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Illusive Competition in School Reform: Comment on Merrifield’s “Imagined Evidence and False Imperatives”

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Abstract: Merrifield (2009) provides a useful polemic about the sad state of data analysis too frequently encountered in the school choice literature. The available data come mostly from limited policy experiments with only modest amounts of choice and competition. These data are then misapplied in debates about more dramatic shifts to new systems to supply educational services that aim for large expansions of choice and competition. It is difficult to cleanly separate theoretical priors from empirical evidence. I contend that it is possible to make a stronger empirical case for dramatic school reform. But doing so would require dealing with six potential pitfalls based on economic theory that might arise when attempting to move to school systems more reliant on private providers of educational services. Given the difficulty of policy experiments, this is a high evidential bar, and may leave us stuck in an unfortunate status quo, as Merrifield suggests. More detailed definitions of competition together with bold, new empirical evidence are clear priorities for advancing debates over school reform, and should be core elements of prescriptive policy analysis.

What Does Competition Mean?

Merrifield points out that different sides in school choice debates too frequently rely on badly defined notions of “competition.” He is surely correct that these debates would benefit from more clarity about the many empirical manifestations of competition, or lack of it, and the extent to which the school choice initiatives studied actually increase choice and competition.

Merrifield emphasizes weakness in the arguments of opponents of school reform who base their opposition on the lack of large, measurable improvements in educational outcomes in empirical studies of school reform initiatives. The weakness, Merrifield explains, is that these school reform initiatives that produced few impressive improvements were very minor initiatives that did not dramatically expand competition or consumer choice for educational services. If all our reform attempts are timid and achieve very little additional range of choice in K-12 education, then one would not expect dramatic shifts in school performance as a result.

In the case of policy experiments with voucher programs, for example, there are still stringent rules for how schools can qualify to be able to accept vouchers. There are sometimes price controls written into the laws calling for voucher experiments that restrict how much schools made eligible to accept vouchers can charge. And, in some cases, political connections between lawmakers and the suppliers of private school services waiting to enter the market following passage of reform legislation may become too close, leading to a handoff of the educational monopoly from the state to favored supplier.

These forces might make education “markets” look very different from those idealized in economics textbooks under the label “perfect competition.” Still, it is a serious question to ask what we mean when we say and write the word, competition. If the world has seen few, if any, markets for educational services that come close to approaching reasonable measures of

competitiveness (e.g., low levels of market concentration, large number of suppliers, or price nearly equal to marginal cost, etc.), then obviously it is unfair to argue that whenever we have tried “competitive school supply,” it fails to achieve better results.

The right answer is that we do not know what a dramatic policy change aimed at large-scale expansion of competition and choice would look like. Would a genuinely competitive educational services market emerge? We do not know, because it has never been tried—perhaps with good reason, or perhaps because of an unfortunate persistence or lock-in of the status quo.

Does More Empiricism Mean Less Theory?

Given an insufficient observational basis to argue in favor of any policy that has never been tried before, advocates for policy change almost necessarily face the daunting task of using both theory and empirics to formulate convincing arguments. Merrifield’s critique of “imagined evidence” implicitly exhorts those debating school reform to adhere to a more rigorous empirical standard. More empiricism seems to me like very good advice for the economists and policy analysts of all stripes.

Yet theory persists in informing the arguments and positions we take. This may be unavoidable. And it may not be all bad. We leave it for another venue to analyze these methodological and philosophical points about whether it is possible or desirable to conduct empirical economic analyses without imposing the structure, and underlying assumptions, of an economic model.

But this challenge, which I take to be an entirely admirable and worthwhile goal, of relying more heavily on empirical rather than theoretical arguments, makes it difficult to extend Merrifield’s critique beyond the absence of good evidence. We may all share the objective of

more strictly tying our arguments to robust empirical scrutiny. But if we have no data in the relevant range (i.e., observations of competitive school supply under an institutional arrangement very different from the status quo), then what avenues are there to argue for the new institution? Are we trapped, as Merrifield suggests, as an “intellectual prisoner of the status quo”?

Statistical Analogy

There is an obvious analogy from Statistics 101 about the inadvisability of using a regression model to make predictions far outside the range of values covered by the sample used to estimate the model. Suppose we want to predict y conditional on x , where y is a measure of educational outcomes, such as schools’ within-school mean test scores, and x is an institutional variable ranging from 0 to 100, measuring how competitive the supply side of the educational services market is. If Merrifield is right that we have almost no observations of highly competitive school markets, then the existing studies are estimating changes in y (educational quality) as a function of very small changes in x (ranging, say, from 0 to 5, very far from anything resembling competitive school markets).

Merrifield’s critique of “imagined evidence” used can be interpreted as: just because observed values of y do not change much on the range of x between 0 and 5 (where school markets remain very far from being competitive), it does not follow that y will not change much when x shifts upward toward a perfectly competitive value of 100.

A Weak Empirical Argument Based on Similarity to Support Evidence-Free Theoretical Priors?

Merrifield’s next line of argument goes well beyond this null result (which, again, is correct and worthwhile to point out). He argues in favor of a much more substantial shift toward

privatization of schools, perhaps doing away with public schools, and removing the role of the state as much as possible in the supply of educational services. If we accept his earlier argument that there are no good observations of truly competitive school supply, then his second argument must be based on theory rather than evidence.

But he attempts an empirical justification for the policy positions he advocates: “Data that could qualify as indirect evidence are plentiful; for example, from competitive industries with much in common with schooling.”

How many competitive industries can you name that have “much in common with schooling”? I cannot think of any.

One private industry that Merrifield mentions is the Japanese cram schools, or *juku*, that offer private tutoring at night and on weekends in Japan. This industry is surprisingly large, and many Japanese children receive private training to help pass school entrance exams. Adults use similar private tutoring services called *senmon gakko* on a relatively large scale in preparing for the challenging written exams used to screen applicants for public sector jobs in Japan.

In both cases, however, the role of the free market in generating demand is open to question, since the motive is to pass an exam whose content is set largely by government entities. Absent strict entrance exams whose content is determined by the state, I suspect these “markets” would disappear—like the market for passport photos if the U.S. government stopped requiring photos on passports. These private industries operate in an overlay with state rules and governmental institutions rather than as spontaneous, self-organized vehicles facilitating exchange between private parties.

I wonder, for example, what a fully competitive education market would provide in the way of courses in history. Would histories that only a few people wanted to consume be sold?

Would groups attempt to influence others' interpretations of history by subsidizing the teaching of their preferred histories? If we let competitive pricing rather than experts granted authority by the state (in some capacity) decide which versions of history are taught, would we have achieved a rich plurality of distinct views about the past, or a chaotic drop in communication between groups with irreconcilable understandings of history? In the "ideal" case (from the point of view of standard supply-demand theory with no externalities) in which every individual was supplied the interpretation of history he or she wanted, would we lose the coordination function of that shared sense of a national history?

Merrifield articulates confidence in "the indispensability of market-determined prices as sources of vital information and powerful incentives," advocating for "a gradual elimination of public schools." But the extent to which the market for education is similar to other markets (e.g., homogenous commodities traded on the Chicago Mercantile Exchange) which appear to be functioning successfully with the full efficiency gains hypothesized by the neoclassical theory remains unclear. Empirical evidence is not what makes the argument. Rather, it is economic theory. And this theory faces a number of empirical challenges.

For example, recent financial and housing market activity reveals that the public's beliefs, psychological frames and emotions should be regarded as fundamental variables in explaining market outcomes, capable of moving endogenous variables such as prices and quantities far from the predictions of neoclassical equilibrium theory (e.g., Berg, 2008; Berg and Gigerenzer, 2008; Berg and Lien, 2005). Merrifield claims, "It is a well-established fact that in a genuine market system, shortages eliminate themselves by initially pushing up prices and hence encouraging their expansion and imitation... ."

But what about the U.S. housing market? Severe and persistent imbalances between quantities supplied and demanded do not seem to automatically disappear, and prices do not always quickly adjust to re-equilibrate the market. It is not hard to imagine that markets for educational services would be at least as sensitive as housing markets to changes in levels of trust, beliefs about future returns, and psychological resistance to sell assets that have lost value.

One can cite pro-market economists like Vernon Smith who put markets to an empirical test, and find that sometimes they work well, and sometimes they don't. And sometimes individuals with no information at all can be coordinated well by market mechanisms (Gode and Sunder, 1993; Becker, 1962). Such an empirical approach to markets is quite distinct from insisting on some conclusions over others in the absence of data.

Similarity Argument in a Regression Model

One may follow the earlier analogy from statistics further to interpret Merrifield's similarity argument in which, even in the absence of data, an empirical argument is put forward. Suppose there are no observations of the pair (x, y) in the range where $x > 10$, reflecting Merrifield's first argument that few, if any genuinely competitive experiments in school reform have to date been undertaken. Merrifield bases his positive claims in favor of privatizing the K-12 market for educational services by inviting us to consider other pairs of allegedly similar variables, y' and x' , whose relationship appears to be similar to y and x , drawing conclusions on the basis of that relationship. He argues that we should consider other markets with high degrees of competitiveness (large x) and high degrees of market performance based on their own outcome metrics (large y) as a basis for assuming that there is a strong positive relationship between x' and y' . On this basis of similarity of (x', y') to (x, y) , Merrifield argues that we can

have faith that the results in educational services markets would be similar. This strikes me as more of a theoretical than an empirical acclaim, however, in tension with his earlier calls for a stronger empirical basis in school reform debates.

Six Potential Problems

In short, the world has seen few, if any, genuine experiments in which the regulatory barriers to entry in the “market” for K-12 educational suppliers (e.g., state-mandated requirements regarding quality, accessibility, pricing, etc.) were low enough to attract a wide variety of firms and, consequently, a wide range of choice for consumers. Perhaps with good reason. I can think of at least six reasons why spontaneous, self-organized, competitive supply of education services might not materialize, even under ideal conditions—or may not be socially desirable from the standpoint of most parents who desire education for their children. I raise these potential problems as implications of economic theory that require empirically grounded responses from proponents of school reform—and as someone who genuinely wants to see more choice and higher quality in schooling.

1 Adjustment Costs to the New Equilibrium

For competition in school supply to develop and mature, some bad educational products would undoubtedly be tested and weeded out in the long run after failing. Failing schools are indeed a major reason for researchers to investigate school reform. Nevertheless, failures much larger than those of currently failing schools may be possible. The potential costs to children who receive failing products, both current failures and newly introduced failures, should be considered as part of the cost to transition to a new system of school provision. Rather than

focusing exclusively on the benefits of moving to a new long-run equilibrium, the transition costs along the path to that new equilibrium should be part of the policy discussion.

Again, this plays into what Merrifield refers to as the imprisoning nature of the status quo. A relevant example might be privatization of state assets in Russia in the early 1990s. The competitive markets of neoclassical theory did not instantaneously appear in Russia. And a significant number of older and lower-income Russians' standard of living fell significantly. Thus, even if we generally agree that the overall transition is positive and welfare-enhancing, we can expect that educational outcomes might very well get worse for many families before they get better under a dramatic shift toward private education.

2 Food Safety as an Analogy for Laissez Faire School Policy?

Food regulation enjoys broad support among American voters, including many conservatives who generally favor de-regulation. Some producers who benefit from government credentialing services (e.g., USDA labels indicating the quality of meat) also might resist shifting toward an entirely *laissez faire* food policy. It is conceivable if not probable that there is widespread support for regulation of quality standards in education, too. This implies another theoretical problem.

If voters largely agree on legislating educational requirements (i.e., a core set of standardized skills that we want all schools to achieve), it could be that these desired requirements are so stringent that very few firms could profitably enter. A preference for regulating quality might shrink the field of firms who have pedagogical technologies that can profitably supply the market. In that case, our efforts to privatize could lead to a highly concentrated and uncompetitive educational services industry.

One need only look at market concentration in the meatpacking industry and the homogeneity that characterizes the vast majority of meat production in the U.S. It is not clear that the quality standards we require will interface well with suppliers' technology sets in a way that produces great heterogeneity of services and or even enough competitive pressure to avoid the large inefficiencies of monopolistic production. I think we need a strong empirical case to support the contention that school reform policies can achieve significant expansions of genuine school choice.

3 Geographical Proximity: Is School Competition Feasible without Extremely High Population Density?

Another potential theoretical problem concerns population density and physical proximity to schools, which may be among the most important variables in many parents' school choice decisions. It is not inconceivable that some parents may even have lexicographic preferences that prioritize geographic proximity over all other factors, and that no amount of additional school quality could compensate for an additional 10 miles of commute distance.

Insofar as schools' locations are a key input in school choice, one would expect monopolistic competition, at best, and monopolistic power of nearly unlimited scope at worst. Consumers of educational services in low-density areas of the country might benefit the least from a move toward decentralized market-based supply of schooling services.

But maybe not. We need more empirics on this point, with special attention to the spatial component of the educational services markets we envision emerging as a result of reform policies. The debate might benefit from breaking out predicted outcomes following policy change separately for low-density and high-density residents.

A related point concerns Tiebout competition. If the “residents will vote with their feet” hypothesis is used as a theoretical rebuttal of this point, then it begs the question of why that competitive pressure does not already produce high-quality competitive results within the public system as school districts “compete” for state revenue based on headcounts of enrolled students. In some parts of the existing public system, it could be that this mechanism is already realizing high degrees of efficiency. More empirics on this point would add greatly to school choice debates.

4 Would the Coordination Function of Public Education Survive Privatization?

Standardized conventions facilitate technical and scientific communication and, doubtlessly, contribute positively to many firms’ production processes. Theory suggests that there is a genuine tension between the gains in terms of expanded choice that could be achieved in a more decentralized system versus coordination gains from speaking a common language and following a common set of conventions in communication.

For example, Americans write the decimal representation of the bank entry, “three thousand dollars and eighty cents” as 3,000.80, whereas whereas Europeans write it 3.000,80. Americans pronounce the variable “z” as “zee” whereas Canadians and British say “zed.” Verbal representation of fractions, units of measure, mathematical definitions, and many other elements of technical language and shared history vary in different parts of the world as the result of different educational institutions. Although translating from one definitional convention to another can be easily accommodated in many cases, potentially high-cost mistakes resulting from non-uniformity should be at least considered. If we moved to a policy of no regulation over school curricula, would we introduce new friction costs associated with non-uniform technical

conventions? A thorough empirical argument against centralization of curriculum and core curriculum content should, I think, deal with this possibility and the apparent success stories (in terms of standardized international tests) of top-performing high school students in relatively centralized systems in places such as Singapore, Hong Kong and Finland.

5 The Possibility of Transitioning from State to Private Monopolists

As a firm believer in competition, I worry a lot about the possibility that, as we move toward privatized schools, we might wind up dumping state monopolies for private monopolies instead, which might be even more difficult to regulate. I have listed several reasons, notably coordination gains from uniformity in certain limited domains and the problem of natural monopolies owing to the importance of physical geography. Badly aligned incentives of politicians overseeing transitions to private school systems are another related concern. In any proposed transition toward greater private provision of schooling, there will be tremendous incentives for well-positioned firms wanting to enter the newly de-regulated education market to lobby lawmakers to write in special requirements favoring one potential provider over another, leading to market power in private as opposed to state controlled hands.

6 Positive Externalities not Captured in Market Prices

Lastly, it is worth recalling the fundamental economic argument underlying state subsidization of education, which is the positive externality that an educated citizen provides to other citizens and firms. Public goods such as a population of fluent speakers of a common language, commonly shared sets of technical terms in specialized technical and scientific endeavors, and the socialization that occurs in schools, are key inputs in nearly all firms'

production functions (Bowles and Gintis, 2002). Subsidizing education lowers the costs of production to firms. And insofar as there are synergistic production processes, subsidizing education should increase economic growth.

Of course, subsidizing education does not imply that the state must be the sole supplier of those services. No doubt there exist far less restrictive subsidy policies if legislators wanted to reduce regulation while hanging onto the education subsidy.

False Imperatives

Regarding false imperatives, Merrifield reminds us to use a fair social welfare criterion, or outcome measure, when evaluating the effects of reform initiatives. As Merrifield points out, measuring the benefit of competition-expanding programs in terms of their effects on the quality of state-supplied schooling unfairly leaves out the benefits to children in private schools. But because many do make the argument that the reason public schools fail is because they have no incentive to compete, investigating changes in public school performance after private competitors appear does not seem to be an entirely empty question.

Considering that this essay argues for improved empirical analysis and more careful linking of arguments about competition to data, I was surprised at the number of claims in the essay that appear to have no empirical support. That doesn't mean that the claims are wrong—just unsupported. Given the author's goal, it seems inconsistent to criticize others for arguing this way and not provide clearer evidence to support claims, which rest entirely on untested theory—importantly, untested in the domain of school choice.

International School Reform Initiatives

Merrifield helps us remember that we have much to learn from programs and education-providing institutions outside the U.S.:

Half-hearted, partial market liberalization can yield misleading results. For example, the ineffectiveness and perverse side effects of presumed market reforms slandered capitalism in South America, and with dire consequences only now becoming evident in places like Argentina, Venezuela, and Bolivia.

Because the author seeks more empirical documentation of our positions on school reform, a few more details about what aspects of the South American cases prevented genuine competition from taking root would have been nice, although Merrifield's article and books are extraordinarily thorough in citing relevant literatures.

Regarding the New Zealand example, which lacked competitive pricing and allowed serious barriers to entry to remain, I wonder if Merrifield is too pessimistic about reforms that only modest changes to facilitate particular forms of competition. In this case, the dimension of competition was that public school students could more easily switch schools, with revenue consequences for winners and losers in terms of head counts. Here, economic theory is not completely clear in offering predictions. Theory gives us comparisons of perfect competition versus everything else. The relevant question is whether there is anything like a second best (e.g., most efficient school-switching policy subject to the constraint of strict price controls).

While acknowledging Merrifield's point that the extent and flavor of competition are important to document and vary with greater magnitude shifts in future policy experiments, there is nothing in economic theory saying that moderate shifts toward limited forms of competition are ineffective or not worth their cost. One could even make the case that the most successful competitive systems benefit from limitations on the dimensions in which competition is allowed.

One thinks of the role of the referee in professional basketball. If brass knuckles, kicking and punching were not forbidden in the rules and enforced by referees, then the competitive system in professional basketball would produce a very different set of winners, and athletes like Michael Jordan would probably not rise to the top in a completely unregulated form of basketball. We depend on the centralized regulation of referees to channel competition along pre-defined ranges that give us the *type* of competition that we desire.

Simulation and Uniformity

Regarding simulation studies as a means of producing new insights about school choice, I share Merrifield's desire to see such studies carried out. There are daunting empirical challenges to carrying it forward, however. It would be nice if the author specified which parameters that cannot be estimated directly from data should be simulated. Relying on simulation in this way, an overarching theoretical model is still required, so that theory may be making as large an imprint on our results as empirics:

Improvements in simulation models, including sensitivity analysis with parameters that can't be estimated from present data, are of the utmost importance. That is probably the only way to explore the importance of key parent, educator, and entrepreneurial behavior, and the implications of the apparent significant diversity in how children learn.

Merrifield's discussion of simulation brings up a terrifically important point regarding uniformity of schools, and the possible benefits of moving to multiple measures of ability rather than singular and universally applied metrics. Public schools in their current state are hardly uniform, as is evidenced by the large role that school quality has in homebuyers' location

choices. If schools were uniform, then parents would not condition their residential choice decisions so heavily on public school attendance zones.

One-size-fits-all measurement schemes and school curricula obviously drive many sincere and creative teachers out of public education. But many good ones choose to remain, which is another indication that the current system is, thankfully, not completely uniform, and that financial incentives alone cannot explain who becomes a teacher and who remains in the profession.

Conclusion

Merrifield's admonitions about imagined evidence are very welcome: "The actual competitiveness of the settings studied to gauge 'market competition' effects is a largely neglected, crucial issue." Regarding the priorities he advocates, I think the empiricism implied in the admonitions should be tied more closely to the policy changes he advocates. If put to a vote, I think I would vote with Merrifield in favor of radical change. My point, however, is that our theoretical priors must be driving these prescriptive policy views more than empirical evidence (see Berg, 2003, for an expanded argument on this point). Economic theory suggests the possibility of pitfalls when moving toward a greater private role in the supply of school services. Perhaps caution has prevailed too strongly in school reform analyses to date (e.g., Hanushek, 1994). Hopefully, empirical evidence can be brought to bear that will assuage those concerns and provide insights to design school reform policy with more choice and with less risk of running into problems predicted by theory. Acknowledging the six points I raised above regarding possibly unforeseen losses should strengthen future policy debates by tying them—to the maximum extent possible—to rigorous empirical analysis.

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