Book review of: Jay P. Greene, *Education Myths: What Special Interest Groups Want You To Believe About Our Schools—And Why it Isn’t So* Lanham, MD: Rowman & Littlefield, 2005 267 pp.

Jay P. Greene, senior fellow in the Economic Research Office of the Manhattan Institute, has published widely on education topics, with his research being cited four times in U.S. Supreme Court decisions. This book brings together his views on the state of contemporary American K-12 education. His ideas are clear and compelling, backed up as they are by extensive research.

He considers eighteen myths about education, that is, views about education that are that are both pervasive and demonstrably false. He groups these myths into four categories: myths about resources; myths about outcomes; myths about accountably; and myths about choice.

He starts with six myths about resources. First is the view that public schools perform badly because they are under-funded. This is a common refrain among liberals, not to mention teachers’ unions, but Greene notes that the average person accepts it as well. He demolishes the myth in part by showing that national inflation-adjusted per pupil spending funding has increased nearly linearly every year from 1945 (when it was only $1,214) to 2001 (when it hit $8,745), while the NAEP (National Assessment of Educational Progress) scores stayed essentially flat from 1971 to 1999. The NAEP is the most reliable and representative national measure of scores we have, and was only started in 1971. During the period it as been used, the per pupil funding went from $4,479 to $8,745, while the average score only increased an insignificant three to five points out of 500 possible. Again, the high school graduation rates from 1971 to 1999 were flat: from 75.6% *down* to 72.5%. He attributes the prevalence of this myth to people’s reluctance to believe that more spending has been useless, as well as the fact that during the period we can reliably measure outcomes, the funding rose only slowly, but the number of students dropped, so people didn’t notice the rise per pupil. He also attributes the prevalence of the myth to well-publicized anecdotal accounts of funding problems.

He then takes up the myth that schools are doing poorly because of a rising tide of developmentally disabled (“special ed”) children. He refutes this myth by noting that while certainly the number of kids enrolled in special ed classes has mushroomed (from 8.3% in 1976-1977 to 13.3% in 2000-2001—an increase of over 50% in less than 25 years), this has been due to changes in diagnostic practices rather than an increase in actual disabilities. For example, during the same period the number of mentally retarded kids has dropped from 961,000 to 599,000. (Greene suggests the drop is due to better prenatal care and improved safety devices, but doesn’t mention a more plausible explanation, viz., abortion). He gives a good deal of evidence that the increase in students in special ed is due to the funding incentives, so that “…not only is the Special Ed Myth false, it is the reverse of the truth: special education is not draining school budgets, it’s inflating them” (p. 34).

The next myth Greene explores is the claim that schools are doing poorly because of social problems (broken homes, poverty, poor parenting, or such like). Greene and his co-researcher Greg Forster addressed this hypothesis by developing a “Teachability Index” of sixteen social indicators, establishing it to be a reliable predictor of student success. They then examined each state’s level of achievement compared to what would be predicted by its teachability index score. If social problems in a student population trump the type of schooling that population receives, then you would expect to see not much variability in student achievement among states with similar teachability indexes. But you do, which means that student achievement is due to more than the level of advantages the students have, but the sort of education which they are given. He urges that four school strategies have been shown to work to improve student achievement: accountability programs; school choice; early intervention for struggling students; and instruction based upon specific sets of skill and factual knowledge.

Greene then addresses the theory that small class sizes produce dramatic gains in student achievement. There is some data that lowering class size improves performance, but as Greene shows (in a very probing evaluation) it is not powerful. The gains are small and the costs quite large—for instance, when California adopted a small class program, the number of teachers went up from 62,226 to 91,112 in three years. (You can see why teachers’ unions push this myth so aggressively!) And again, we have nation-wide statistics that under-cut the theory: from 1970 to 2001 the average number students per teacher dropped 29% (from an average class size of 22.3 down to 15.9) while achievement scores and graduation rates remained flat.

Next is the myth that professional credentials and experience make a big difference in teacher effectiveness. Here again Greene does an excellent literature survey. The evidence shows that possessing a teaching credential doesn’t result in any significant increase in teaching effectiveness, nor does having an advanced degree in education—though he does note that one study has found that having a masters degree *in the academic subject they teach* does result in a significant improvement of performance of high school teachers. This indicates that what is taught in ed schools is of dubious value. As Greene nicely puts it (p. 70),

The main focus in education courses is the study of pedagogical theory, and it is possible that the formal study of pedagogy might not contribute substantially to the teacher’s ability to actually teach. It is certainly true in many other fields that the study of theory, although important to scholars, does not contribute much to professional performance. Economic theory is not the main focus in schools of business, legal philosophy is even less important in law schools, and biological theory (which includes, for example, debates over evolution) is either peripheral or totally absent in medical schools.

The evidence also indicates that teaching experience improves effectiveness only for the first few years of a teacher’s career, and is associated with a *decline* in effectiveness later—probably due to the role of tenure in deterring teachers from striving for better performance.

Then there is the myth that teachers are paid badly. Here Greene begins by noting the obvious: teachers work far fewer hours than other professionals. They work only nine months per year, have far more holidays, and work about 7.3 hour days. Factor this in, and the average teacher salary of $44,600 (in 2001) works out to a full-time equivalent salary of $65,440. The common reply is that teachers do a lot of work off-site such as grading papers and “keeping up with the field” (as if that were similar to what a science professor at a research university or medical doctor has to do). But most other professionals do off-site work as well. If we look at the 2002 Department of Labor figures on *hourly* compensation, teachers do quite well indeed: elementary school teachers earn $30.75 per hour and high-school teachers earn $31.01. This compares well with other professions: architects ($26.64); biologists ($28.07); civil engineers ($29.45); mechanical engineers ($29.46); physicists ($32.86); computer scientists ($32.86); electronic engineers ($34.97); and even dentists ($35.51). And this doesn’t include the notoriously generous benefits teachers receive—medical insurance, retirement programs that are defined benefit (pensions) rather than defined contribution plans, not to mention the king of all perks, tenure.

Turning next to the myths about outcomes, Greene starts with the myth that schools are performing worse than they used to. He argues surprisingly that test scores show that students are performing roughly as well now as at any time over the last thirty years. For instance, 12th grade scores on the NAEP scores have been essentially flat from 1971 to 1999, as are the percentage of students scoring at the highest levels of proficiency. Again, the ACT and SAT scores were roughly stable over this period, and graduation rates have stayed about the same or declined only slightly. The problem I have with Greene’s claim is that it doesn’t address the period prior to 1971. Even if schools are doing as well now as in 1971, how does that performance compare to the 1960s? Or the 1940s?

He then addresses the myth that almost all kids graduate from high school. This proves to be a most enlightening chapter. The official figure put out by the U.S. Department of Education in 2000 has the high school graduation rate of 86.5%, with many states claiming even higher rates. He reviews various methods for estimating graduation rates, and shows them to be flawed—for example, the NCES method used to come up with the official U.S. graduation figure excludes from the sampling anyone who is incarcerated (which disproportionately includes dropouts), and counts anyone who gets a GED as having graduated (even though the GED is not really comparable to a real diploma). Greene’s more accurate method involves dividing the number of diplomas actually awarded in the year 2000-2001 by the average of the enrollments of this twelfth grade cohort in their eighth, nine and tenth grades (modified to reflect an actual slight increase in the high school population). Greene’s method yields a more realistic figure of 71%. This is disquieting enough, but when Greene does the breakdown by ethnicity, the results are horrific: only 51% of black and 52% of Latino students graduated in 2001.

Another bogus notion he debunks is that the reason for low college enrollment figures for minority students is that financial and other barriers stop them. He shows that this claim is based upon an unrealistic notion of what counts as “college-ready.” If you mean (as the NCES does) that college-ready just means having graduated with a 2.7 GPA, then it will indeed look like qualified students are not attending college. But if you mean (as Greene’s measure does) by “college-ready” students only those who have graduated with a regular diploma, taken the minimum courses required by the colleges, and demonstrated basic literacy, then nearly all college-ready students wind up entering college. That is true of minority students as well as of white students.

The third group of myths concern accountability. Greene considers first the prevalent belief that the results of “high-stakes’ tests are unreliable because of extensive cheating, student stress, and teachers “teaching to the test.” By “high-stakes” tests he means tests with substantial consequences for poor performance, such as students not being allowed to graduate, or schools receiving less funding. He again reviews the evidence, and shows the flaws in some highly touted studies. His own properly designed study (conducted with Marcus Winters and Greg Forster) shows that there are strong correlations between high-stakes test and low-stake test results. This correlation would be unlikely if high-stakes tests had more cheating, stress-induced failure, or coaching than low-stakes tests (such as the Sat-9). He makes an excellent point regarding the buzz phrase “teach to the test.” Teaching to the test is bad if you mean by that coaching students with the correct answers (i.e., cheating by the instructor) or forcing students to memorize unimportant facts. But teaching to the test is good if you mean changing curricula and methodology to make all teachers focus more on the skills that the *public* (which pays for the whole educational system) deems important.

Greene also critiques the similar misperception that high school exit exams increase drop-out rates. He begins by noting that employers are becoming increasingly dismissive of the worth of a high-school diploma. In response, some states have instituted high-school exit exams, which in turn have caused concern that drop-out rates will soar. But in a study he conducted with Marcus winters, they found that “…changes in graduation rates in states that adopt exit exams are not significantly different from the normal fluctuations in graduation rates that are present in states that do not such exams” (p.131). This is surprising, and he considers three possible explanations. First, exit exams might be so easy that they affect almost no students. Second, it may be that since students can take the test repeatedly (and get additional instruction to help pass it), that even students with poor academic skills can get through by chance. Third, it may be that the number of kids weeded out may be offset by the number of kids the schools are forced to help. Greene then argues that the third explanation is the most adequate: the exams indeed weed some out, but are encouraging some students who would otherwise drop out to work harder and some schools to give them the requisite remedial education. The tests take diplomas away from unqualified students and put them in the hands of qualified students…what a revolutionary concept!

Greene then reviews the common lament by administrators that accountability programs, such as the admittedly flawed No Child Left Behind Act (NCLB), are unduly burdensome for schools. (In other words: give us the money, but don’t require that we test students to verify results). Numerous “experts” testify that instituting NCLB’s mandates will require expensive reforms. But Greene wryly notes that “No field other than education makes important financial decisions solely based on claims made by experts…without demanding empirical evidence to back up those claims. The reason for this is simple: expert professionals often have a large financial or political interest in the recommendations they make” (p. 137). I would add that no other field has lower quality “experts” than education. Greene then argues that the real costs of accountability programs lies only in the cost of test preparation and administration, not in the cost of preparing students for the tests—after all, that is what the education budget is for to begin with. So he cites one careful study (by Harvard economist Caroline Hoxby) that shows the costs of testing is at most 0.35% of the education budget even under the most onerous testing regimen. This is quite modest; especially when one considers the increase in quality such testing produces.

The fourth group of myths are the about choice. In my view, these chapters are the most valuable in the book, since they debunk myths about consumer choice in education, which I hold to be the precondition for the success of any other reforms (such as testing). Accountability is great, but unless parents can remove their children form lousy schools, it is of limited effect. Chapter 13, which addresses school choice, is perhaps the most useful of all. It addresses the pernicious myth that there is no evidence that vouchers work.

Vouchers have been implemented in various places in the U.S. They have been around for a century in Maine and Vermont. Arizona, Florida, Ohio and Pennsylvania more recently have launched publicly funded voucher programs, as has the city of Milwaukee (where now 15% of the children attend private voucher schools). Also, philanthropists have set up privately funded voucher programs in a number of American cities, such as Charlotte, Dayton, New York and Washington, D.C. And numerous studies have been conducted on these limited voucher experiments. (Vouchers have been implemented in a number of other countries, such as Denmark, New Zealand and Sweden, but strangely, Greene doesn’t look at those experiences as being useful in discussing the American system. I will return to the point later).

Major media (such as the New York Times, Time Magazine, and the Washington Post say that the evidence is inconclusive about the results of these programs. But Greene, in a detailed literature review, argues strongly that the evidence is not in the least inconclusive. He rightly focuses on random assignment studies, since they are the most reliable statistical studies. (In a random assignment study, subjects are assigned to the experimental or control group randomly, which helps guarantee that the two groups are matched, hence ruling out confounding variables). There have been eight random assignment studies of vouchers, and all but one showed statistically significant gains on test scores for the kids in the program, and the one exception still showed a gain in test scores, but it wasn’t statistically significant. Moreover, besides showing gains in scores, all the voucher programs have proven to be tremendously popular with parents. All this, from programs that cost half what public schools spend per child—none of the American voucher programs is *pro rata,* meaning giving the parents a share of public taxes equal to what is spent per capita by the public schools.

Greene then debunks the common perception that private schools are elite, expensive institutions that can dump low-performing students. U.S Department of Education figures for the year 2000 show that the average private school charges $4,689 per year tuition; religious private schools charge only $4,063 on average; and Catholic school charge only $3,236. All this is markedly lower than the $8,032 that public schools average in spending annually per pupil (which is tuition the taxpayer pays). Nor does the research support that claim that most private schools are very selective, or that they expel a lot of students. (Greene doesn’t make the point, but it seems clear that the *possibility* of expulsion tends to concentrate the minds of students wonderfully, and they behave accordingly, so that the option seldom has to be exercised). Indeed, many private schools—especially the Catholic schools—wind up taking and dealing with students expelled by public schools.

Another major impediment to school choice hurts public schools by draining them of all the decent students. Greene grants that this is a possible concern *a priori*, but that the *a posteriori* evidence belies it. He reviews the studies of voucher programs, and shows that, in fact, far from hurting the public schools, voucher programs improve them. The reason for this is clear if we think dynamically rather than statically: competition forces public schools to rapidly improve the quality of their service out of fear that they will lose students. For example, the aforementioned Caroline Hoxby’s study of the Milwaukee program showed that found that public schools exposed to greater voucher competition made significantly more test gains than those less exposed. Charter schools force similar improvement, as does school districting that makes it easy for people to choose their public school.

Green dispatches are the view that private schools don’t and won’t serve disabled kids, again by a close review of the evidence. The evidence shows private schools do accept disabled students, and (especially in religious schools) for far less money than public schools spend. And the parents of disabled students tend to me more satisfied with private schools, especially because private schools have proven better at protecting these students from bullying, and teaching them better behavior. He likewise dispatches the myth that private schools don’t teach tolerance as well as public schools. No, studies show they actually do a better job of teaching the civic virtues such as tolerance and volunteerism. First, private schools teach everything else better. Second, it appears that students who have a better sense of self-identity are less threatened by people who are different. Third, private schools are typically smaller and have more student and parental involvement, which encourages civic spirit more than large, bureaucratic schools do.

The last myth Greene takes up is the view that private schools are more racially segregated than public schools (and so vouchers would make segregation even worse), a myth pushed by teachers’ unions and civil rights organizations. Here again the evidence argues to the contrary, though fewer relevant studies exist on this matter. For one thing, most measures of “racial diversity” are flawed. Greene’s own study (based on the 1992 National Education Longitudinal Study) showed that racially classrooms were more prevalent in public schools than private. This whole subject is tricky, however, because a school can be integrated but there can still be segregation within it, and after decades of busing, people have already done a good deal of self-segregation.

Greene concludes that all the myths he debunks are part of a “mega-myth,” namely, “…that education is different from other policy areas in that the types of incentives that normally shape human behavior do not shape educational behavior” (p. 218). We think that rewarding good behavior and penalizing bad behavior works in parenting, and in every kind of business—but not in K12 education! He reasonably urges us to begin to think more in terms of economically realistic terms.

Greene’s work is a testament to the great usefulness of libertarian and conservative think tanks. The academy today is thoroughly dominated by leftists of all stripes. Most Humanities and social science departments in particular are almost entirely inhabited by people of the left, with the possible exception of economics departments, and even that is changing rapidly. Add to that the creation of new departments, such as “labor studies,” funded by labor unions (including teachers’ unions), and the difficulty in getting research done that reflects both sides of issues (or even addresses some issues considered politically incorrect) is formidable. These think tanks are a desperately needed counter-balance to the academy. Indeed, I recently heard a leftist radio commentator lament, “We need our own think tanks!” I immediately shouted at the radio, “You have them already—they’re called universities!”

However, I feel that in dealing with several myths, Greene might have looked at broader categories of data. This is especially true regarding the issues surrounding consumer choice in education (which encompasses charter schools, private schools, home schooling, and most importantly voucher programs). He notes rightly that parents are very happy with vouchers when they can get them, but I think light could be shed on this preference if we were to examine, not just testing and attendance data, but measures of social pathology, such as rates of drug usage, rates of pregnancy, assaults and other violent crime, and so on. That is, use data from criminology and sociology as well as pedagogy to determine in what other ways private and voucher schools are superior. I believe you will see that parents are as much or more interested in sending their kids to schools that are safe and uphold standards as one with high test scores, as important as that is. Indeed, recent work by the psychologist Judith Harris supports strongly the theory that peer groups are even more important in older children’s lives than parental training, and vouchers enable poor parents to put their kids in schools where the peer groups are competing to see who gets into the best college rather than who is handiest with a switchblade.

Another area where is data sets could be enhanced is the experience of other countries with vouchers. While comparing an American system with some other country’s system is fraught with the danger of false analogy, it can also be both enlightening and rhetorically effective. For years privatizing retirement systems was theoretically debated, but the fact that Chile and other countries have actually done it adds immensely to the power of the argument for doing it here. For years the superiority in theory of a flat income tax was argued, but the fact that Russia and several other Eastern European countries have successfully implemented flat taxes is immensely instructive. Again, the case for “loser-pays” tort reform is made stronger by looking at the fact that the British and most other Western nations have it. It helps to convince skeptics that something is possible if you can point to an actual case.

In particular, looking at the successful implementation by Sweden of vouchers is instructive when it comes to the common criticism of vouchers that they would increase segregation. While Sweden is more racially homogeneous than America, they certainly are (as we are) a country with economic classes. And their experience with vouchers shows that vouchers actually *decrease* segregation by class. The reason is clear: there, as here, the wealthy (rich and upper middle class) parents *already have* full school choice and mobility, so avoid under-performing schools. But when vouchers give everyone mobility, and schools spring up specializing in science, say, or the arts, wealthier and poorer parents are attracted to them. Again, opponents of vouchers in Sweden argued that vouchers would suck all the good students out of the public schools, but in fact the public schools lost only about 15% of their students, because the public schools quickly improved their quality of service.

Still, Greene’s book is an indispensable compendium of data and argumentation on contemporary American education, and deserves to be read.

**Gary James Jason**

**Department of Philosophy**

**California State University, Fullerton**