have effects on the type of society and polity that the citizens educated in them want? This paper examines this question by analyzing variation in the English schooling experiences using the British Cohort Study and British Panel Study. It shows that the social environment of schooling affects adults' attitudes to fairness and Conservative vote choice, but that policies targeting these social environments have weak effects. The paper theorizes that actual policy feedback in education is often limited, because the effects of policies on school experiences are mediated by the behaviors of other actors on the ground.*

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促进学校平等的政策是否会影响处于其中的受教育公民对社会和政体形态的期望? 为探讨这一问题, 本文使用英国的面板数据和队列研究来检验英国学校教育的差异。本文表明, 学校教育的社会环境会强烈影响学生成年后对公平的态度和对保守党的投票选择, 但针对社会环境的政策所带来的这方面影响却效果微弱。本文认为, 教育中的实际政策反馈往往是有限的, 因为政策对学校经历的影响并非一个简单直接的过程。

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### 1. Introduction

The idea that education policy exerts powerful feedback effects on citizens' values and behaviors is widespread, and indeed, well pre-dates academic debates over policy feedbacks. The history of educational expansion in the early part of the 20th century was often conflictual, pitting the Church versus the state, and central versus local governments, precisely because the actors involved saw education policy as a tool to shape both future hearts and minds (Ansell & Lindvall, 2013). In the post-war period, conflict over education policy moved to new terrains, yet political debates over academic streaming, segregation, equal funding, and school choice have continued to raise questions not just about the economic effects of policy but also its role in shaping the social values of pupils.

This paper examines whether policies that shape equity in schooling feed back into beliefs about the type of society and polity that those educated in them want. It makes two claims. First, it argues that schools themselves have strong effects on beliefs about social fairness that persist into adulthood. In schools that are *stratified*

by social class and where quality is less *standardized*, those educated in affluent schools tend to view society as fairer while those in deprived schools see it as less fair than similar students in mixed schools.

Second, it argues that, despite the idea that the policies lying behind schools are the “shapers of society,” the link between policies toward standardization and stratification and attitudes is often weak. Schools matter for attitudes, policies often matter for schools, but the link between policies and attitudes can be attenuated by parental behaviors. In order to conceptualize why schooling can simultaneously be incredibly important for social attitudes but schooling policies can have limited effects on such attitudes, the paper unpacks the process of policy feedback in education within the broader context of class reproduction in schooling.

The paper first develops a series of theoretical claims about why differences in the stratification and standardization of schooling matter for attitudes. Schools where children are more divided by class or racial background—*stratified* environments—and where these divisions result in differential access to high-quality education—less *standardized* environments—provide different long-run material resources to citizens and interpretive frames about social fairness (Pierson, 1993). These claims resonate with a long line of literature on policy feedbacks, which show that citizens’ actual experiences with government shape both political participation and attitudes (Campbell, 2003, 2012; Gingrich & Watson, 2016; Kumlin, 2004; Mettler, 2002; Soss, 1999; Soss & Schram, 2007) as well the ways that families socialize their children into future political behavior (Barnes & Hope, 2017).

In theory, policies can shape how stratified or standardized the schooling system is (Allmendinger, 1989). A range of studies show that policy structures often do moderate or enhance the role of social background on short-term performance (Van de Werfhorst & Mijs, 2010) and long-run income mobility (Breen, 2010; Kerckhoff, 2001; Müller & Karle, 1993). However, parental behaviors also shape how schools operate: policy does not fully determine school experiences. These “on the ground” behaviors can complicate the steps linking policies to attitudinal outcomes, reducing the impact of policy.

To develop these claims, I draw on the case of schooling in England, which offers both variation in the policy environment and the existence of long-term panel studies allowing a careful test of the above claims. I examine the long-run effects of schooling on adults’ attitudes using the British Cohort Study (BCS), which follows a single cohort born in 1970 across their lifetime. I show that in a context of low standardization, those who attended a more stratified affluent school see society as fairer as adults and are more likely to vote Conservative than those with similar backgrounds who attended more mixed schools, whereas those attending a more stratified deprived school see society as less fair and are less likely to vote Conservative.

I then turn to testing the independent impact of policies targeting stratification and standardization. First, to investigate policies toward stratification, I continue to examine the 1970s birth cohort, looking at whether those educated in schools in geographic areas with more overt policies of stratification differ from those who are not. Here I find only weak long-run effects on attitudes.

Second, as there is little information on geographic variation in policies toward standardization for the 1970s birth cohort, to examine the effect of policies standardizing education I turn to more recent birth cohorts. Cohorts educated in the 1990s and 2000s experienced different policies centralizing control over performance, with a dramatic decline of overall variation in school test scores through this period. The paper turns to parent–children pairs using the British Household Panel Study (BHPS) to see whether either this temporal decline in variation, or different levels of variation across geographic areas, are systematically linked to the attitudes of young adults. Here I find evidence that the cohorts educated during the late-1990s and early 2000s view society as fairer than those educated in the early 1990s, but there is little evidence that geographic variation in standardized quality has effects on attitudes.

This paper uncovers a paradox in policy feedbacks in education. Schools matter deeply, but the policies that shape them seem to be more limited. This claim dovetails with longstanding arguments in sociology that educational policies matter less than both the practices in schools (Bourdieu & Passeron, 1990) and the more generic experience of being a student (Meyer, 1977). The paper concludes, though, by arguing that educational policy can have effects on citizens' attitudes, but to understand this process, we need to be more attentive to how policies promoting equality interact with social inequalities to shape experiences.

## 2. Education Policy and Attitudinal Feedbacks

Schools are simultaneously one of the most important levers in the reproduction of inequality and one of the most important levers in minimizing it. On aggregate, policies toward schools are associated with more or less equality among different social groups (OECD, 2010). But do policies enhancing equality in schooling have attitudinal feedbacks on how citizens educated in them think about the world?

In order to examine whether (dis)equalizing educational policies feed back on attitudes, we first need to define the realm of attitudes and behaviors that schooling can affect, and then examine the link between schooling experiences and policies. The following two sections develop a series of theoretical claims about policy feedbacks in education that suggest first that inequality across schools is likely to create different material incentives and interpretive frames shaping evaluations of social fairness and support for redistribution, and second, that policies *can* affect these experiences but often *do not* because parental behaviors are hard to alter.

### 2.1. *Do Schools Shape Social Attitudes?*

School-age children generally have universal access to education; however, the distribution of the quality of education can create dramatically different experiences. Educational sociologists often distinguish the broad features of education systems based on the degree of “stratification” and “standardization” (Allmendinger, 1989). Stratification refers to the extent to which pupils are sorted

into different schools based on background or ability. Standardization refers to whether educational quality and experiences are uniform across a geographic unit, such as a region or state, or not. I use these terms to refer to school *outcomes* and in the next section I turn to the *policies* that shape stratification and standardization.

In some places, schools draw from a broad cross-section of society—they are not highly stratified by class or race—and schools in a given area are more standardized—meaning that they are relatively equal in terms of quality. In other contexts, schools draw on students from less diverse backgrounds—they are stratified by class or race—and/or schools are highly unequal in terms of the quality of education they offer children—they are less standardized. Stratification of schools by social groups, and the standardization of quality across schools, are often, but not always linked (Van de Werfhorst & Mijs, 2010). However, both high levels of stratification and low levels of standardization can create class- or group-based educational inequalities. We can think about educational inequality on a continuum, from a relatively low-inequality system defined by low stratification and high standardization where different social groups mix with each other and have access to similar quality schooling, to a relatively high-inequality system where schools are stratified on class or racial lines in ways that are linked to differences in quality due to low standardization.

I investigate whether we would expect schools under conditions of high inequality, due to more stratification and lower standardization, to have different attitudinal feedback effects on young people than those under conditions of low inequality due to lower stratification and/or higher standardization. In this respect, in theorizing school effects, I treat stratification and standardization as having a similar logic via the level of educational inequality. Because policies toward stratification and standardization differ, however, I separate these two features conceptually in the next section when discussing policy.

There is a large body of work investigating whether educational experiences have causal effects on attitudes and long-term behaviors. Early work on young adult socialization offered mixed results regarding the impact of schooling, with much work arguing that schools have limited effects, as attitudes and behaviors stabilized in adulthood (Stoker & Bass, 2011). Moreover, recent studies of electoral turnout, which attempt to causally disentangle the effect of education from the factors shaping selection into education via experimental and quasi-experimental designs, have yielded inconsistent findings.<sup>1</sup>

Despite these mixed results, recent work returning to the question of schooling as a source of economic and social values has found schooling can affect basic beliefs about the structure of society in ways that persist into adulthood (Bruch & Soss, 2018; Marshall, 2016a; Mendelberg, McCabe, & Thal, 2016; Surridge, 2016). This work emphasizes two major channels through which more/less equality in the schooling can affect long-run economic attitudes, which mirror Pierson's (1993) material and interpretive channels.

First, differing levels of stratification and standardization—and the resultant equality in the distribution of high-quality schooling—can have straightforward *material effects* that are likely to shape attitudes.

Schools affect future adult resources by shaping skills and lifetime income. Economists have long used variation in birth-year cohorts and/or geographic variation in the expansion of compulsory education to estimate the effects of returns to education; finding that education does indeed increase earnings beyond selection into it (Angrist & Krueger, 1991). John Marshall's (2016a) work on the expansion of compulsory education in the UK uses this same strategy to assess feedbacks from schooling on political attitudes. Marshall finds that increased schooling reduces support for redistribution and increases Conservative party choice, arguing that this link largely works through a lifetime income effect, something he also finds in the U.S. case (Marshall, 2016b).

While Marshall's interest is in the generic expansion of education, the process through which schools "sort" and "sieve" students into different lifetime opportunities can be powerful (Sorokin, 2011). Variation in the stratification and standardization of schools, and the resultant distribution of quality, something Marshall does not explicitly examine, is likely to matter to the mechanisms that he highlights.

Early moves to both raise the school leaving age and reduce academic selection in Europe, for instance, often had a greater impact on low-income pupils and the schools they attended. In Sweden, Meghir and Palme (2005) find that the expansion of non-selective education benefited the earnings of all social groups, but it was particularly beneficial for the children of low-skilled fathers. Deondra Rose's (2018) work on mass higher education expansion among women shows a similar dynamic in a very different context. Rose demonstrates how Title IX policies in the United States affected women's equal participation in political life and the labor market by expanding their access to higher education on equal terms. Other types of schooling experiences may distribute the material benefits of education toward the already privileged. Looking at the overall correlation between school quality and parental income, and using an estimate of the effects of high-quality kindergartens on lifetime earnings (drawn from the Tennessee STAR experiment), Raj Chetty and John Friedman (2016), argue that: "if school quality was unrelated to income in the United States, the intergenerational transmission of inequality would fall by 40 percent." Put differently, the material effects of schooling are large and can be more or less equally distributed across social groups.

Following Marshall's logic, where schooling is less equal due to higher stratification/lower standardization, and post-school opportunities are also less equal, those with access to more privileged schooling achieve more lifetime material benefits from it, and are thus more likely to adopt economically conservative attitudes on income redistribution. Those facing the negative consequences inequality, and thus lower lifetime income, are less likely to be economically conservative on redistributive issues. Where more standardization and less stratification produce more equality in schooling across groups, *ceteris paribus*, those from different schools should be less distinct on these issues.

*Hypothesis 1: Where there is lower standardization, those who attended a stratified affluent school will be less pro-redistribution, and those who attended a stratified*

*deprived school will be more pro-redistribution, compared to pupils of a similar background who attended more standardized or less stratified schools.*

However, schools do not just contribute to a student's lifetime income, but are lived experiences that form one of an individual's earliest and most profound social encounters. Work on educational socialization points to how the experience of attending a school (or college) can provide such *interpretive frames* to children and young adults about social fairness, order, and the policies aimed at addressing it. This resonates with Schneider and Ingram's (1997) work conceptualizing the ways that policies can include or stigmatize recipients, providing them with varying frames to understand their experiences. However, the frames encountered in schools can come from multiple sources, not just policies, but teachers, peers, quality of the infrastructure, and so on.

Are there systematic interpretative frames that schools provide? Work on schooling under conditions of educational inequality suggests that there are. This work argues that affluent schools, and the social networks around them, explicitly cultivate frames that naturalize social inequalities. For instance, in his ethnography of an elite private school in New Hampshire, Khan (2011) argues that the school's framing of pupils' experiences as "meritocratic," rather than "privileged," allows its wealthy students to perceive inequality as fairer. Mendelberg et al. (2016), looking at young adult socialization on college campuses, argue that attending an affluent college leads students to become more economically conservative (compared to their pre-adult attitudes) than similar students educated in less affluent environments, an effect they associate with socialization by wealthy and allegedly meritocratic networks.

Where these interpretive effects combine with the aforementioned material effects, sorting pupils into privileged post-school social networks, they can compound. Recent work by Mo and Conn (2018), looking not at pupils, but at teachers, argues that exposure to inequality among privileged young adults participating in the "Teach for America" (TFA) program shapes attitudes. Mo and Conn find that those who were exposed to prolonged class and racial diversity through their teaching experiences adopted economic and social attitudes closer to their more disadvantaged pupils than those who applied for TFA but did not participate. In the absence of the "shock" of TFA participation, most wealthier graduates remain insulated from the perspectives of low-income citizens, holding economically distinct attitudes.

Low-income and ethnic minority pupils often experience the long-lasting effects of more stratified/less standardized environments by seeing injustice, rather than fairness. A number of studies, both quantitative and ethnographic, find that low-income students in the United States, and in particular African American and Hispanic young people, are more likely to experience harsh disciplinary treatment and less positive attention from teachers. Bruch and Soss (2018) show that these disciplinary experiences in schools shape citizens' long-run attitudes toward authority, with harsh treatment depressing political efficacy and trust. Gimpel, Lay, and Schuknecht (2003), in their study of teenagers at a variety of schools in the Baltimore area, find that cues provided to pupils from perceived fair treatment by teachers



also contribute to beliefs about fairness in society more generally. While less stratified schools do not necessarily treat minority pupils more fairly, the concentrated disadvantage produced by high stratification and low standardization can magnify the scope of such treatment.

However, a second line of work suggests that those in disadvantaged environments may be less aware of injustice, because they lack a comparative reference point. Carla Shedd's (2015) mixed-methods study on Chicago public schools argues that it is young people with exposure to inequality—those that leave disadvantaged neighborhoods and cross the physical and social boundaries of Chicago schools—who are the most aware of social injustice. Many of the low-income and largely African American teenagers in the inner-city school that she studies are simply not aware of the extent of their disadvantage. Newman, Johnston, and Lown (2015) make a similar argument about adults. They argue that contexts of greater exposure to income inequality can activate class-based thinking, leading low-income people to see society as less fair while pushing high-income people to see it as more meritocratic. Michener (2013) further shows that subjective perceptions of neighborhood experiences of deprivation are often more important than objective conditions. Put differently, this work argues that high stratification and low standardization may simultaneously shape attitudinal differences across groups and reduce the salience of these differences.

When we combine the material and interpretive arguments over the full life-cycle, however, we produce less ambiguous expectations. Although young people still in school may not understand the depth of their advantage, or lack thereof, as they encounter unequal life circumstances, these early inequalities are more likely to become visible, amplifying the initial frames. By contrast, those educated in less stratified or more standardized schools (or who are simply from less economically unequal areas), have broader social networks and share more common frames.

*Hypothesis 2: Where standardization is low, those who attended stratified affluent schools will have increased beliefs in social fairness, whereas those who attended a stratified deprived school will have decreased beliefs in social fairness, compared to pupils of similar background who attended more standardized or less stratified schools.*

## 2.2. Stratification and Standardization: A Policy Feedback?

The above section argued that features of the schooling environment—the degree of standardization and stratification—shape inequality in schools, providing different material and interpretive frames to pupils that are likely to affect their adult social attitudes. What then produces school-level stratification and standardization? The following section argues that both parental behaviors and policies shape the way schools operate, and that behavior and policy can be in tension.

Sociologists studying the intergenerational transmission of inequality often distinguish between the “primary” and “secondary” effects of class background on children’s educational outcomes (and income mobility) (Jackson, 2013). Primary effects involve the direct effects of class transmission between parents and children, for instance, parental characteristics and behaviors in the home (e.g., genetics, reading to children). Secondary effects refer to the more indirect effects that emerge from school choices, meaning the added effects of schooling following from higher-income children having access to better quality education.

Where these “secondary effects” are strong, parental choices can produce more stratified and less standardized outcomes; where “alike” parents (whether by class or race) sort themselves into different schools through their residential choices or through post-residential school choices it can lead to stratified school environments with low social mixing (Gingrich & Ansell, 2014). When this process leads uneven peer-effects, teacher recruitment (Eppe & Romano, 2011; Jackson, 2009; Sirin, 2005), or (particularly in the U.S. case) funding and resources, the result can be much less standardized quality. Parental choices then, can produce more stratification, and less standardization, when they follow class or racial background and filter quality education to different groups.

Policies can affect this process in two ways: first by shaping the options available to different groups (the scope for stratification) and second by shaping the translation of these choices into differences in quality (the degree of standardization). Jackson and Jonsson (2013), reviewing the evidence from eight country-based cohort studies argue that policies can indeed modify the relationship between parental background and school choices (“secondary effects”) much more than they can alter the way parents behave in the home (“primary effects”).

First, policies can shape stratification by expanding or limiting tracking pupils into different academic streams. In some countries, like Germany, students are tracked at a young age, spending most of their teen years in streamed schools; whereas elsewhere streaming occurs later (around 16). In theory, academic streaming does not necessarily stratify schools by race or class if selection into the streams is independent of background. In practice, parental background often matters, particularly where there is early streaming, as upper- and middle-class pupils are more likely to attend academic streams. Indeed, there is evidence that academic streaming can increase the link between test scores of social background (Van de Werfhorst & Mijs, 2010) and early streaming can reduce long-run equality of opportunity (Brunello & Checchi, 2007). While streaming also occurs within schools, streaming across schools can be more constraining in creating “categorical inequalities” that are hard to alter (Domina, Penner, & Penner, 2017).

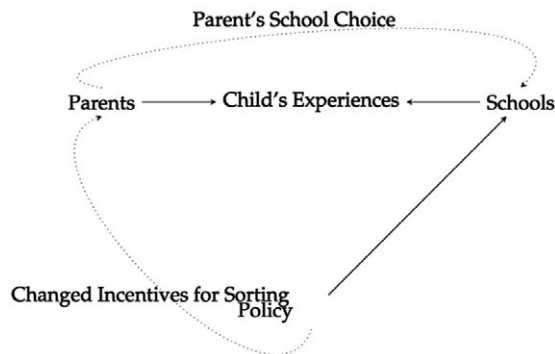
Second, policies that equalize resources across schools or limit highly localized responsibility for education, through a common curriculum, testing, or teaching qualifications, can increase standardization. While some scholars argue that school autonomy over such practices can exert positive pressure on schools to improve—Caroline Hoxby (2003) argues choice and competition are “a rising tide that lifts all boats”—it can risk leaving some pupils in lower quality schools (Ladd, 2003). This



outcome is more likely when resources are uneven and when there are a few levers to ensure basic teacher and infrastructural quality.

It can be useful then, to think of government's direct policy levers over schooling as the inverse of the family's levers. The primary effect of policies lies in the scope for schools to differentiate themselves (through stratified selection or unstandardized quality) to pupils from different social backgrounds. Policies can have secondary effects by reducing the incentives of parents to seek out more stratified schooling. Where policies reduce the links between resources and choices *and* reorient parental behaviors, they will reduce the level of stratification and standardize quality at the school level. Where they shape school behavior but not parental behaviors, they will be less effective. In his work on school segregation in the United States, Jeremy Fiel (2013) shows an example of the latter process, arguing that while local government policies of racial de-segregation often push toward more equity in schooling, the residential choices of parents push against it (see also Reardon and Bischoff [2011] on income-based sorting). Put more bluntly, "social closure" behaviors can dull the impact of policies aimed to reduce stratification and increase standardization. Figure 1 schematizes this argument, with the solid arrows representing the primary effects and the dotted arrows representing the secondary effects of parental behaviors and policy choices.

Where equalizing policies do have an effect, they are likely to attenuate the relationship between class background and future opportunities, and thus the uneven attitudinal feedbacks theorized above. Indeed, such policies may even have an effect beyond just the distribution of school experiences, signalling to children a distinct norm. Suzanne Mettler's (2002) study of the U.S. GI Bill shows such a compounding effect in a different context. Mettler argues that the policies which directly expanded veterans' access to college education in the post-World War Two period formed an important component of the process of turning "soldiers into citizens." The GI Bill provided veterans with material resources, but also, in signalling their social value, it provided them with a sense of fair treatment that created a norm of political participation.



**Figure 1.** Primary and Secondary Effects in Education, and Policy Feedbacks.

*Hypothesis 3a: Policies that decrease stratification in education (through academic selection) will reduce the polarizing effects of schools on beliefs in redistribution (hypothesis 1) and social fairness (hypothesis 2).*

*Hypothesis 3b: Policies that increase standardization in education (through improved performance or regulation) will reduce the polarizing effects of schools on beliefs in redistribution (hypothesis 1) and social fairness (hypothesis 2).*

In summary, the last two sections have argued that schools should matter in systematic ways, and that the policies that shape how schools operate should also matter. The next sections turn to testing these claims by turning to panel data in England.

### 3. Education in England

Most studies of policy feedback suffer from challenges in identifying policy effects because of the lack of high-quality over-time data. These problems are particularly acute in the study of education, as, for the reasons outlined above, selection into schooling is related to a number of features of pupil background that also shape long-run attitudes.

To examine both (i) school effects and (ii) policy effects, I examine secondary-schooling in one country, England, in depth. The English case offers two distinct advantages for studying policy feedback: there are high-quality panel datasets that permit the analysis of children and parents over the life-cycle, and there exists both substantial local and temporal variation in policies of stratification and standardization that allow a test of policy effects.<sup>2</sup>

English children attend a common primary school until the age of 11. At 11, children enter secondary schools, where they sit standardized tests at age 15/16, and then can choose to leave school, or follow an academic A-level path or vocational training (since 2015 children must stay in education until 18).

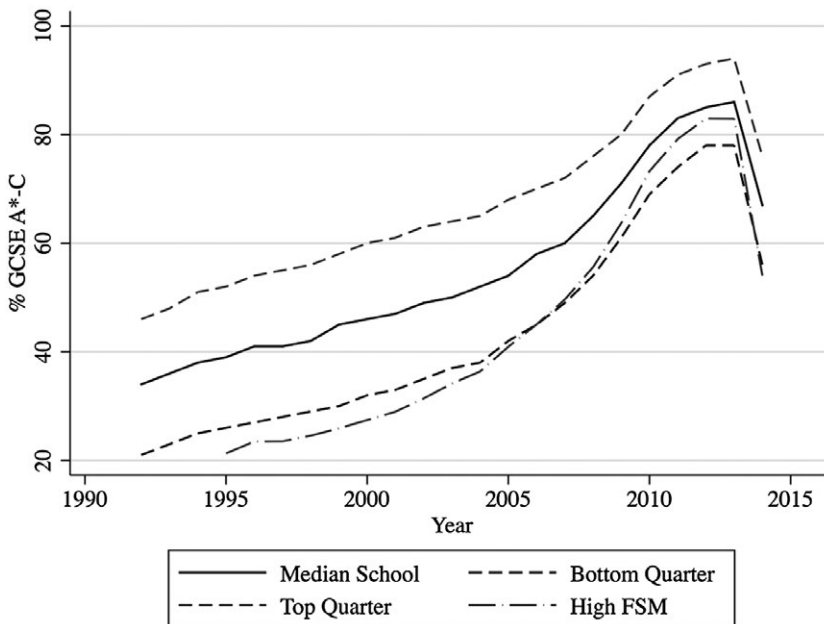
Schools across England have long varied in the extent of social mixing, but explicit *policies of stratification* vary across geographic regions. Some local authorities in England have a system of streaming children into selective “grammar” schools or non-selective comprehensive schools at age 11.<sup>3</sup> From the mid-1980s, there has been a stable number of selective grammar schools in England (164), with full-fledged systems of academic selection operating in several populous counties, including Kent, Buckinghamshire, and Lincolnshire. Other counties offer a more limited set of selective schools (or none), educating the large majority of pupils in non-selective comprehensive schools with mixed ability.

In terms of *policies of standardization*, over the past three decades, the central government has expanded both central regulation and deregulation. On the one hand, since 1988, the curriculum in England has been highly standardized and successive governments have expanded a relatively heavy-handed system of national school inspection. On the other hand, a series of policies that began with the Conservative government in 1988, partially retained and expanded by Labour through the 2000s,

and then dramatically expanded by the Conservative-Liberal coalition (2010–2015) allowed more school differentiation (Gingrich, 2011). Prior to the early 1990s, over three quarters of schools were run by local authorities (LEAs), with the rest operated by the Catholic Church or Church of England. The Thatcher government allowed schools to opt out of local control, creating more school self-governance. The Labour government further allowed nonstate actors (e.g., charities, businesses, or other schools) to run low-performing schools and since 2010, the Conservative-led governments have pushed for schools to convert to a self-managed (academy) status. These moves have given schools more ability to set working practices and engage in softer forms of differentiation.

Despite these contradictory pressures, through the 2000s schools became much more standardized in terms of academic performance (although variation is now growing again with new testing rules). Figure 2 shows this trend. In 1995, only 44 percent of English 15-year-olds achieved a basic lower-secondary school qualification, measured by 5 A\*-C General Certificate of Secondary Education (GCSE) qualifications. By 2011, 82 percent of 15-year-olds achieved 5 A\*-C GCSE. This shift came from particularly strong improvement in low-performing schools, and those with high levels of free or reduced school meal pupils (FSM) (in the top 25 percent nationally) and followed from an increase in central intervention in failing schools, increased inspection and target setting, as well as increased spending through the 2000s (Johnson, 2004).

I use this cross-sectional variation in policies of stratification and both temporal and geographic variation in standardization to test whether policies have any independent effect on attitudes.



**Figure 2.** Variation in Performance 1992–2015.  
*Source:* Gingrich and Ansell (2014)

#### 4. Data and Results

Ideally, to test the above hypotheses, I would have panel data on individual attitudes from the teenage years onward, data on the characteristics of the school the respondent attended, and parental background for multiple cohorts. Unfortunately, such data is not available in a single source. Instead, I combine two sources that collectively allow me to investigate the above hypotheses. The first study, the BCS, follows one cohort over the life-cycle, and has relatively comprehensive data on schooling. The second study, the BHPS, follows multiple cohorts over time, with detailed information on family background, but less information on the schooling environment. I begin by examining the effects of *schools* themselves and then *policies of stratification* using the BCS, and then turn to examining over-time changes in *policies of standardization* using the BHPS.

##### 4.1. Testing the Impact of Stratification: The BCS

The BCS is a panel study which follows 10,000 people born in the same week in 1970 over time (University of London, Institute of Education, Centre for Longitudinal Studies, 2016a, 2016b). The BCS has a wide range of data on individual background, including parents' social background, income, and geographic location, as well as children's performance on a range of cognitive tests, and attitudinal items at various points in adulthood. The nature of the questions asked changes across waves, making a direct comparison of views from childhood to adulthood difficult, but it does offer a wide range of adult attitudinal items. Surridge (2016) has used the BCS to investigate the impact of college curriculum on attitudes. Persson (2014) has used it to demonstrate a lack of long-run effects of education on political participation.

The BCS offers a unique opportunity to study school effects because, for about 3000 respondents, head teachers (school principals) answered a survey in 1986 (when cohort members were 16), describing the school environment. This survey means that for a subset of the panel, it is possible match respondents to school characteristics, isolating the effect of schooling on adult economic attitudes.

In order to first test whether schools themselves matter (hypotheses 1 and 2), I first construct two measures of the degree of school-level stratification that pupils experienced. At age 16, head teachers were asked to estimate the proportion of free-school meal pupils (a measure of low-income) in the school, and the percentage of parents who are professionals. Although in 1986 many head teachers lacked systematic data on their own pupils—and in the comments section of the survey head teachers report uncertainty about their school (particularly on questions related to the average performance levels of pupils)—their perceptions are likely to be relatively accurate on free-school meal take-up given it has an administrative component and a rough estimate of parental background. For each measure, I create a dichotomous treatment variable, with the top quarter of schools scored as 1, and the rest as 0. The thresholds for the two variables are above 20 percent free-school meal pupils for the former, and above 25 percent professionals for the latter. Because the

degree of standardization was low at this point I only directly measure school-level stratification.

For each panel member, I examine a range of individual covariates that are likely to affect selection into different types of schools; these include parental income, parental class, cognitive skills, sex, a measure of parental authoritarian attitudes, and the geographic region of residence. Income, class, and cognitive skills are measured when the children are 10 (and still in primary school), thus at the stage in which they are selecting into secondary school in most parts of the country. Children's cognitive abilities are measured through the Edinburgh reading test and a math test given at age 10, which I average. Income is measured as the total weekly family income (measured in seven grouped categories) and class is measured by the father's occupation (mother's if missing).<sup>4</sup> Finally, parental attitudes are measured at age five, producing an aggregate "authoritarian" values score, with higher scores representing more liberal views, based on a variety of parental attitudes toward gender equity and children rearing.

In order to estimate the impact of schools, I use coarsened exact matching techniques (Blackwell, Iacus, King, & Porro, 2009), matching respondents on the aforementioned covariates and then running a series of logistic regressions using the matching weights on adult attitudes. The results are similar when using propensity score matching.

In later waves, at ages 29 and 42, the BCS asks respondents a series of attitudinal questions, including perceptions of the economy and attitudes toward redistribution. I use two measures to capture attitudes toward redistribution (testing hypothesis 1) and two measures of preferences toward social fairness (testing hypothesis 2).

First, to measure attitudes toward redistribution, I look at a standard question asking: "Government should redistribute income from wealthier to poorer," again on a five-point scale from agree to disagree, which I dichotomize along the aforementioned lines, so that higher values imply more pro-redistributive attitudes. I also look at voting for the Conservative party in the last election, with Conservative voters scored as 1, and all other voters scored as 0 (nonvoters are excluded). While voting Conservative is not necessarily an attitudinal item, it taps into a related stance toward taxing and spending (Marshall, 2016a).

Second, on economic fairness, the BCS asks respondents whether: "There is one law for the rich and one for the poor," which taps an underlying perception of an "even playing field." Respondents can answer on a five-point scale from strongly agree to strongly disagree. I dichotomize the answers, scoring strongly agree and agree as 1, and neither to strongly disagree as 0. To get at political fairness, I use the following question: "Politicians are mainly in politics for their own benefit," which runs on the same five-point scale. I dichotomize this variable in the same way as above. In both cases, higher values signify lower belief in social and political fairness.

These school-level analyses aim to test whether being educated in a stratified high affluence school or a stratified high deprivation school has any long-run effect on attitudes toward social fairness or economic redistribution over and above the correlates of selecting into these school.

To see whether *policies* toward stratification have any independent effect beyond these school effects, I examine additional differences between selective and nonselective schools. Here I compare the effects of attending a selective grammar school to a group of similar children attending a nonselective school (comprehensive). I engage in the same exercise comparing nonselective schools in counties with grammar schools (former secondary moderns) with another group of similar children attending nonselective comprehensive schools.

Because the number of respondents in the panel who have data from the head teacher survey is limited (there are only 2,383 English respondents at age 29 and 2,118 at age 42), and only a small proportion attended selective grammar schools (113–117) in this sample, I use a self-report of the type of school attended. For grammar schools, I code all adults who report attending a grammar school as one. In order to compare grammars to non-grammars in the matching exercise, I need to compare similar students who had the option of a grammar school in one place to those that did not in another. As such, I restrict my comparison to pupils in comprehensive schools that come from regions with fewer than 10 percent grammar schools (i.e., pupils in a selective school in Kent are not being compared to pupils in a comprehensive school in Kent, but to matched pupils in a nonselective county). The later data on the overall composition of academic selection in a region is taken from the 1986 school census and matched to the 1986 county of residence of the BCS respondent. For former secondary moderns, the schools that are the less-selective counterparts to selective grammars, I also rely on a self-report, scoring those that attended secondary moderns and those that attended comprehensive schools in counties where more than 10 percent of schools are selective as a 1, and comprehensives in non-selective counties as a 0. In sum, for this analysis I am comparing matched groups of respondents in environments stratified by policies of selection, to similar ones in environments not stratified explicitly by policy.

#### 4.2. *Stratification at the School Level*

I start by examining whether schools themselves matter for adult attitudes using the BCS data—testing hypotheses 1 and 2. As outlined above, Section 4.3 then turns to examining whether the *policies* shaping these inequalities have an independent effect on attitudes, testing hypothesis 3a.

To begin with the school effects. I use coarsened exact matching techniques drawing on a range of pretreatment variables. While the matching exercise does not include all possible attributes shaping selection into schooling—it can only match on observables—it nonetheless brings us closer to isolating the effect of the schooling environment. Table 1 shows the unmatched and matched descriptive statistics for stratified high-deprivation schools (high-FSM), and affluent (high-professional) schools, grammar schools, and secondary moderns.<sup>5</sup> The matching exercise reduces, but does not eliminate, differences in the balance of the samples; thus, I include the covariates in the regression analysis.<sup>6</sup>



**Table 1.** Unmatched and Matched Differences in Means across Groups of Schools

	High Professional Before Matching		High Professional After Matching		High Free School Meals Before Matching		High Free School Meals After Matching	
	No	Yes	No	Yes	No	Yes	No	Yes
Female	0.53	0.57	0.58	0.58	0.55	0.53	0.54	0.54
Income (weekly £)	<b>122</b>	<b>165</b>	159	161	<b>134</b>	<b>115</b>	110	111
Parents workers	<b>0.18</b>	<b>0.09</b>	0.07	0.07	<b>0.15</b>	<b>0.24</b>	0.19	0.19
Cognitive score	<b>42.7</b>	<b>49</b>	48	48	<b>44.7</b>	<b>41.9</b>	41.7	41.7
Parents authoritarian	<b>-0.03</b>	<b>0.36</b>	0.33	0.37	<b>0.11</b>	<b>-0.07</b>	-0.05	-0.08
Southeast	<b>0.16</b>	<b>0.3</b>	0.25	0.25	<b>0.24</b>	<b>0.10</b>	0.07	0.07
London	0.05	0.06	0.02	0.02	0.06	0.05	0.03	0.03
N (Age 29)	1,853	487	891	362	1,840	735	1,027	601
	Grammar Before Matching		Grammar After Matching		Secondary Modern Before Matching		Secondary Modern After Matching	
	No	Yes	No	Yes	No	Yes	No	Yes
Female	0.53	0.53	0.58	0.59	0.54	0.55	0.54	0.55
Income (weekly £)	<b>127</b>	<b>151</b>	157	170	<b>127</b>	<b>122</b>	<b>127</b>	<b>122</b>
Parents workers	<b>0.19</b>	<b>0.13</b>	0.07	0.03	0.18	0.2	17.5	19.5
Cognitive score	<b>43.3</b>	<b>50.6</b>	<b>48</b>	<b>53.5</b>	<b>43.9</b>	<b>40.6</b>	<b>44</b>	<b>41</b>
Parents authoritarian	<b>0.04</b>	<b>0.37</b>	0.34	0.48	<b>0.05</b>	<b>-0.07</b>	<b>0.05</b>	<b>-0.07</b>
Southeast	<b>0.15</b>	<b>0.27</b>	0.25	0.37	<b>0.19</b>	<b>0.21</b>	0.19	0.21
London	0.09	0.07	0.02	0.07	<b>0.08</b>	<b>0.05</b>	<b>0.09</b>	<b>0.05</b>
N (Age 29)	5,601	426	4,753	381	4,362	1,357	3,230	997

Bold text indicates that differences in means are significant at the 5% level.

Tables 2a and 2b present the results of the first analysis, testing hypotheses 1 and 2 by examining whether attending more stratified schools has long-run effects on attitudes at age 29 compared to matched respondents who attended less stratified schools (the supporting information appendix shows attitudes at age 42). The first column for each outcome variable shows the results without matching, using the background factors as control variables. The second column includes models using the matching weights, and the third columns includes models using both the matching weights and the individual controls.

Table 2a shows that adults who attended a stratified high-deprivation (high FSM) school are much less likely to view society as fair than similar respondents who attended a less stratified school (Columns 1–3). The coefficients are signed in the anticipated direction for political fairness, but not significant (Columns 4–6). Adults from high-deprivation (high-FSM) schools were not more likely to support redistribution (Columns 7–9) but were less likely to vote Conservative than similar respondents (Columns 10–12).

Table 2b conducts the same analysis on respondents who attended stratified affluent schools. At age 29, adults educated in such schools are more likely to disagree that there is one law for the rich and another for the poor—i.e., they are less likely to perceive economic unfairness (Columns 1–3)—and are less likely to see politicians as self-interested (Columns 4–6). Although these respondents are not

Table 2. Results for Stratified Affluent and Non-Affluent Schools

	Rich Law		Politicians		Redistribution			Vote Conservative				
	1	2	3	4	5	6	7	8	9	10	11	12
	Unmatched w/cntrl	Match w/o cntrl	Match w/cntrl	Unmatched w/cntrl	Match w/o cntrl	Match w/cntrl	Unmatched w/cntrl	Match w/o cntrl	Match w/cntrl	Unmatched w/cntrl	Match w/o cntrl	Match w/cntrl
(a) Non-Affluent Schools (High Free School Meals)												
High FSM	0.40** (0.13)	0.43** (0.15)	0.43*** (0.15)	0.13 (0.12)	0.12 (0.15)	0.13 (0.15)	0.15 (0.12)	0.03 (0.15)	0.03 (0.15)	-0.56** (0.18)	-0.56** (0.22)	-0.58** (0.22)
Log pseudo-likelihood	-921.6	-670.13	-662.79	-959.9	-695.7	-676.7	-963.9	-694.9	-689.1	-528.36	-362.6	-354.49
N	1,451	1,028	1,028	1,451	1,028	1,028	1,451	1,028	1,028	914	637	637
(b) Affluent-Schools (High Professional)												
High Prof.	-0.47** (0.16)	-0.37* (0.2)	-0.37* (0.21)	-0.29* (0.15)	-0.49** (0.2)	-0.50** (0.21)	-0.2 (0.15)	-0.28 (0.2)	-0.28 (0.21)	0.44** (0.2)	0.60** (0.25)	0.61** (0.26)
Log pseudo-likelihood	-822.12	-500.0	-491.9	-861.46	-553.1	-541.8	-858.18	-858.2	-527.0	-477.0	-307.6	-299.78
N	1,300	820	820	1,300	820	820	1,300	820	820	813	520	520

\*Significant at the 10% level.  
\*\*Significant at the 5% level.

more likely to dislike redistribution (Columns 7–9), they are more likely to vote Conservative (Columns 10–12).

In sum, Tables 2a and 2b show mixed evidence for hypothesis 1. There are no clear long-run effects of school type on redistributive preferences in any specification. However, there are strong and consistent effects on choosing to vote Conservative. Those educated in more stratified affluent schools are more likely to vote Conservative than similar students in non-stratified schools. By contrast, those in more stratified high-deprivation (high-FSM) schools were less likely to vote Conservative than students of a similar background in less stratified environments.

In terms of attitudes about fairness, here we see more consistent long-run school effects in line with hypothesis 2. There are stark differences between those educated in more stratified environments on the question of economic fairness than others with similar background. Those in high-deprivation (high-FSM) schools were more likely to see society as unfair, whereas those in high-affluence schools saw society as fairer and politicians as less self-interested. Overall, more stratified schools do seem to have a long-run impact on basic beliefs about fairness and voting, but not redistribution. These results weaken by age 42 (see supporting information appendix), although whether this weakening is an aging or a period effect (related to the financial crisis) is difficult to tell.<sup>7</sup>

Because the attitudinal items examined here were not asked when respondents were 16, it is hard to know whether these differences were already emergent during the school years or whether a dynamic closer to that which Shedd witnessed is at play, where those in stratified schools are less aware of their advantages/disadvantages. There is evidence that at age 16, respondents across schools were less distinct in their beliefs about whether it is “Possible to get job if really determined” across schools than these same respondents were as adults about economic fairness (supporting information Appendix A.4). These teenage attitudes about job possibilities also do not strongly predict adult perceptions, hinting that differences in schooling experiences may not harden until students leave school and enter adulthood.

Finally, the results above do not distinguish between material and interpretive effects. I rerun the above results using a post-treatment variable, a standardized measure of gross pay at age 29. The inclusion of gross pay does not dramatically change the school effects in most models (although the effects of high affluence schools of attitudes on fairness is no longer significant), suggesting that both effects are likely in play (supporting information Appendix A.3).

#### *4.3. Do Policies Toward Stratification Matter?*

The above section showed that the social stratification of schools is associated with long-run attitudinal effects. I now turn to examining whether policies shaping stratification, here academic selection, have an independent effect, testing hypothesis 3a. I match pupils in selective grammar schools to similar pupils in comprehensive schools, and secondary modern pupils to similar pupils in comprehensive schools, aiming to test the additional effects of policies toward stratification.

Tables 3a and 3b follow the same approach as 2a and 2b, and show very weak policy effects. Those who attended grammar schools are less likely to see society as fair (Columns 1–3), although the significance of these results varies across specifications. Grammar schools are not associated with more beliefs in political fairness (Columns 4–6), nor with less pro-redistribution attitudes (Columns 7–9) nor a stronger Conservative vote (Columns 10–12). When we turn to secondary moderns, the flipside of grammar schools, we also see weak results. Although the coefficients are signed in the anticipated directions, none are significant.<sup>8</sup> In neither the unmatched nor the matched analysis are selective school experiences a strong predictor of future attitudes, beyond some limited effects on beliefs in social fairness.

Based on this analysis, there is little evidence that *policies* toward stratification affect attitudes in ways that are additional to the effects exerted by the actual school experience. This outcome is likely because parental choices play a similar role to policies of academic selection in bringing together pupils those from privileged background, with policies related to stratification having less of a direct impact once this is accounted for.

To show this outcome visually, Figure 3 plots the simple unmatched means for respondent attitudes on four attitudinal items at age 29 and 42 by school type (including private schools, which are excluded from the above analysis). The unmatched data show similar patterns to the matched data. Respondents from highly stratified schools due to policy (grammars/secondary moderns) look very similar to those stratified due to behavior (high affluence/deprivation comprehensives). This finding suggests that while school stratification matters (hypotheses 1 and 2), the policies directly shaping stratification (here, academic selection) have a less direct effect than anticipated by hypothesis 3a, in part because parental sorting remains even in areas without selective policies.

#### 4.4. Testing Standardization: The BHPS-UKHLS Analysis

The previous section showed that the level of stratification in schools matters for social and political attitudes, but that long-standing policies that reinforce or push against stratification have only a limited direct impact on attitudes. What about policies of standardization?

Quality in education is multi-dimensional and thus the standardization of quality is difficult to conceptualize, let alone measure. Despite this complexity, the use of academic performance measures captures an important element of the standardization of the educational environment. Students that leave school without 5 A\*-C GCSEs, as many did in the early 1990s, have reduced labor market opportunities, and are effectively excluded from many high paying jobs. Thus, the compression of academic performance in terms of GCSE results has potential consequences for students in terms of anticipated material effects. This standardization of performance also follows partly from explicit policies targeting low-performing schools through inspection, intervention, and resources. Given the argument here, this targeting is also likely to affect how students in schools perceive their opportunities. Thus, while

Table 3. Grammar and Secondary Modern Schools

	Rich Law			Politicians			Redistribution			Vote Conservative		
	1	2	3	4	5	6	7	8	9	10	11	12
	Unmatched w/cntrl	Match w/o cntrl	Match w/cntrl	Unmatched w/cntrl	Match w/o cntrl	Match w/cntrl	Unmatched w/cntrl	Match w/o cntrl	Match w/cntrl	Unmatched w/cntrl	Match w/o cntrl	Match w/cntrl
<i>(a) Grammar Schools</i>												
Grammar	-0.15 (0.15) -1879	-0.30* (0.18) -1081	-0.31* (0.18) -1051	0.02 (0.15) -1960	0.04 (0.17) -1123	0.05 (0.17) -1109	-0.11 (0.15) -1949	-0.19 (0.17) -1118	-0.19 (0.17) -1109	0.23 (0.19) -1057	0.24 (0.22) -690	0.22 (0.22) -672
Log pseudo- likelihood												
N	2,917	1,665	1,665	2,917	1,665	1,665	2,917	1,665	1,665	1,852	1,078	1,078
<i>(b) Former Secondary Moderns</i>												
Sec Mod.	0.07 (0.09) -2174	0.02 (0.1) -1838	0.02 (0.1) -1805	-0.02 (0.09) -2244	-0.11 (0.1) -1880	-0.11 (0.1) -1842	-0.02 (0.09) -2237	-0.04 (0.1) -1852	-0.04 (0.1) -1836	0.13 (0.12) -1210	0.08 (0.14) -1003	0.1 (0.14) -980
Log pseudo- likelihood												
N	3,343	2,757	2,757	3,343	2,757	2,757	3,343	2,758	2,758	2,120	1,745	1,745

## Percent Agreeing at Age 29 and 42, by School Type

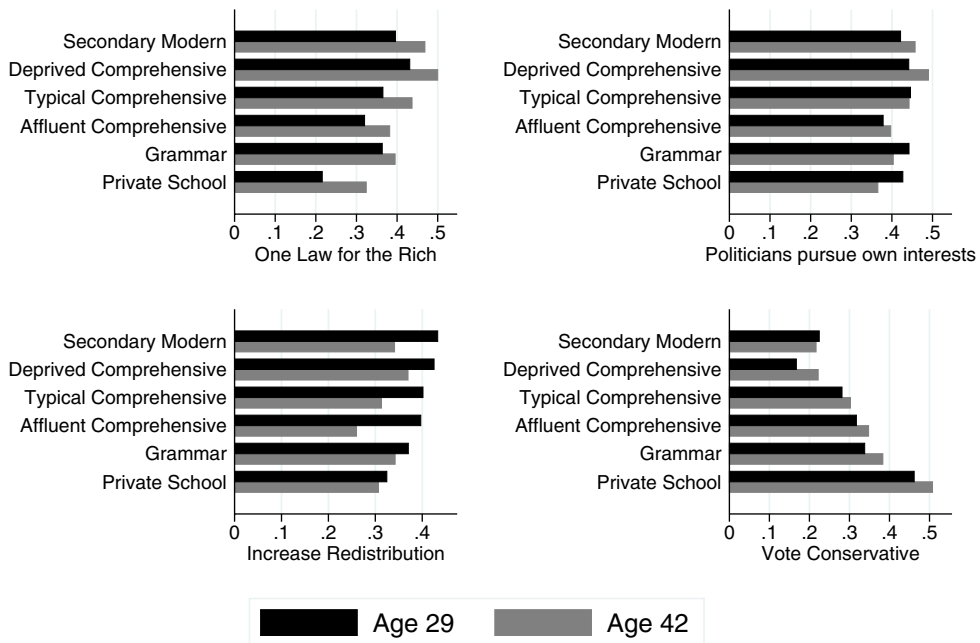


Figure 3. Average Attitudes at 29 and 42 by School Attended.

not exhaustive of quality standardization, GCSE performance measures capture an important element of standardizing change in England.

As shown in Figure 2, across England, mean school-level performance increased through the 1990s and 2000s, with a declining coefficient of variation. These patterns, though, occurred differently across place, with some local authorities having much more standardized school systems than others. In the period examined here, local authorities in the 95th percentile of performance had a mean GCSE over two times higher than those in the 5th percentile, and those in the 95th percentile of local variation had a coefficient of variation five times larger than those in 5th percentile of local variation (Gingrich & Ansell, 2014).

I thus use cross-sectional differences in performance levels and over-time performance variation, and the broad over-time changes, as policy-based measures of standardization, examining whether increases in standardization across place or time, are associated with reducing differences across respondents in terms of their beliefs in economic redistribution or social fairness (hypothesis 3b).

In order to see whether this standardization matters for attitudes, the following analysis uses the BHPS and its successor survey the UK-Household Longitudinal Survey (UKHLS) (University of Essex 2018). The BHPS is a high-quality panel study that has been running since 1991, and unlike the BCS, surveys multiple cohorts, asking parents and teenagers similar questions. The design of the BHPS means that I



have about 3000 unique respondents in England with parental background data and both a teenage and adult observation (although the number answering any given item is often lower). This panel offers strong possibilities to disentangle school or geographic effects from family effects. Its disadvantage, however, is that until the most recent cohorts (children born in the late 1990s), it is not possible to match children to their actual schools, only their local areas, and it has a much more restricted range of attitudinal variables.

There are two ways to think about how policy standardization might affect attitudes. First, people can experience changes directly, through their schooling years. Since most British children start secondary school at age 11 and remain in the school until 18, many directly experienced rising standards. However, very few panel respondents have multiple observations through the teen years; moreover, it is not clear that one would expect 11-year-olds to have well developed attitudes (despite these limitations, I consider this approach in supporting information Appendices A.5 and A.6).<sup>9</sup>

Second, since children may not understand that they are experiencing change in education, an alternative way to think about the effects of changing standardization is not as something directly experienced, but something that cohorts experience. We might not expect changes in an individual as their school improves, but changes across similar types of individuals socialized in more or less standardized environments.

I follow this second approach. I collapse the panel into just two observations per respondent—an average of their childhood attitudes (from age 15 to 18) and adult attitudes (above age 18). I analyze children and adults as separate groups (i.e., I am not looking at the change from childhood to adulthood).

For both children and adults, I regress their attitudes on a range of individual covariates and two measures of standardization. In terms of attitudinal items, there are very few items asked in the BHPS. The BHPS asks an identical question to the BCS in terms of “one law for the rich,” and I dichotomize this variable as above. This question was asked in 1991, 1993, 1995, 1997, 2000, 2004, and 2007, giving a wide time coverage. Second, as with the BCS, I look at Conservative party choice, which is asked in all waves.

To capture standardization, I first break apart the cohorts by age, looking at those who turned 18 before 1997 those who turned 18 after 1997. I use 1997, the date of New Labour’s election, as a cut point, because of the salience it brought to educational performance improvement. Concretely, I am comparing children educated in the 1990s to children in the late 1990s and early 2000s, and adults educated in the 1990s to those educated in the post-1997 period.

Next, to move beyond simple cohort differences, I also include a second measure of standardization, this one from the respondents’ local educational context. I match each respondent to characteristics of their local authority at age 15: the overall GCSE scores (the percentage receiving 5 A\*-C marks) and the coefficient of variation for GCSE scores across schools within a local authority. These measures capture the average performance of the local area, and the degree of dispersion in scores within the area. While the local authority in England is a large unit, upwards of one

hundred thousand citizens (or more in large urban areas), analysis using smaller ward-level data yields similar results, albeit with substantial missing data. As outlined above, there are contradictory tendencies in England, with policies pushing decentralization to the school level even during this period of general standardization. To account for this trend, I also consider the percentage of non-LEA run schools in the local area, which captures the counter-standardization tendency. All three measures are calculated from an original dataset of all schools in England (Gingrich & Ansell, 2014).

If standardization matters as outlined by hypothesis 3b, we would expect that for similar types of respondents (i) those in the post-1997 cohorts will be systematically less likely to see society as unfair and engage in more Conservative voting than pre-1997 cohorts and (ii) those educated in a local context with higher average performance and a lower coefficient of variation to see society as less unfair and engage in more voting for Conservatives compared to other people with the same age and background educated in less standardized environments.

To test these claims, I run a series of linear models. The collapsed data averages attitudes, so the outcome variables run continuously from zero to one. I control for parental attitudes, logged parental income averaged through the teenage years, sex, ethnicity (coded as white British or not), and school type. I include one contextual control, the percentage of working age adults claiming state benefits, a measure that captures local unemployment (NOMIS, 2014). I exclude respondents in local authorities with a sole school. I include regional fixed effects.

#### 4.5. *Testing the Effects of Standardization*

As outlined above, for each attitudinal item I run two analyses, one for children (under 18), looking at average attitudes through the teen years, and adults' (post-18) average attitudes through the adult observations. The results are presented in Table 4. In each, the variables of interest are the geographic measures of standardization (average GCSE performance and variation in performance) and the post-1997 dummy, which measures whether the respondent is in school before or after 1997.

Column 1 starts with the analysis of children's attitudes toward economic fairness (the "rich law" question). We see that for children, the father or mother's response to the same survey item and family income dominate all other features. There are some school-level differences—compared to the baseline group in comprehensive schools, those in grammar school are associated with *higher* belief in unfairness (significant at the 10-percent level) whereas being in a private school is associated with less belief in economic unfairness.

The measures of geographic standardization do not have strong effects, as neither mean performance nor variation in performance, or the local de-standardization variable (percent non-LEA schools), is significant for beliefs about unfairness. The coefficient on post-1997 is negative and statistically significant, meaning that generically, children in the post-1997 period are less likely to agree that there is economic unfairness. Collectively, Column 1 shows that more recent cohorts of children

Table 4. Examining Standardization—BHPS Cohort Analysis

	(1)	(2)	(3)	(4)	(5)	(6)
	Rich Law Under 18	Rich Law Over 18	Rich Law Under 18 Low Inc.	Rich Law Over 18 Low Inc.	Conservative Under 18	Conservative Over 18
Sex	0.04 (0.03)	-0.04 (0.03)	0.08* (0.04)	-0.03 (0.05)	-0.02 (0.02)	0.00 (0.02)
White	-0.01 (0.06)	0.11 (0.07)	-0.02 (0.08)	0.19* (0.09)	0.07* (0.04)	0.10** (0.04)
Income (15)	-0.08*** (0.03)	-0.05* (0.03)	-0.07 (0.05)	-0.03 (0.06)	0.03 (0.02)	0.02 (0.02)
Parents' views	0.17*** (0.03)	0.10*** (0.04)	0.21*** (0.05)	0.08 (0.06)	0.61*** (0.03)	0.45*** (0.03)
Sch. = Modern	-0.01 (0.04)	-0.01 (0.04)	-0.07 (0.06)	-0.02 (0.07)	-0.04 (0.03)	-0.05* (0.03)
Sch. = Gramm.	0.11* (0.07)	0.08 (0.07)	-0.00 (0.09)	0.08 (0.10)	0.11** (0.05)	0.02 (0.06)
Sch = Private	-0.15** (0.06)	-0.18*** (0.07)	-0.30*** (0.09)	-0.23** (0.11)	0.14** (0.06)	0.18*** (0.06)
GCSE (15)	-0.00 (0.00)	-0.00 (0.00)	-0.01 (0.00)	-0.01** (0.00)	0.00 (0.00)	0.00* (0.00)
Variation (15)	-0.04 (0.12)	-0.08 (0.12)	0.09 (0.17)	-0.24 (0.16)	0.02 (0.10)	0.11 (0.10)
Non-LEA (15)	0.00 (0.08)	-0.14 (0.10)	0.15 (0.13)	0.04 (0.15)	0.08 (0.07)	0.03 (0.08)
Claimants (15)	-0.00 (0.01)	0.01 (0.01)	-0.01 (0.01)	-0.01 (0.02)	0.02** (0.01)	-0.00 (0.01)
Post-1997	-0.09** (0.04)	-0.05 (0.04)	-0.14** (0.06)	-0.11* (0.07)	0.02 (0.03)	0.00 (0.03)
Constant	1.27*** (0.32)	0.97*** (0.37)	0.90 (0.59)	1.11 (0.69)	-0.58** (0.24)	-0.40 (0.27)
Observations	1,180	892	538	411	875	1,005
R-squared	0.08	0.06	0.13	0.10	0.48	0.35
Region FE	Y	Y	Y	Y	Y	Y

\*Significant at the 10% level.

\*\*Significant at the 5% level.

\*\*\*Significant at the 1% level.

are more likely to view society as fair; but there are no real differences in attitudes among children being educated in high or low performance local areas, or high or low variation areas, once parental attitudes and income are controlled for. The policy environment is less important than either the family or global shifts in cohort attitudes.

Column 2 examines the same specification for adults. Again, parental attitudes and childhood income exert a strong effect on adults' attitudes. Adults who attended private schools are less likely to believe there is economic unfairness, but those who went to grammar or secondary moderns are not significantly different from those that attended comprehensive schools. For adults, the post-1997 dummy is signed in the same direction, but not significant, suggesting that adults educated post-1997 are not significantly different from those educated before (although, the sample is small). None of the local educational variables (the experiences at age 15) are significant. In this regard, column 2 shows even weaker policy effects than column 1, with few systematic differences cross-place or cross-time in attitudes.

Columns 4 and 5 restrict the sample to those raised in families with income below the mean, the group most like to be affected by standardization, since performance improvements in England were particularly pronounced for schools with lower income pupils. For lower-income children still in school (Column 3), we see similar patterns to the full sample. Cohort differences remain in place for this subgroup, but the local education variables are not strongly related to attitudes.

For adults raised in low-income families (Column 4), however, we see a weaker effect of family background variables than for the full sample. Again, those that attended private school are significantly less likely to see economic unfairness, and there are cohort differences (although only significant at the 10 percent level). Here, though, we see that those raised in high performance environments are less likely to see society as unfair. Contra expectations, there are also hints that those raised in high variation environments are also less likely to see society as unfair, although this result is not significant.

When it comes to Conservative party support among children (Column 5), we see that parental Conservative support is a dominant force. Children in private schools and grammar school are, not surprisingly, more likely to support the Conservative party. Here, there are no major cohort differences, and the local standardization variables do not have a strong effect (except, somewhat surprisingly, living a place with a higher number of benefit claimants).

In adulthood (column 6), we see similar effects, but here, there is a stronger effect of having been socialized in a higher performing area, as these respondents are more likely to vote Conservative (significant at the 10 percent level). None of these effects are different for the post-1997 cohort.

Overall then, we see limited evidence of policy feedback on cohort-experienced change in standardization, contra hypothesis 3b. Overall, beliefs in unfairness were falling through this period and cohorts have distinct attitudes. Although these differences may be related to education, many other features of British society were also changing for children educated post-1997, meaning that these cohort effects are suggestive evidence at best. The direct measures of schooling standardization

at the local level have only limited effects, with local variation not directly related to preferences and local mean scores only having a small negative relationship with attitudes toward unfairness for the low-income sample.

Whether policies of standardization fail to have feedback effects on citizens, or the BHPS sample is simply too small to uncover them, is hard to say. However, average GCSE A\*-C performance nearly *doubled* through the time period considered here in some local authorities (from under a third of students completing secondary at the required level to over two-thirds), thus even if these policies did feed back into attitudes in ways that are not being captured here, this process is certainly much weaker than the changes in performance.

Partly, this lack of attitudinal feedback effects may be explained by returning to the theory developed in Section 2 about the relative importance of parental behaviors vis-à-vis policy. Even though policies changed through this time period in England, and on paper performance become much more equal, parents continued to sort in schools and inequality in British society remained high (Gingrich & Ansell, 2014), meaning that schools remained quite stratified. This underlying differentiation is shown in Table 2. When testing rules were changed to encourage more performance variation, it re-emerged rapidly, showing the limits of early reforms that increased standardization were in terms of reorienting the system as a whole.

## 5. Conclusion

Do schools matter for economic attitudes? This paper suggests that the social structure of the school matters a great deal, but that the policies attempting to shape that social structure have a weak impact. These results come from a single country, and one in which class dynamics are particularly entrenched, but they nonetheless provide empirical and theoretical insights that may travel beyond this case.

Analysis of the British Cohort Survey, with detailed data on school type, and the British Household Panel Survey, with an extended panel sample, revealed that the social stratification of schools in the UK did appear to shape beliefs about social fairness and support for the Conservative Party. However, the education policy environment had limited direct effects on attitudes. Both policies that changed stratification between schools—such as the development of grammar schools—and those that changed standardization across schools—particularly the upward convergence of exam results during the New Labour period of government—had remarkably weak effects on the attitudes of students who experienced those systems, both at the time and later as adults.

Theories of policy feedback often focus on the ways in which policies can systematically shape the experiences that citizens have of the state. These experiences, however, are embedded in broader social and economic structures in which other actors can push back against policy. Equalizing policy in schools is deeply contentious. From desegregation in the United States to policies aimed at reducing class privilege in the United Kingdom, the actions of parents and schools on the ground does not always match policy intentions. This tension means that moving from policies to student experiences to beliefs is a complex process, and one in which policy makers have imperfect levers to shape parental behavior let alone the “hearts and minds” of

pupils. The study of schooling should remind us that policy feedback effects never occurs in a vacuum, nor do they operate unidirectionally. Instead, the implementation and experiences of policies can remain open to contest, shaping the way citizens relate to the state in ways that may not be fully intended or controlled by the policy.

These claims however, should not be read as suggesting that education policy is unimportant. The study of school effects show that the practice of less stratified schooling does have different long-run effects that differs from stratified affluent and non-affluent environments. While policymakers may be limited in the short-run in shaping these schooling experiences, and this paper certainly suggests that they cannot mechanically control them through policies of stratification and standardization, in the long run, comparative research and education and social mobility suggests that policies can matter, particularly where these policies work with other changes in society that expand social equity.

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1. A number of experimental studies show some independent effects of education on turnout (Sondheimer & Green, 2010) while other work suggests limited effects (Berinsky & Lenz, 2011; Kam & Palmer, 2008; Persson, 2014). Scholars also contest the causal impact of civic education on long-run behaviors (Neundorff, Niemi, & Smets, 2016; Niemi & Junn, 2005).
2. Education policy in the UK is decentralized to the constituent countries, and the analysis here focuses on England, excluding Scotland, Wales, and Northern Ireland.
3. The 1944 Education Act in the UK established a so-called tripartite model of schooling, with secondary schooling split between academic grammar schools and less academic secondary modern and technical schools. Admission to grammar schools is based on a selection exam at age 10/11. Most local authorities phased out mass academic selection in the 1960s and 1970s moving to an all ability comprehensive school model, but some maintained selection.
4. Class is measured in groups: student, unskilled, partly skilled, skilled manual, skilled non-manual, managerial/technical, and professional. I dichotomize this variable into a "working class" variable, with unskilled, partly skilled, and skill manual coded as workers.
5. The multivariate L1 distance (a measure of imbalance) falls from 0.76 to 0.68 for the FSM schools group, 0.81 to 0.76 for the high professional schools group, 0.91 to 0.78 for the grammar schools group, and 0.86 to 0.77 for the secondary modern group.
6. Because moving toward closer matching reduces the sample size, I match for authoritarian values and cognitive scores on quintile ranges. Allowing closer matching substantially reduces imbalance, but the results are substantively quite similar with one exception: the coefficient on high-FSM schools falls below significance in the vote-choice models.
7. At age 42, the impact of attending a high FSM school on attitudes about social fairness is substantially weaker than at 30, while there is a stronger effect on attitudes about political fairness (see supporting



information Appendix Tables A.1 and A.2). The effect of high- affluence schools loses statistical significance at age 42, although the coefficients are signed in the same direction.

8. There is some mixed evidence that at age 42 there is a stronger relationship between attending grammar schools and believing in social fairness and voting (see supporting information Appendix A.2).
9. In order to investigate the possibility that directly experienced change does matter, I run two individual-level fixed effects models, examining levels of local performance over time and the change in performance since the child turned 11. I restrict both models to respondents who have not moved cities during the panel. I look at both children only (15–18-year-olds) and a sample including young adults who did not directly experience policy changes. Since most confounders are time invariant, in the fixed effects models, I include only age and student status as controls. These are outlined in supporting information Appendices A.5 and A.6, and in line with the results below, find no substantial evidence of policy effects.

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