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ABSTRACT

This paper describes education reforms over the past 25 years, considering the consequences of earlier state-level standards. Most states introduced stricter, state-level standards for high school graduation in response to highly publicized concerns about student effort and public school quality. The key first wave of reforms consisted of a test-based performance standard, minimum competency testing (MCT), and a process standard, course graduation requirements (CGR) that mandated the amount of academic credits necessary in core academic areas. This paper describes these reforms and their implications for student outcomes, presenting new evidence on how they influenced such outcomes as educational attainment, labor market experiences, and high school curricula. Overall, the first wave of student-level standards have had positive and negative effects. For example, they led to reductions in educational attainments, particularly among black students. MCT led to reductions in curricular efforts. Higher CGR had pejorative effects on amounts of time spent doing homework and reading for pleasure. The standards also increased subsequent employment possibilities, and higher CGR were partly responsible for substantial academic upgrading of high school curricula. The paper recommends that ongoing public discussions about the desirability of centralized standards address how to value these reforms. (Contains 10 tables and 48 references.) (SM)

Standards and Student Outcomes: Lessons from the "First Wave" of Education Reform

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1. INTRODUCTION

The "No Child Left Behind" Act, signed into law by President Bush in January, mandates the development of test-based student assessments and holding schools accountable for their academic performance relative to their state's minimum threshold (Robelon 2002). This bipartisan legislation will promote, and shape, the accountability systems that have been introduced in almost every state over the last several years. The accountability policies recently adopted at the state level have taken several forms including the publication of "report cards" and ratings for schools, teacher evaluations coupled with merit pay and the legal authority for states to control or close failing schools (Meyer et al. 2002). An increasing number of states also hold students directly accountable by withholding grade promotion or high school graduation for low performance on tests. The impetus for output-based accountability has grown out of the widely held perception that the long-standing focus of prior reforms on educational inputs and processes has been relatively unproductive. In particular, proponents of accountability policies argue that we do not have reliable information about how to systematically use educational programs and resources to improve student outcomes. According to this line of reasoning, the idiosyncratic nature of educational production implies that output-based incentives provide a more reliable method for enhancing productivity (e.g., Hanushek and Raymond 2001). However, as suggested by the title of the recent Federal legislation, another clear intent of accountability programs has been to close the performance gap between advantaged and disadvantaged students, in particular the gap between white and minority students.

Critics of these policies suggest that explicit standards may actually exacerbate that performance gap, particularly in the absence of other systemic reforms related to local control, teacher training and available resources (e.g., Murnane and Levy 2001). Another major concern with standard-based reforms is that they may promote an undesirable narrowing of teaching styles and student curricula (Kohn 2001, Murnane and Levy 2001). A recent national survey of public school teachers suggests that those concerns could be well-founded (Doherty 2001). While a majority of surveyed teachers (87 percent) support establishing higher standards, nearly 70 percent also thought that their teaching overemphasized testing to the detriment of learning in other important areas (Doherty 2001). A large majority of these teachers also opposed using state tests as the sole basis for grade promotion and graduation. Nonetheless, there have also been some early indications that the states making the strongest recent gains in measured student achievement have been those that aggressively implemented new standards and assessments (Olson 2001). However, the proper interpretation of the recent experiences within particular states has been the subject of considerable controversy (e.g., Schrag 2000, Greene 2000) and many of the new state policies have been implemented too recently to be evaluated with currently available data.

The premise for this paper is that we may gain useful insights into these controversies by looking back to consider the consequences of earlier state-level standards. Over the last twenty-five years, almost every state introduced stricter, state-level standards for high school graduation in response to highly publicized concerns about student effort and the quality of public schools. The key "first wave" reforms consisted of a test-based performance standard, minimum competency testing (MCT), and a "process" standard, course graduation requirements (CGR) that mandated the amount of academic credits that must

be earned in core academic areas. Though the adoption of these reforms occurred with much fanfare, there has been surprisingly little study since then of their consequences. In this paper, I describe these reforms and what we currently know about their implications for student outcomes. I also present new evidence on how these reforms influenced a variety of student outcomes (e.g., educational attainment, labor market experiences, high school curricula) in empirical specifications that address the possible shortcomings of prior evaluations. I conclude by discussing what this broad set of results may contribute to our understanding of the currently evolving state and Federal experiments with standards-based reform.

2. THE "FIRST WAVE" OF EDUCATION REFORM

Much of the ongoing public interest in reforming public education can be traced to the mid-1970's and the widely discussed perception that student test scores and the quality of public schooling were in decline.[1] In particular, critics of that period emphasized that a high school diploma, once a significant and hard-earned, personal accomplishment, had been debased through the abuses of social promotion and the tolerance of low academic standards (e.g., Popham 1981). Politicians at the state level proved highly responsive to these concerns and began enacting a variety of new standards and regulations now known as the "first wave" of education reform.[2] The earliest manifestation of these centralized reforms was the widespread adoption of a test-based performance standard: minimum competency testing (MCT). Beginning in 1975, nearly every state introduced new MCT programs designed to assess students' basic skills (Pipho 1978). Most of these programs were simply intended to identify low-performing students and to direct them to sources of remediation. However, several of these states also mandated that students pass an MCT in order to graduate with a standard diploma. By 1992, the graduating high school seniors in fifteen states were required to pass an MCT (see Table 1).[3] Typically, students would first sit for these exams in the 9th or 10th grades and have multiple opportunities for re-tests. The conventional wisdom regarding these test-based diploma sanctions has been that they were "legislated as a lion but implemented as a lamb" (Catterall 1989). Specifically, the MCT standards typically required that students demonstrate basic math and reading skills at only an 8th or 9th grade level. Furthermore, in response to failure rates on

[1] Prior to this, the most extensive public discussions of education reform were occasioned by the Sputnik launch in 1957. Ironically, the discussions in the 1970s were motivated in part by the declines in Scholastic Aptitude Test (SAT) scores, a trend with ambiguous implications since an increasing proportion of students were likely to take the test (CBO 1986, 1987).

[2] These "top-down" reforms consisted of state-level standards and regulations that influenced teachers (e.g. licensing and salaries), schools (e.g., curricula, length of school day) as well as students (e.g. graduation requirements). In contrast, the subsequent, "second-wave" of reform stressed decentralized improvements like school-based management, teacher professionalism and school choice (Saban 1997). The second wave reforms are sometimes viewed as a response to the failure of the first wave (e.g., Chubb and Moe 1990).

[3] The enactment of this requirement was often delayed for several years in order to provide students with adequate notice, to allow the state to test and develop their assessments, to allow schools to adjust curricula and in response to court challenges (e.g., *Debra v. Turlington* in Florida). I identified the existence and effective date of MCT requirements for graduation by drawing on several sources including Jacobs (in press), Bishop and Mane (2001), publications from the Education Commission of the States and Lexis-Nexis searches of state newspaper accounts.

initial tests that were deemed politically unacceptable, these standards were sometimes lowered (Catterall 1989). As a consequence, the ultimate pass rates among high school seniors were extremely high (Serow, 1984). However, whether MCT has had a more substantial influence on dropout rates is an open empirical question since the attrition of discouraged students may make the ultimate pass rates misleading. Furthermore, a full consideration of MCT policies should also consider their effects on other outcomes that are relevant to all students (e.g., student curricula and labor market experiences).

The adoption of "first wave" reforms accelerated more dramatically in the early 1980's after the publication of several panel reports, which were highly critical of public education. The most widely discussed of these reports, *A Nation at Risk* (National Commission on Excellence in Education 1983), emphasized the need for higher expectations and standards for high school graduates. In particular, the report alleged that the combination of a "cafeteria-style curricula" and "extensive student choice" meant that too many students pursued a diffuse and unchallenging course of study. The report recommended that states respond with new high school graduation standards mandating a minimum amount of course taking in core academic areas. The report specifically recommended a "New Basics" curriculum requirement consisting of 4 years of English and 3 years each of social studies, science and mathematics.[4] Again, politicians proved highly responsive to the strong public interest in these policies. By 1992, nearly every state had increased its course graduation requirements (CGR) in the four core academic areas (see Table 1).[5] However, in all but three states (Florida, Louisiana and Pennsylvania), the new CGR fell short of the "4/3/3/3" standard recommended by *A Nation at Risk.*

3. STANDARDS AND STUDENT ACHIEVEMENT

The fundamental motivation for these two first-wave reforms (MCT and CGR) was simply to promote student effort and learning, making high school diplomas "worth the paper they're written on." Are new educational standards likely to have beneficial effects on student achievement? Prior commentators on this issue disagree sharply for reasons that are worth considering explicitly. Proponents of higher standards (e.g., Betts and Costrell 2001) make the straightforward claim that the "incentive effects" of such policies will raise the level of achievement among those students who would pass under a weak standard and choose to increase their effort to meet the new standard. Betts and Costrell (2001) also suggest that those students whose prior levels of effort would clearly imply failing or passing both standards (i.e., those at the top and bottom ends of the ability distribution) will not have any incentive to change their behavior. However, they also recognize that the incentive effects for some students who

[4] The amount of credit for a course is typically defined in terms of "Carnegie units," which represent a standardized amount of time spent studying a subject over a full academic year. One unit represents successful completion of a class which averages fifty minutes per day, five days per week for 180 school days (Lillard and DeCicca 2001). *A Nation at Risk* also recommended 0.5 Carnegie units of computer-related courses and 2 of a foreign language for college-bound students.

[5] State-year data on CGR were drawn from several sources, most of which were published by the Education Commission of the States.

marginally passed under weaker standards will promote discouragement and reduced effort. They recommend targeted policies to attenuate these losses.

However, the potential benefits of higher standards are not necessarily limited solely to those marginal students who choose to increase their effort. For example, Bishop (1999) discusses how a high, external standard can limit the "nerd harassment" and peer pressure that encourages high-ability students to shirk educational effort. It is also possible that standards can generate broader educational gains through general increases in educational expectations and school productivity. Furthermore, even those students who fully anticipate dropping out of high school may be compelled in the short term to greater educational effort through curricular mandates such as course graduation requirements (CGR). The "sorting effects" of higher standards may also lead to passive labor market rewards (Betts and Costrell 2001). Specifically, if educational attainment functions as a signal of unobserved individual ability, higher standards could increase the attractiveness to employers of all students by increasing the average level of ability among both dropouts and graduates.

The critics of standards-based reform emphasize the many negative consequences associated with the expected reductions in educational attainment. Furthermore, they note that the reductions in educational attainment are disproportionately likely to be among those whose poor socioeconomic priors make it unusually difficult to meet new standards. In particular, several observers suggest that higher standards will exacerbate the troublesome performance gaps between black and white students (e.g., Serow 1984, Murnane and Levy 2001, Philips and Chin 2001).[6] However, the critics of standards also suggest that these reforms will have other, pejorative effects that harm all students. For example, the introduction of high-stakes testing such as minimum competency tests (MCT) may lead to a narrowing of teaching styles and curricula (i.e., "teaching to the test") that comes at the expense of substantive learning (Murnane and Levy 2001). Furthermore, the establishment of minimum competency tests (MCT) and stricter course graduation requirements (CGR) may suggest to students that learning for its own sake is not worthwhile. In particular, these standards may encourage otherwise high-achieving students to avoid challenges and simply choose the path of least resistance to satisfying their requirements (Phillips and Chin 2001). Interestingly, the authors of *A Nation at Risk* made a similar allegation, suggesting that minimum competency tests (MCT) were inadequate since they would become "maximum" standards and lower expectations for high-ability students.

PRIOR EMPIRICAL EVIDENCE

The prior discussion suggests that there are fundamental, policy-relevant issues about the educational consequences of higher standards that can only be informed by empirical evidence. Given this, one might expect that the first-wave reforms have been subjected to exhaustive empirical evaluation.

[6] The consequences of higher standards for the racial gap in educational performance may actually be driven by more than simple differences in socioeconomic backgrounds. Higher standards and high-stakes testing may also harm minority students if they generate "stereotype threat": academic underperformance due to the risk of confirming negative stereotypes (Steele 1997, 1998).

Surprisingly, there is relatively little empirical evidence on the consequences of these policies that would allow us to sort through these conflicting theoretical predictions. Furthermore, what evidence is available is often directly contradictory. Most of the prior studies have focused on how minimum competency tests influenced student achievement as measured by test scores. For example, Frederiksen (1994) compared state-level NAEP scores before and after the introduction of minimum competency tests (i.e., 1978 and 1986). He found that math scores rose in "high-stakes" states (those that use MCT for grade promotion and graduation) relative to "low-stakes" states (those that have no stated use for their exams). Interestingly, these basic "difference-in-differences" comparisons also suggested that these achievement gains were concentrated among 9 and 13-year olds, not 17-year olds. However, both Bishop et al. (2001) and Jacob (in press) present evidence that residence in an MCT state is unassociated with either the average level or gain in student test scores.[7]

Most studies also find that residency in an MCT state has no apparent effect on the overall probability of completing high school (e.g., Catteral 1987, Lillard and DeCicca 2001, Bishop and Mane 2001, Jacobs in press). However, there is evidence that, for low-achieving students in MCT states, the chances of graduating from high school are lower (Bishop and Mane 2001, Jacobs in press). Bishop and Mane (2001) also evaluate the effects of minimum competency testing on the probability of entering college and on early labor market experiences. They found, among high-achieving students, a positive association between residence in an MCT state and the likelihood of entering college. They also find that the students from MCT states have subsequently higher earnings, even after controlling for their educational attainment.[8]

The evidence on how course-taking standards influenced achievement and educational attainment is more limited. Some studies find that the adoption of higher CGR led students to pursue more demanding and academic curricula (e.g., Clune and White 1992, U.S. Department of Education 1997).[9] However, studies attempting to identify the effects of new CGR on test scores have found no significant effects (e.g., Bishop et al. 2001).[10] The available evidence on how CGR influenced educational attainment is also mixed. Hoffer (1997) finds no effect on the probability of completing high school while Lillard and DeCicca (2001) report statistically significant, negative effects. Bishop and Mane (2001) also find negative associations between living in a state with CGR and the probabilities of completing high school and entering college. However, the increased propensity to drop out of high school appears to be offset by an

[7] However, Bishop (1998) and Bishop et al. (2001) presented evidence that *curriculum-based* exit exams do raise test scores. These evaluations relied on cross-national comparisons as well as other cross-sectional comparisons based on data from Canadian provinces and U.S. states.

[8] Furthermore, these gains appear among both high and low-achieving students. Similarly, Bishop and Mane (1999) found that attendance at an MCT high school was positively associated with wages and earnings.

[9] However, since these studies only considered the transcripts of *graduated* seniors or students with at least 16 Carnegie units, this evidence may merely reflect the reform-driven attrition of students who took relatively few academic courses.

[10] Furthermore, studies that exploit the variation in graduation policies across schools also find no effects on test scores (e.g., Hoffer 1997).

increased probability of acquiring a general equivalency degree (GED). They also find that, conditional on educational attainment, exposure to higher CGR are negatively associated with subsequent wage rates.

4. EVIDENCE FROM THE 1990 PUMS

Overall, what does this body of evidence suggest about the desirability of standards-based reforms? Bishop et al. (2001, page 312) argue that the comparative effects of CGR and MCT on educational attainment and labor market outcomes imply that MCT are a relatively attractive policy. However, these sorts of policy inferences may be premature for two reasons. First, almost all of the available empirical evidence motivating this conclusion relies effectively on cross-sectional comparisons of students who reside in states with different policies.[11] Since the considerable variation in individual educational outcomes across states reflects a variety of cultural, socioeconomic and political determinants, this approach may generate substantive biases. In particular, the unobserved but state-specific determinants of educational achievement are also likely to be associated with each state's propensity to adopt high school graduation requirements like MCT and CGR. Relying on a cross-state identification strategy to estimate the effects of these policies could, therefore, produce biases of an uncertain direction.[12] A second drawback of prior empirical studies is that they have not directly addressed claims about whether these graduation standards would be particularly harmful or beneficial to minority students. In this section, I present new evidence that addresses both of these concerns by relying on individual-level data from the 1990 Public Use Microdata Sample (PUMS).

DATA AND SPECIFICATIONS

The 1990 PUMS consists of approximately 12 million respondents (5 percent of the population) who completed the long form questionnaire to the decennial Census (U.S. Bureau of the Census 1993). One useful feature of the PUMS is that, because it contains individuals from multiple birth cohorts within each state, the resulting identification strategy can rely on the within-state variation in MCT and CGR instead of the cross-state variation. Furthermore, the large number of respondents in the PUMS implies increased statistical precision and, in particular, a better ability to detect race-specific responses to the new graduation standards. My extract from the PUMS data consists of the 1,348,766 white (non-Hispanic) and black respondents who were aged 18 between 1980 and 1988 and born in one of forty nine states (Ruggles

[11] One exception is the difference-in-difference comparisons reported by Frederiksen (1994). Another is Lillard and DeCicca (2001, page 467, model 4) who find that higher CGR increase the probability of dropping out. However, they do not address the effects of MCT in these fixed effect models.

[12] For example, one plausible conjecture is that the states most willing to adopt higher standards are those with relatively high-achieving students. However, it is similarly plausible to suspect the opposite: that the states with an unobserved propensity for low student achievement would adopt higher standards more aggressively.

et al. 1997).[13] Two of the outcome variables defined for each respondent identify educational attainment, a binary indicator for high school graduation (mean=0.858) and another for college entrance (mean=0.519).[14] I limited the sample to those who were at least 18 by 1988 because of the biases that could be generated by state-specific trends in the "incomplete spells" of high school completion and college entrance among cohorts that were younger at the time of the Census interview (Angrist and Evans 1999). The other dependent variables reflect the labor experiences of each PUMS respondent. One is a binary indicator for employment participation (mean=0.745), which is defined for all respondents.[15] The other is the natural log of average weekly wages, which is only defined for 1,143,352 respondents. This wage variable is the ratio of pre-tax wage and salary income reported for the previous calendar year and the corresponding number of weeks worked (mean=327).

The basic specification used for regression models based on these data is:

$$Y_{ist} = \beta \mathbf{X}_{ist} + \gamma \mathbf{Z}_{st} + \mu_s + \nu_t + \varepsilon_{ist}$$

where Y_{ist} is the dependent variable and the matrix, $\mathbf{X}_{ist}$, includes observed, individual-level traits. In most models, these controls simply include binary indicators for race and gender. However, in the models for labor market outcomes, these controls also include measures of educational attainment (i.e., separate dummy variables for high school graduates, those with some college and those with bachelor degrees) and a dummy variable for whether the respondent attended school within the last year.[16] The terms, μ_s and ν_t, represent fixed effects specific to each state of birth and year of birth. The term, ε_{ist}, is a mean-zero random error.[17]

The matrix, $\mathbf{Z}_{st}$, includes determinants that were specific to the birth cohorts within each state. These determinants include the two independent variables of interest: dummy variables that reflect the state MCT and CGR policies in effect for each birth cohort at age 18. One dummy variable simply indicates

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[13] Respondents born in Nebraska were omitted since that state does not use Carnegie units in defining its graduation standards. I identified the year in which each respondent was 18 by their age on enumeration day (April 1, 1990).

[14] College entrants are those whose highest reported educational attainment was "Some college, no degree" or higher. Unfortunately, PUMS does not distinguish high school graduates from GED completers. However, to the extent that higher standards generated a reduction in this measure of high school completion, we can conclude there was not a completely offsetting increase in GED completion. It should also be noted that this sample, of course, includes students who attended private schools. However, their inclusion is arguably appropriate since it is possible that students may switch schools to avoid the consequences of stricter standards.

[15] Those who report that they are not in the labor force are defined as unemployed to avoid omitting discouraged workers. However, the exclusion of these respondents does not substantively alter the subsequent results.

[16] The school attendance variable is meant to control for the fact that those respondents still in school over the last year would have had limited labor market experiences. This specification is similar to those used by Bishop and Mane (2001). However, I also estimated purer "reduced-form" versions of this model that excluded the measures of educational attainment and school attendance. The results were similar to those reported here (Table 4).

[17] I report Huber-White heteroscedastic-consistent standard errors, which assume clustered effects specific to each state-of-birth by year-of-birth cell. Also, I found that probit models for the binary dependent variables generated similar results.

whether a MCT was required for that particular graduating class (Table 1). The second dummy identifies whether the state had a high, academically focused CGR in effect for that graduating class. A high CGR is defined here as a required high school curriculum that includes at least 3 Carnegie units in English, 2 in social studies, 1 in science and 1 in mathematics.[18] These and other state-year controls were matched to the respondents by their state of birth and year of birth. One noteworthy limitation of the PUMS data is that relying on state of birth may introduce measurement error since some children have moved to different states by the time they reached high school. However, the attenuation bias implied by such measurement error suggests that the reported estimates can be interpreted as lower bounds on the true effects.[19]

As suggested earlier, the identification strategy embedded in this model makes a potentially important contribution to our understanding of the consequences of first-wave reforms because it removes the possible biases due to unobserved state-level determinants. This model effectively does this by comparing the cohort differences in the "treatment" states before and after the introduction of new standards to the contemporaneous cross-cohort changes in the "control" states. I present some evidence on the empirical relevance of relying on within-state versus cross-state comparisons by comparing the results of models that do and do not include the state fixed effects, μ_s. I also present some heuristic evidence on this specification issue through the use of a simple counterfactual in which I estimate the "effect" of a state policy that should not have large and statistically significant effects on educational attainment. To the extent that a particular specification suggests that this policy did have large and statistically significant effects, the existence of specification error is suggested.[20]

In the preferred specifications, which include state fixed effects, the possible sources of omitted variable biases are the unobserved determinants of Y that are also related to the timing of new standards within states. The matrix, $\mathbf{Z}_{st}$, addresses this concern by including other regression controls that vary by state and year. For example, new state standards were sometimes part of omnibus education bills that included other policy changes such as increased spending. To control for the possible effects of school spending, some models include, as an independent state-level variable, real expenditures on K-12 public schools per student in average daily attendance when the respondents were 16 to 17 years old. For example, respondents who were 18 in 1980 were matched to the school expenditures in their state during the 1978-1979 school year. Another state-year control in most models is the state unemployment rate when the respondent was 17 years old. This variable is expected to have a positive effect on educational attainment since it reduces the opportunity costs associated with remaining in school (Duncan 1965). Card

[18] Some studies represent state CGR policies by the total number of Carnegie units required. However, this measure may more accurately reflect the focus of reform efforts (e.g., *A Nation at Risk*), which was to course taking in core academic areas.

[19] Also, I found that the results were similar in models that matched respondents to the state-year variables by their state of residence five years prior to the Census.

[20] Specifically, I estimate, in models with and without state fixed effects, the "effects" on educational attainment of whether there were any state executions at age 18. One virtue of using state executions for this counterfactual is that there was considerable variation over this period both within states and across states. An increasing number of states began executions in 1984.

and Lemieux (2001) present evidence that the natural variation in the size of particular birth cohort's population can also influence educational attainment. At the college level, this could occur if temporary increases in cohort size are not fully matched by an increased supply of enrollment space at local colleges and universities. At the secondary level, increased cohort size may reduce the benefits of remaining in school by lowering school quality.[21] Therefore, I also include a measure of cohort size based on the natural log of the U.S. Census Bureau's estimate of 18 year-olds in the respondent's state of birth at age 18. I also include a measure of the real costs of postsecondary tuition based on the in-state rate at "lower-level" state colleges and universities when the respondent was 17 years old (Card and Lemieux 2001, Kane 1994). Finally, as a control for within-state changes in socioeconomic priors, I also matched each respondent to the poverty rate in their state when they were 17 years old.

RESULTS

In Table 2, I present the estimated effects of MCT and CGR policies on educational attainment across a variety of specifications. These results demonstrate the estimated effects of the first-wave reforms on educational attainment are quite sensitive to controlling for unobserved state fixed effects. For example, the models that exclude state fixed effects but include the other state-year controls suggest that MCT significantly reduced the probability of graduating high school and attending college. These results also suggest that CGR policies had no statistically significant effects on either measure of educational attainment. However, the models that include state fixed effects and the other state-year controls, imply that MCT had small and statistically insignificant effects on both outcomes. These models also suggest that higher CGR reduced the probability of graduating high school by a statistically significant 0.48 percentage points.[22]

Several dimensions of these results in Table 2 suggest that the inferences from the models with state fixed effects are more reliable. First, F-tests indicate that the state fixed effects are jointly significant determinants of educational attainment. Second, in models that exclude state fixed effects, the key results are highly sensitive to the presence of the other state-year controls. For example, similar to Bishop and Mane (2001), the first model implies that MCT significantly increased the probability of attending college. However, after introducing the other controls, this estimate becomes negative and significant. This type of sensitivity suggests the difficulty of relying on proxies for the determinants of educational achievement across states. Third, the sensitivity of these evaluation results to the introduction of state fixed effects does not appear to reflect any loss of sampling variation or statistical precision. Specifically, some of the cross-state models suggest that MCT and CGR had effects roughly 2 percentage points in size. However, in the

[21] Card and Lemieux (2001) find that cohort size is associated with significant increases in pupil-teacher ratios.

[22] An effect of this size represents a 0.6 percent reduction in the mean probability of graduating from high school, or, alternatively, a 3 percent increase in the mean probability of dropping out.

preferred specification (column 5 of Table 2), the standard errors are sufficiently small that effects of that size can be rejected at conventional levels of statistical significance.

A final way to provide some ad-hoc evidence on the reliability of cross-state versus within-state comparisons is through the use of a simple counterfactual. To the extent that empirical evaluations relying on cross-state comparisons generate reliable results, we should find that, conditional on the other controls, irrelevant state policies have small and statistically insignificant effects on educational attainment. However, to the extent that an irrelevant policy appears to have a large and statistically significant effect, it suggests that the existence of biases driven by the unobserved, state-specific determinants of educational outcomes. The results in Table 2 present such evidence by reporting the estimated "effects" on educational attainment of having any state executions at age 18.[23] The models without state fixed effects suggest that capital punishment generates large and statistically significant reductions in the probability of high school completion (1.3 percentage points) and college entrance (1.7 percentage points). However, in the models that rely on the within-state variation in executions, these estimates are much smaller, more precisely estimated and statistically insignificant.

In Table 3, I present the key evaluation results from the preferred specifications that include both the state-year fixed effects and the state-year controls. I also report the estimated effects of these first-wave reforms from separate models for white males, white females, black males and black females. All of these models suggest that the first-wave reforms had statistically insignificant effects on the probability of entering college. However, these results also indicate that these reforms had fairly large and statistically significant effects on the probability of completing high school and that these effects varied considerably by race and gender. In particular, these estimates suggest that higher CGR significantly reduced the probability of completing high school for white males and blacks but not for white females.[24] Notably, the reform-driven reductions in educational attainment were particularly large among blacks (roughly 2 percentage points). These estimated reductions are roughly four times larger than those for white males. Similarly, these results suggest that the only large and statistically significant effect of introducing MCT was among black males who experienced an estimated 1.26 percentage point reduction in the probability of completing high school.

The evidence from Table 3 is largely consistent with the concerns sometimes raised by critics of standards-based reform (e.g., Murnane and Levy 2001). The introduction of high school graduation standards led to reductions in educational attainment that were particularly concentrated among black students. However, a full evaluation should also consider the implications of these reforms for labor market experiences. Attention to the labor market consequences of these policies also has a strong intuitive

[23] The execution of criminals could have an actual influence on educational attainment since the prosecution of these cases can be a meaningful drain on public resources. However, it should be noted that this reduced-form effect should be quite small and that these models control for real school spending per pupil.

[24] It should be recalled that the PUMS definition of a high school graduate includes GED completers so these results may actually understate the true reform-induced reduction in high school completion.

appeal since local business leaders concerned with the quality of their work force were often instrumental in the adoption of first-wave reforms. As noted earlier, higher standards may benefit students (even those who drop out) by inducing increased educational effort that is rewarded in the labor market (an "incentive effect"). There may also be distributional consequences of these reforms to the extent that higher standards increase the prestige of being a high school graduate and correspondingly reduce the stigma associated with being a dropout (a "sorting effect").

Table 4 presents new evidence on these issues by reporting the estimated effects of the first-wave reforms on employment participation and log wages for the full PUMS sample and for samples defined by race and gender. These models include state and year fixed effects, the state-year controls and additional individual-level controls for educational attainment and student status. Unlike the prior cross-sectional evaluations, these results suggest that both reforms had small and statistically significant effects on wages for all groups. However, these results also suggest that the first-wave reforms had statistically significant effects on employment participation. Specifically, these estimates suggest that higher CGR increased the probability of being employed by roughly 1 to 3 percentage points for white males and blacks. As with the model for high school completion, higher CGR had small and statistically insignificant effects for white females.[25] The introduction of MCT significantly increased the probability of employment for black males (1.64 percentage points).

The results in Tables 3 and 4 suggest that new CGR were a meaningfully binding standard that had educational and labor market consequences for almost all students. In contrast, the effects of MCT were more limited. These results are consistent with the anecdotal evidence suggesting that MCT were "implemented as a lamb" in response to political realities. These results also suggest that, when binding, higher standards of either type had decidedly mixed distributional consequences. They reduced educational attainment, particularly among black students. However, they also generated some labor market rewards in the form of increased employment probabilities that were concentrated among black students. How can we compare these gains and losses? One possibly useful point of reference is the expected wage associated with being a high school graduate or a dropout. A rough calculation based on these data suggests that high school graduates receive an expected wage premium equal to approximately 33 percent of a dropout's average wage, w_d.[26] This implies that those who dropped out of school in response to the higher standards suffered substantive consequences. Their loss of this wage premium was offset only somewhat by a .0081 increase in the probability of employment as a dropout. However, for those who would have dropped out

[25] However, MCT significantly reduced the employment probability for white females by 0.95 percentage points. Since MCT did not significantly influence educational attainment among white females, this reduction could reflect a lowering of educational effort. The results in the next section provide some suggestive evidence that this may be so by evaluating the effects of these reforms on individual "process" measures drawn from transcript data and survey questions.

[26] The wage regression indicates that the wages of high school graduates are 18 percent higher than those of dropouts. The mean probability of employment among dropouts is 54 percent. The regression model for employment participation suggests that high school graduates are 20 percentage points more likely to be employed. Therefore, the implied premium in expected wages is $0.33w_d$ (i.e., .74 x 1.18 - .54).

or graduated without regard to the changed graduation requirements, there were unambiguous labor-market gains since they are significantly more likely to be employed.

Another possible useful way to frame these costs and benefits is to ask how these reforms might change the expected wage for someone who was uncertain about whether they would be a high school graduate or not. For such a person, the expected cost of higher CGR is the reduction in the probability of enjoying the 33 percent wage premium of high school graduates. This equals $.0016w_d$ (i.e., .0048 x .33). The benefit of a higher CGR is an increased probability of being employed (.0081) at an expected wage equal to $1.155w_d$ (i.e., .86 x 1.18 + .14).[27] This expected benefit equals $.0094w_d$. Therefore, the expected wage benefits of a higher CGR exceed expected wage costs by a factor of roughly 6 (.0094/.0016).[28] This suggests that a risk-neutral person might prefer a regime with higher standards to one without and that the net effects of the higher standards on expected wages are positive. However, it should also be noted that these back-of-the-envelope calculations do not constitute a full cost-benefit analysis. In particular, these comparisons ignore the other social losses that may be associated with reform-induced reductions in educational attainment (e.g., those related to health, criminal and civic behaviors).

5. STANDARDS AND EDUCATIONAL PROCESSES

The evidence from the prior section suggests that first-wave reforms sometimes reduced educational attainment but also generated some improvements in the probability of employment. These labor market consequences of stricter graduation standards could, in most cases, simply reflect passive sorting effects (Betts and Costrell 2001). However, they could also indicate reform-induced increases in educational effort, which were subsequently rewarded in the labor market (i.e., incentive effects). In this section, I provide some empirical evidence on the second possibility by examining how the first-wave reforms influenced several educational "process" measures. Most of these process measures reflect patterns in academic course-taking among individual high school students. Interestingly, the amount of academic course-taking among public high school graduates did increase significantly during the 1980s across students of varying demographic traits (U.S. Department of Education 2001a, Table 138). These increases were particularly large in mathematics and science. For example, the average number of Carnegie units among public high school graduates in 1982 and 1994 increased 27 percent in mathematics and 38 percent in science (U.S. Department of Education 2001a). Some studies have suggested that the new CGR were at least partially responsible for these increases. For example, the U.S. Department of Education (1997) finds that the graduates in high-CGR states have higher levels of academic course-taking. Similarly, Clune and White (1992), in a study of four states, found that academic course-taking among graduates increased after

[27] Given the stated uncertainty about whether the observer will be a dropout or not, the expected wage is based on the probabilities of being a graduate or a dropout (.86 and .14) and the wage premium for graduates (18 percent). However, these calculations generate similar results if we assume the observer only receives w_d.

[28] For black males and females, the policy-induced gains in expected wages are also several times larger than the corresponding losses.

the introduction of higher CGR. However, there are two specification issues that may bias these inferences about the effectiveness of first-wave reforms. One is that the appearance of policy-induced increases in academic course-taking could simply be due to the increased dropout rate (Table 3) instead of genuine increases in academic effort. A second concern is that the identification strategies, which rely exclusively on either cross-state or time-series comparisons, may lead to substantively biased inferences.[29] The evaluations presented here provide new evidence on these issues by examining estimates from models that include eventual dropouts and that control for unobserved state and year fixed effects.

In this section, I also present new evidence on concerns raised by critics of standards-based reform that higher standards may have unintended and pejorative effects on other educational processes. For example, one concern is that creating minimum standards may encourage high-performing students to reduce their academic effort. I present evidence on this issue by evaluating how first-wave reforms influenced high-level course taking in math and science (i.e., Carnegie units in calculus, physics and chemistry). I also present evidence on whether first-wave reforms narrowed student curricula by reducing Carnegie units in the visual and performing arts. Another related concern about establishing explicit standards for students is that they may discourage intellectual engagement and lifelong learning by suggesting that learning for its own sake is not worthwhile. While measuring these sorts of attitudes is inherently difficult, the U.S. Department of Education (2001b), in its annual report, *The Condition of Education*, suggests that students' use of time is closely related to their educational aspirations and feelings about school. They also note that, between 1984 and 1999, 17-year olds report watching less television. However, these 17-year olds are also less likely to read for pleasure and spend less time doing homework (in part, because it is less likely to be assigned). In this section, I also present evidence on how first-wave reforms may have influenced the amount of time students spend on these three activities.

DATA AND SPECIFICATIONS

The data for these evaluations were created by pooling observations from two of the National Center for Education Statistics' (NCES) major longitudinal studies: the sophomore cohort from High School and Beyond (HS&B) and the National Education Longitudinal Study of 1988 (NELS). These surveys provide student-level data from before and after the time when most first-wave reforms were implemented (Table 1). More specifically, HS&B and NELS provide nationally representative samples of 10th grade students from 1980 and 1990, respectively.[30] Since each of these studies had a transcript component, they also include data on the Carnegie units earned in particular subject areas in addition to

[29] For example, a pre-reform/post-reform time-series comparison may attribute to higher CGR increases in academic course-taking that are actually due to other time-varying determinants (e.g., the growing college wage premium in the 1980s).

[30] My NELS extract does not include all NELS respondents since some of the base-year sample (i.e., those in 8th grade in 1988) were not in 10th grade at the time of the 1990 follow-up. However, in order to be representative of high school sophomores, the extract does include respondents who "freshened" the sample: 10th graders who were not in 8th grade in 1988.

survey questions on students' use of time.[31] My extract from these surveys consisted of white non-Hispanic and black respondents who were 10th graders in 1980 (HS&B) and 1990 (NELS) and includes eventual dropouts. The combined sample size with available transcript data consists of 18,134 observations (9,331 from HS&B and 8,803 from NELS).[32]

The econometric specification I used for models based on these data is similar to the preferred specification from the previous section. The independent variables of interest reflect the state high school graduation requirements in effect for the graduating classes of 1982 and 1992 respectively (Table 1). The other independent variables include state and year fixed effects, individual-specific variables and variables specific to each state-year cell. The individual-level controls include single dummy variables for race, gender and age (born before 1964 for HS&B respondents and before 1974 for NELS respondents). These controls also include four dummy variables for the highest level of parental education, five dummy variables for family composition and four dummy variables for SES quartile (including one for a missing SES index). I matched each respondent to the relevant graduation requirements and other state-year controls by exploiting the state identifiers in the restricted-use versions of these surveys.[33] The state-year controls again include 1981 and 1991 data on real public school spending per capita, the state unemployment rate, the poverty rate and the real postsecondary tuition level. I also matched each respondent to the size of their state-year cohort: 1982 and 1992 data on the natural log of the 18-year old population in their state.

RESULTS

In Table 5, I present estimates of how first-wave reforms influenced the amount of academic credit earned in the four core subject areas. These results uniformly suggest that the introduction of MCT reduced course taking in these academic areas. However, these estimated reductions are statistically significant only in the sciences and mathematics. The estimated reductions in science and mathematics attributable to MCT are roughly equal to 5 percent of the dependent means. In contrast, the evidence in Table 5 suggests that higher CGR had the desired effect of generating substantive increases in the credits earned in these core areas. In particular, a high CGR (3 or more Carnegie units required in the subject) led

[31] Each course taken by a student was assigned one of the more than 2,200 unique codes associated with the Classification of Secondary School Courses (CSSC). For details of each transcript study, see U.S. Department of Education (1995, 1997).

[32] The sample sizes for the non-transcript outcomes are over 19,000. The lower sample size for course-taking data largely reflects the fact that the schools of some NELS respondents were not surveyed for transcripts (U.S. Department of Education 1995). Respondents from Nebraska were also excluded because the state does not use Carnegie units in defining its graduation standards.

[33] Interestingly, the restricted use version of HS&B does not directly identify each high school students' state. Lillard and DeCicca (2001, footnote 13) imputed the state of each HS&B school by identifying the state in which its graduates attended community college. I identified the states of HS&B schools through a cross-walk of the HS&B data on the 1980, 1981 and 1982 state unemployment rates and published numbers.

to increased credit in each academic subject.[34] For example, a high CGR in science increased credits earned by 0.393 relative to the reference category of a weak or non-existent CGR (less than 1 Carnegie unit required in subject). This estimated effect is roughly equal to 16 percent of the mean science credits. However, the estimated effect of a high CGR on social studies was statistically insignificant as were the much smaller estimated effects of weaker CGR (1-2.99 Carnegie units required). Nonetheless, these results suggest that new CGR did contribute substantively to the academic upgrading of high school curricula over this period, particularly in English and the sciences. For example, the estimated effect associated with a high CGR in science is equal to roughly 60 percent of the average growth in science credits over this period.

The results in Table 5 suggest that the introduction of an MCT led to a reduction in curricular effort. However, there is some ambiguity to these results since it is unclear to what extent these reductions are driven by students who may be induced into dropping out (Table 3) or by high-performing students who choose only to meet the new minimum requirements. In Table 6, I provide some indirect evidence on these and other issues by reporting the estimated effects of these reforms, by race and gender, on Carnegie units earned in science and mathematics. The results indicate that MCT led to particularly large reductions in science and mathematics credits for black students. Since black males were also more likely to drop out because of MCT (Table 3), the policy-induced reductions in credits earned may be due in part to an increased intent to drop out. However, the introduction of MCT also led to particularly reduction in math and science credits for female students. These reductions are more likely to reflect lower levels of curricular effort among otherwise high-performing students since there is no indication that females were more likely to drop out because of a new MCT (Table 3). Interestingly, the results in Table 6 also indicate that high CGR in mathematics and science had particularly large and positive effects on the academic credits earned by black students, substantially reducing their gap relative to white students.[35]

The results in Tables 5 and 6 suggest that MCT sometimes had negative effects on curricular effort while the effects of higher CGR were often positive. One of the difficulties of interpreting those results is that they define the policies' effects for the "average" student. However, it is reasonable to suspect that these reforms could have unique effects on low-performing students. In Table 7, I present alternative evidence on how first-wave reforms influenced the amount of academic credits earned at a low-performing margin. More specifically, I constructed new dependent variables: dummy variables equal to 1 for students who earned at least one Carnegie in a given academic subject. Across the four academic subjects, 95 to 98 percent of students met this weak standard. In Tables 7 and 8, I present regression results from models whether these indicators are the dependent variables.

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[34] The reference category is a weak or non-existent CGR (less than 1 Carnegie unit required in subject).
[35] Cook and Evans (2000) find that only 25 percent of the convergence in black-white test scores is attributable to changes in family background while 75 percent reflects changes within schools. The relative academic upgrading generated for black students by higher CGR may be one of the within-school changes responsible for this convergence.

The regression results in Table 7 suggest that, in each academic subject, the introduction of MCT reduced the probability that students would meet this low standard. However, these estimates are estimated imprecisely and most are statistically indistinguishable from zero. In contrast, these results also suggest that higher CGR sometimes increased the probability of having at least one Carnegie unit in these academic subjects. In particular, these estimated effects were, again, relatively large and statistically significant in English and the sciences. These results are striking since they suggest that, while higher CGR may have encouraged more students to drop out (Table 3), they may have also upgraded their academic curricula prior to dropping out. This implies that any subsequent labor market gains associated with higher CGR could reflect incentive effects as well as sorting effects. In Table 8, I present the results for science and mathematics in race-specific and gender-specific models. These results indicate that higher CGR increased the probabilities that students from each demographic group would have at least one Carnegie unit in the sciences. Notably, these policy-induced increases in science credits at this margin were particularly large for black and female students.

In Table 9, I present some evidence on how these reforms influenced the amount of credit earned in advanced courses in mathematics (i.e., calculus) and the sciences (i.e., physics and astronomy). Interestingly, these regressions suggest that the introduction of MCT led to a large and statistically significant reduction in calculus credits but had smaller and statistically insignificant effects on physics and chemistry credits. These results also suggest that an increased CGR in science led to large and statistically significant increases in physics credits but not in chemistry credits. The results in Table 9 also indicate that a high CGR led to a weakly significant increase in calculus credits. Overall, like the results in Tables 5 through 8, these results provide fairly consistent evidence on how first-wave reforms influenced student curricula in core academic areas. More specifically, these results indicate that the introduction of MCT led to some reductions in curricular effort among students likely to meet the basic testing requirement. However, these results also indicate that higher CGR made substantive contributions to the upgrading of academic curricula over this period and did so for students at very different margins.

However, other concerns about student-level standards involve whether they narrow student curricula in undesirable ways or reduce the intellectual engagement of students. I present some evidence on these issues in Table 10. First, I report the estimated effects of the first-wave reforms on student involvement in the visual and performing arts (as measured by Carnegie units) and on student participation in school musical activities (i.e., the school band, orchestra or chorus). For both outcomes, the estimated effects of the first-wave reforms were imprecisely estimated and statistically indistinguishable from zero. The final regression results in Table 10 focus on indirect measures of intellectual engagement and effort based on the students' reported use of time. Specifically, I created dummy variables for whether the respondent never or rarely reads for pleasure outside of school, for whether they watch four or more hours of television per weekday and for whether they do no homework.[36] The results in Table 10 suggest that the

[36] The HS&B and NELS questions on time spent doing homework were not consistently phrased. The HS&B question (BB015) allowed students to distinguish between doing no homework and having none

introduction of MCT led to a large but weakly significant increase in the probability of doing no homework (t-statistic = 1.95). However, the estimated effects of a high CGR (i.e., a "3/2/1/1" standard or higher) on students' use of time were larger and more precisely estimated. More specifically, the results in Table 10 indicate that a higher CGR led to large and statistically significant reductions in the amount of time students spend reading for pleasure and doing homework and a correspondingly large increase in time spent watching television. It should be noted that there is some ambiguity in interpreting these results. It could reflect the direct effect of imposing minimum requirements on student perceptions of the value of learning. However, there may also be other issues related to the implementation of new CGR that contribute to these results as well. For example, it may be that less homework was assigned after the increases in state CGR. Nonetheless, these results suggest that CGR-induced increases in academic course taking were attenuated by undesirable changes in how students allocate their time.

6. LESSONS FROM THE FIRST WAVE

Our ongoing debate about the design and desirability of standards-based reform hinges critically on how such policies may influence a variety of outcomes among students with different backgrounds. In this paper, I provided new evidence on those issues by examining the effects of the earlier state-level standards on several outcome and process measures. These results demonstrated that the first wave of student-level standards appear to have had many of both the positive and negative effects suggested by commentators on both sides of these issues. For example, these reforms led to reductions in educational attainment that were particularly large for black students. Furthermore, minimum competency testing (MCT) led to some apparent reductions in curricular effort while higher course graduation requirements (CGR) had pejorative effects on the amounts of time students spend watching television, doing homework and reading for pleasure. However, these reforms also increased subsequent employment probabilities. And higher CGR were partly responsible for the substantial academic upgrading of high school curricula that occurred over this period. In light of this mixed evidence, what can these prior state-level experiences contribute to our current discussions about standards?

A productive, though modest, initial step may be to consider what these results would suggest, to a proponent of standards-based reform, about how those standards should be designed. In particular, the first-wave reforms provide an interesting basis for comparison since they included both a test-based standard (MCT) and a "process" standard (CGR). The results presented here suggest that advocates of standards-based reform may prefer the ultimate effects of process standards to those of a test-based standard. More specifically, minimum competency testing had relatively few of the desired effects on educational attainment and early labor market experiences (Tables 3 and 4). These results are consistent with the widely held perception that these test-based standards were often quite weak because of political pressures and the relatively easy and veiled manner in which they could be subsequently lowered. In

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assigned but did not distinguish between homework done in and outside school. My coding of this variable is based on this HS&B question and the NELS question (F1S36A2) for homework done outside of school.

contrast, newly introduced course graduation requirements (CGR) created more binding, new standards for students (Tables 3 and 4) and they were also largely immune to subsequent political redesign.

The evidence from student-level transcripts provides additional support for the relative attractiveness of process standards. More specifically, the results in Tables 5 through 9 suggest that CGR contributed directly to the academic upgrading of the high school curricula of a wide variety of students. In contrast, this evidence also suggests that some students actually lowered their curricular effort, particularly in the sciences and mathematics, in response to the introduction of MCT. The one caveat to the comparative attractiveness of CGR is that their benefits may be attenuated by changes in teacher expectations (e.g., how much homework is assigned) and changes in how students allocate their time (Table 10). Furthermore, whether these comparative, first-wave results have much "external validity" for ongoing efforts to develop test-based accountability is clearly open to conjecture. But, at a minimum, the early state-level experiences with minimum competency testing provide an important, cautionary tale.

The implications of the results presented here for the broader debate over whether standards are desirable type of education reform must be based on more subjectively normative grounds. For example, an advocate of a Rawlsian social welfare function (Rawls 1971) would almost certainly look with favor on such reforms since they increase the employment probability of those who would have dropped out of school anyway. However, others with a more utilitarian perspective may be less willing to accept small employment gains for many students at the cost of significant welfare losses among those encouraged by new standards to drop out of high school. It is exactly those welfare losses that encourage advocates of standards-based reform to recommend the simultaneous adoption of targeted efforts to assist those who may be newly at risk of dropping out (e.g., Betts and Costrell 2001). Similarly, critics of standards also suggest that, if standards are to be implemented, they should be accompanied by increased "capacity building" in the form of higher teacher salaries, teacher training and local control of schools (e.g., Murnane and Levy 2001).

However, such approaches to attenuating the difficult trade-offs implied by higher standards may provide a deceptively facile solution. In particular, it should be recalled that a fundamental motivation for instituting standards in the first place has been the controversial claim that educational inputs cannot be targeted in ways that systematically promote student achievement. So, recommendations to help somehow the students harmed by standards bring us back to the notoriously difficult research questions about which programs or expenditures might actually be effective.

The experiences within some of the states that adopted first wave reforms suggest we should not be too sanguine about their ability (or willingness) to craft solutions that soften these difficult trade-offs. For example, we can consider the context in which first wave reforms were introduced in the two states with the largest public school enrollments. The state of California instituted a new course graduation requirement (first effective for the graduating class of 1987) as part of the Hughes-Hart Educational Reform Act of 1983 (Senate Bill 813). This legislation was a comprehensive school reform package that

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However, I found that alternative coding schemes generated similar results.

combined a new state CGR with $800 million in new funds targeted at over 80 other initiatives including higher starting salaries for teachers and a teacher mentoring program (Cuban 1984, Pipho 1986). Similarly, the state of Texas introduced minimum competency testing in 1984 (first effective for the graduating class of 1987) as one component of an extensive package of school reforms (House Bill 72). These reforms included a variety of other complementary initiatives such as increased starting salaries for teachers, a teaching career ladder, management training for principals and superintendents and a "no pass, no play" restriction on extracurricular activities (Pipho 1986). Some districts in Texas also responded to the MCT by developing summer-school initiatives targeted at those at risk for dropping out because of the new standards (Archer and Dresden 1987). These examples indicate that many of the students who faced new state-level graduation standards were also supported by a contemporaneous mix of other financial and regulatory changes. This implies that the difficult trade-offs indicated by this study's empirical results are a relatively intractable feature of introducing higher student-level standards. This interpretation suggests that our ongoing public discussions about the desirability of highly centralized standards should explicitly address how we might value those trade-offs. Furthermore, these discussions should also consider how the diverse set of policy effects presented here might compare to those of alternative proposals such as the second wave reforms that stress decentralization and local control.

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Table 1 - State High School Graduation Requirements, 1980 and 1992

State	Minimum Competency Test (MCT)	Course Graduation Requirements (CGR)	
		1980	1992
Alabama	1985	4/3/1/1	4/3/2/2
Alaska	-	1/1/1/1	4/3/2/2
Arizona	-	3/2/1/1	4/2.5/2/2
Arkansas	-	4/1/0/0	4/3/2.5/2.5
California	-	-	3/3/2/2
Colorado	-	-	-
Connecticut	-	-	4/3/2/3
Delaware	-	4/3/1/1	4/3/2/2
Florida	1983	0/0/0/0	4/3/3/3
Georgia	1985	3/1/1/1	4/3/2/2
Hawaii	1983	4/4/3/3	4/4/2/2
Idaho	-	3.5/2/2/1	4/2/2/2
Illinois	-	3/1/0/0	3/2/1/2
Indiana	-	3/2/1/1	4/2/2/2
Iowa	-	0/1.5/0/0	0/1.5/0/0
Kansas	-	4/2/1/1	4/3/2/2
Kentucky	-	3/2/2/2	4/2/2/3
Louisiana	1991	3/2/2/2	4/3/3/3
Maine	-	4/1/0/0	4/2/2/2
Maryland	1982	4/3/2/2	4/3/2/3
Massachusetts	-	-	0/1/0/0
Michigan	-	0/.5/0/0	0/.5/0/0
Minnesota	-	3/2/0/0	4/3/1/1
Mississippi	1989	3/2.5/1/1	4/2/2/2
Missouri	-	1/1/1/1	3/2/2/2
Montana	-	4/1.5/1/2	4/2/2/2
Nevada	1982	3/2/1/1	4/2/2/2
New Hampshire	-	4/2/1/1	4/2.5/2/2
New Jersey	1981	4/2/1/2	4/3/2/3
New Mexico	1990	4/2/1/2	4/3/2/3
New York	1979	4/3/1/1	4/4/2/2
North Carolina	1980	4/2/2/1	4/2/2/2
North Dakota	-	3/3/2/1	4/3/2/2
Ohio	-	3/2/1/1	3/2/1/2
Oklahoma	-	4/1.5/1/1	4/2/2/2
Oregon	-	3/4/1/1	3/3.5/2/2
Pennsylvania	-	3/2/1/1	4/3/3/3
Rhode Island	-	4/1/1/1	4/2/2/2
South Carolina	1990	4/3/1/2	4/3/2/3

South Dakota	-	4/2/1/1	4/3/3/2
Tennessee	1982	4/1.5/1/1	4/1/2/2
Texas	1987	3/2.5/2/2	4/3/2/3
Utah	-	3/2/1/1	3/3/2/2
Vermont	-	-	4/3/2.5/2.5
Virginia	-	4/3/1/1	4/3/2/2
Washington	-	6/5/2/3	3/2.5/2/2
West Virginia	-	4/3/1/1	4/3/2/2
Wisconsin	-	-	4/3/2/2
Wyoming	-	0/1/0/0	0/1/0/0

The year in the "Minimum Competency Test" column refers to the first graduating class for which the requirement was effect. The course requirements indicate the Carnegie units required for the graduating classes of 1980 and 1992 in English, Social Studies, Science and Mathematics, respectively. Nebraska was excluded because it does not use Carnegie units. The sources for these data include various publications from the Education Commission of the States, Jacob (forthcoming) and state-specific searches of newspaper articles.

Table 2 - Linear Probability Models for Educational Attainment, 1990 PUMS

Independent Variable	Estimated Effects					
	High School Graduate					
Minimum competency test (MCT)	.0001	-.0187‡	-.0165‡	-.0013	-.0014	-.0015
	(.0065)	(.0033)	(.0031)	(.0020)	(.0020)	(.0020)
High course graduation requirement (CGR)	-.0051	-.0027	-.0020	-.0055†	-.0048†	-.0048†
	(.0041)	(.0024)	(.0023)	(.0025)	(.0022)	(.0022)
Any state executions at age 18?	-	-	-.0126‡	-	-	.0004
			(.0041)			(.0019)
	College Entrant					
Minimum competency test (MCT)	.0244†	-.0240‡	-.0211‡	-.0054*	.0002	.0009
	(.0113)	(.0056)	(.0054)	(.0028)	(.0030)	(.0030)
High course graduation requirement (CGR)	-.0197‡	-.0058	-.0047	-.0079*	-.0028	-.0028
	(.0075)	(.0046)	(.0045)	(.0048)	(.0033)	(.0033)
Any state executions at age 18?	-	-	-.0170‡	-	-	-.0026
			(.0064)			(.0024)
State Fixed Effects?	No	No	No	Yes	Yes	Yes
State-Year Controls?	No	Yes	Yes	No	Yes	Yes

These results are based on the 1,348,766 white non-Hispanic and black respondents who were age 18 between 1980 and 1988. All the models include fixed effects for race, gender, and year of birth. The state-year controls are the unemployment rate, the natural log of cohort size, poverty rate, real K-12 expenditures per-pupil and real post-secondary tuition. Heteroscedastic-consistent standard errors, adjusted for state-of-birth by year-of-birth clustering, are reported in parentheses.

* Statistically significant at 10-percent level

† Statistically significant at 5-percent level

‡ Statistically significant at 1-percent level

Table 3 - Estimated Effects of Minimum Competency Tests (MCT) and High Course Graduation Requirements (CGR) on Educational Attainment by Race and Gender, 1990 PUMS

Sample	Dependent Variable: High School Graduate MCT	High School Graduate CGR	College Entrant MCT	College Entrant CGR	Sample Size
All respondents	-.0014 (.0020)	-.0048† (.0022)	.0002 (.0030)	-.0028 (.0033)	1,348,766
White Males	.0025 (.0033)	-.0053† (.0025)	.0049 (.0045)	-.0026 (.0040)	585,376
White Females	-.0022 (.0026)	-.0005 (.0032)	.0007 (.0035)	-.0017 (.0035)	588,611
Black Males	-.0126† (.0055)	-.0211† (.0087)	-.0024 (.0073)	-.0161 (.0111)	81,799
Black Females	.0010 (.0054)	-.0203‡ (.0070)	.0044 (.0070)	-.0188 (.0015)	92,980

These estimates are based on linear probability models, which include fixed effects for race, gender, state of birth and year of birth as well as the controls for the unemployment rate, cohort size, poverty rate, real K-12 expenditures per-pupil, real post-secondary tuition. Heteroscedastic-consistent standard errors, adjusted for state-of-birth by year-of-birth clustering, are reported in parentheses.

* Statistically significant at 10-percent level

† Statistically significant at 5-percent level

‡ Statistically significant at 1-percent level

Table 4 - Estimated Effects of Minimum Competency Tests (MCT) and High Course Graduation Requirements (CGR) on Employment Participation and Wages by Race and Gender, 1990 PUMS

Sample	Dependent Variable			
	Employed		Log Wages	
	MCT	CGR	MCT	CGR
All respondents	-.0053*	.0081†	.0088	-.0074
	(.0031)	(.0035)	(.0060)	(.0070)
White Males	.0012	.0106‡	.0094	-.0075
	(.0033)	(.0041)	(.0069)	(.0078)
White Females	-.0095†	.0011	-.0073	-.0041
	(.0031)	(.0044)	(.0066)	(.0070)
Black Males	.0164†	.0339‡	.0108	-.0279
	(.0069)	(.0101)	(.0148)	(.0177)
Black Females	-.0025	.0182*	.0169	-.0251
	(.0080)	(.0104)	(.0133)	(.0166)

These estimates are based on linear probability models, which include fixed effects for race, gender, ethnicity, state of birth and year of birth as well as the controls for the unemployment rate, cohort size, poverty rate, real K-12 expenditures per-pupil and real post-secondary tuition. These models also include individual-level controls for educational attainment and student status. Heteroscedastic-consistent standard errors, adjusted for state-of-birth by year-of-birth clustering, are reported in parentheses.

* Statistically significant at 10-percent level

† Statistically significant at 5-percent level

‡ Statistically significant at 1-percent level

Table 5 - Estimated Effects of Minimum Competency Tests (MCT) and High Course Graduation Requirements (CGR) on Carnegie Units by Academic Subject, HS&B and NELS88

Academic Subject	Dependent Mean	Independent Variable: MCT	Independent Variable: 1-2.99 Units Required in Subject	Independent Variable: 3+ Units Required in Subject
English	3.7	-.067 (.056)	.032 (.051)	.329‡ (.051)
Social Studies	3.1	-.088 (.084)	-.015 (.098)	.133 (.101)
Science	2.4	-.135† (.055)	.092* (.052)	.393‡ (.084)
Mathematics	2.7	-.127† (.050)	-.020 (.046)	.110* (.059)

These regressions include fixed effects for state, year, race, gender, age, parental education, family composition, SES quartile as well as the state-year controls for the unemployment rate, cohort size, poverty rate, K-12 expenditures per-pupil, post-secondary tuition. Heteroscedastic-consistent standard errors, adjusted for state-year clustering, are reported in parentheses.

* Statistically significant at 10-percent level

† Statistically significant at 5-percent level

‡ Statistically significant at 1-percent level

Table 6 - Estimated Effects of Minimum Competency Tests (MCT) and Course Graduation Requirements (CGR) on Carnegie Units in Science and Mathematics by Race and Gender, HS&B and NELS88

Sample	Dependent Mean	Independent Variable: MCT	1-2.99 Units Required in Subject	3+ Units Required in Subject
		Science		
Full sample	2.4	-.135†	.092*	.393‡
		(.055)	(.052)	(.084)
Whites	2.5	-.075	.109*	.338‡
		(.065)	(.060)	(.100)
Blacks	2.1	-.225†	.241†	.548‡
		(.093)	(.113)	(.084)
Males	2.5	-.082	.121*	.449‡
		(.060)	(.067)	(.108)
Females	2.4	-.209‡	.064	.339‡
		(.057)	(.053)	(.072)
		Mathematics		
Full sample	2.7	-.127†	-.020	.110*
		(.050)	(.046)	(.084)
Whites	2.8	-.062	-.004	.071
		(.053)	(.046)	(.064)
Blacks	2.5	-.181*	-.058	.251†
		(.109)	(.080)	(.112)
Males	2.8	-.128†	.055	.173†
		(.062)	(.045)	(.065)
Females	2.7	-.157‡	-.092	.059
		(.060)	(.069)	(.073)

These regressions include fixed effects for state, year, race, gender, age, parental education, family composition, SES quartile as well as the state-year controls for the unemployment rate, cohort size, poverty rate, K-12 expenditures per-pupil, post-secondary tuition. Heteroscedastic-consistent standard errors, adjusted for state-year clustering, are reported in parentheses.

* Statistically significant at 10-percent level

† Statistically significant at 5-percent level

‡ Statistically significant at 1-percent level

Table 7 - Estimated Effects of Minimum Competency Tests (MCT) and Course Graduation Requirements (CGR) on Probability of At Least One Carnegie Unit by Academic Subject, HS&B and NELS88

Academic Subject	Dependent Mean	Independent Variable		
		MCT	1-2.99 Units Required in Subject	3+ Units Required in Subject
English	.979	-.011*	.028‡	.029‡
		(.006)	(.006)	(.006)
Social Studies	.964	-.008	-.002	.003
		(.007)	(.007)	(.006)
Science	.951	-.010	.028‡	.042‡
		(.008)	(.010)	(.014)
Mathematics	.961	-.001	.003	.009
		(.007)	(.005)	(.007)

These regressions include fixed effects for state, year, race, gender, age, parental education, family composition, SES quartile as well as the state-year controls for the unemployment rate, cohort size, poverty rate, K-12 expenditures per-pupil, post-secondary tuition. Heteroscedastic-consistent standard errors, adjusted for state-year clustering, are reported in parentheses.

* Statistically significant at 10-percent level

† Statistically significant at 5-percent level

‡ Statistically significant at 1-percent level

Table 8 - Estimated Effects of Minimum Competency Tests (MCT) and Course Graduation Requirements (CGR) on Probability of At Least One Carnegie Unit in Science and Mathematics by Race and Gender, HS&B and NELS88

Sample	Dependent Mean	Independent Variable: MCT	Independent Variable: 1-2.99 Units Required in Subject	Independent Variable: 3+ Units Required in Subject
			Science	
Full sample	.951	-.010	.028‡	.042‡
		(.008)	(.010)	(.014)
Whites	.956	-.016*	.029‡	.038‡
		(.009)	(.010)	(.016)
Blacks	.926	-.008	.043‡	.068‡
		(.014)	(.018)	(.012)
Males	.943	-.001	.029‡	.035‡
		(.010)	(.012)	(.020)
Females	.958	-.020†	.029‡	.050‡
		(.010)	(.010)	(.011)
			Mathematics	
Full sample	.961	-.001	.003	.009
		(.007)	(.005)	(.007)
Whites	.965	.0003	.002	.006
		(.006)	(.005)	(.006)
Blacks	.937	.016	.022	.021
		(.020)	(.014)	(.021)
Males	.954	.003	.008	.014†
		(.007)	(.005)	(.007)
Females	.967	-.009	.001	.006
		(.009)	(.009)	(.011)

These regressions include fixed effects for state, year, race, gender, age, parental education, family composition, SES quartile as well as the state-year controls for the unemployment rate, cohort size, poverty rate, K-12 expenditures per-pupil, post-secondary tuition. Heteroscedastic-consistent standard errors, adjusted for state-year clustering, are reported in parentheses.

* Statistically significant at 10-percent level

† Statistically significant at 5-percent level

‡ Statistically significant at 1-percent level

Table 9 - Estimated Effects of Minimum Competency Tests (MCT) and Course Graduation Requirements (CGR) on Carnegie Units in Calculus, Physics and Chemistry, HS&B and NELS88

Dependent Variable	Dependent Mean	Independent Variable: MCT	Independent Variable: 1-2.99 Units Required in Subject	Independent Variable: 3+ Units Required in Subject
Calculus	.08	-.037†	.003	.023*
		(.015)	(.009)	(.013)
Physics	.20	-.007	.053‡	.094‡
		(.019)	(.014)	(.017)
Chemistry	.46	.026	-.011	-.007
		(.023)	(.022)	(.023)

These regressions include fixed effects for state, year, race, gender, age, parental education, family composition, SES quartile as well as the state-year controls for the unemployment rate, cohort size, poverty rate, K-12 expenditures per-pupil, post-secondary tuition. Heteroscedastic-consistent standard errors, adjusted for state-year clustering, are reported in parentheses.

* Statistically significant at 10-percent level

† Statistically significant at 5-percent level

‡ Statistically significant at 1-percent level

Table 10 - Estimated Effects of Minimum Competency Tests (MCT) and High Course Graduation Requirements (CGR) on Arts and Use of Time, HS&B and NELS88

Dependent Variable	Dependent Mean	Independent Variables	
		MCT	High CGR
Carnegie Units in Visual and Performing Arts	1.4	-.071 (.079)	.022 (.079)
In Band, Orchestra or Chorus	.28	-.013 (.012)	.017 (.012)
Never or Rarely Reads For Pleasure	.37	.023 (.015)	.041‡ (.014)
4 or More Hours of TV per Weekday	.29	-.007 (.014)	.046‡ (.009)
Does No Homework	.07	.011* (.006)	.030‡ (.006)

These regressions include fixed effects for state, year, race, gender, age, parental education, family composition, SES quartile as well as the state-year controls for the unemployment rate, cohort size, poverty rate, K-12 expenditures per-pupil, post-secondary tuition. Heteroscedastic-consistent standard errors, adjusted for state-year clustering, are reported in parentheses.

* Statistically significant at 10-percent level

† Statistically significant at 5-percent level

‡ Statistically significant at 1-percent level

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