Lost Ground: Catholic Schools, the Future of Urban School Reform, and Empirical Legal Scholarship


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The central themes in Margaret Brinig and Nicole Garnett’s Lost Classroom, Lost Community: Catholic Schools’ Importance in Urban America distill as easily as they haunt. Well understood is that the United States needs to improve the quality of education as well as its equitable distribution across various subgroups of students. Paradoxically, students most in need of high-quality education services—including minority students, particularly those from low-income households in urban areas—are more likely assigned to underperforming public schools. Historically, the nation’s Catholic schools provided urban students, including many minority students from low-income households, with more efficacious yet less expensive educational services than their urban public school counterparts. Brinig and Garnett’s book identifies and discusses an especially lethal interaction of an array of key trends: While the need for high-quality, low-cost education services continues its ascent, Catholic school closures accelerate and, in so doing, threaten efforts to help improve the urban education landscape. To make matters even worse, as Brinig and Garnett also argue, the consequences of Catholic school closures extend beyond the education realm and degrade the stability of urban communities. Brinig and Garnett’s work on this topic is important as the policy issues remain timely and novel, and they enlist data and empirical methods into their analyses. As a result, Brinig and Garnett’s book is not only important for what it says but also how it says it.

I. Introduction

Lost Classroom, Lost Community: Catholic Schools’ Importance in Urban America explores a difficult and discomforting issue with important policy consequences: What happens when an increasing number of Catholic schools “vanish from the urban landscape forever”?1 The story that unfolds—buttressed by careful empirical legal research—is an unhappy story

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for many, and one with distressing educational and public policy consequences. While any school closure warrants careful attention and analysis, what Brinig and Garnett tell us is that the accelerating trend of Catholic school closures in urban America poses particular problems for education reform efforts generally and, more particularly, for the many families, including many low-income minority families, who now have fewer education options. But that is not all. Brinig and Garnett’s additional—and more provocative—claim is that Catholic school closures pose important deleterious consequences for many urban neighborhoods and communities.

Despite a relatively robust and well-developed scholarly literature on Catholic schools, *Lost Classroom* contributes in two important ways. First, while much of the existing literature frames Catholic schools as educational institutions, Brinig and Garnett expand the traditional analytic frame by assessing Catholic schools not only as educational institutions but also as community institutions. In so doing, the authors endeavor to better understand the complex relations between Catholic schools and the “neighborhoods where they are (or were) situated.”

Second, also critical to *Lost Classroom*’s success is that it brings a sophisticated and creative empirical perspective to timely research questions. As a consequence, *Lost Classroom* is an important scholarly contribution not only for what it says but also how it says it.

II. Background

Public perceptions about the persistent and substantial challenges confronting America’s public schools, particularly its urban public schools, are well-known, well rehearsed, and, to some degree, too quickly devolve into caricature. Public perceptions about urban Catholic schools are similarly both well understood and well rehearsed. Public and political rhetoric aside, some, perhaps even many (but certainly not all), of the perceptions about urban public and Catholic schools benefit from well accepted and robust empirical support.

Perceptions about urban public and Catholic schools, particularly those flowing from the inevitable comparisons among schools, contribute to uneasy relations among public and private schools. Further exacerbating already strained relations is the growing presence of market forces found in the education setting. As the need for higher quality educational services

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2. *Id.* at 3.
increases, so too does parental demand for increased control over their children’s educational destiny. As the locus of control over educational decisions continues to migrate from the state to individual households, competition between public and non-public (including Catholic) schools increases. The combination of the Supreme Court’s decisions in Pierce v. Society of Sisters5 and Zelman v. Simmons-Harris6 legally preserves Catholic schools as one viable alternative to public schools7 or, more accurately, at least for those families who can afford such options. Where urban public and Catholic schools may have uneasily coexisted in the past, these schools increasingly find themselves competing with each other for market share and students. While increased competition may further strain relations between urban public and Catholic schools, it underscores why policymakers need to better understand the consequences triggered by the acceleration of Catholic school closures in many cities.

A. Urban Public School Challenges

An array of reasons warrants attention to urban public schools. One involves the sheer scale of urban schools. The largest 100 urban districts enroll more than 22% of the nation’s public school students.8 Moreover, a sizable majority of the students attending these largest districts are nonwhite (71.1%) and eligible for reduced-price lunch programs (55.9%).9 Notwithstanding the particular challenges that confront the vast range of American urban public schools, critiques typically flow from two broad charges: Urban public schools do not adequately generate desired student academic achievement levels, and they are too expensive.10 The challenges that confront many urban public schools, while important, are no longer important enough to obscure the stark and persistently uncomfortable data on urban public school performance.

In terms of one critical school-level outcome—graduation rates—large urban school districts lag behind national averages. In 2007–2008, for example, the national average freshman graduation rate was 75%; for the

5. 268 U.S. 510 (1925).
7. See id. at 644–45, 652–55 (upholding Ohio’s voucher program against an Establishment Clause challenge); Pierce, 268 U.S. at 516–20 (invalidating a statute that forced students to attend public schools instead of private and parochial schools).
9. Id. at 2 tbl.2.
largest 100 school districts it was 65%. Of course, in many ways worrisome graduation rates merely reflect the culmination of persistent and complex challenges relating to student academic achievement.

More granular assessments of student academic achievement require data. For generations, however, data limitations hamstrung efforts to compare student achievement across schools, districts, states, and, increasingly, nations. While critical student academic data limitations persist, the terrain shifted for the better by the turn of the twenty-first century. The array of critiques of the No Child Left Behind Act of 2001 (NCLB), some of which are important, are already well documented. Notwithstanding deserved (and some undeserved) criticism, however, for the narrow purpose of gaining greater clarity into student academic progress, NCLB possesses two critical attributes. First, NCLB requires the production—and distribution—of student achievement data. Prior to NCLB, efforts to assess student achievement in the United States proved far more difficult, likely by design, principally owing to a paucity of consistent, coherent data. Second, one key provision in NCLB involves “adequate yearly progress” (AYP). Whether a school or district achieves AYP flows from whether annual student test results required under NCLB achieve state-defined proficiency thresholds. As one might imagine, many states have lowered proficiency thresholds in light of the consequences that flow from NCLB. That is, NCLB has effectively transformed what was once a “race

11. CHARACTERISTICS OF THE 100 LARGEST PUBLIC SCHOOL DISTRICTS, supra note 8, at 7–8. Actual graduation rates remain in some dispute. See, e.g., Clint Bolick, Civil Rights and the Criminal Justice System, 20 HARV. J.L. & PUB. POL’Y 391, 394 (1997) (“[I]n most urban areas in the United States today, the graduation rate from public high schools is less than 50%, and it is substantially less than 50% for minorities.”).


16. Id. § 6311(b)(2)(B)–(C).

17. For discussion of the “race to the bottom” created by the NCLB’s ever-increasing performance standards see, for example, Michael Heise, The Political Economy of Educational Federalism, 56 EMORY L.J. 125, 144–47 (2006); Ryan, supra note 12, at 944, 948 & n.77; David J. Hoff, States Revise the Meaning of ‘Proficient,’ EDUC. WEEK, Oct. 9, 2002, http://www.edweek.org/ew/articles/2002/10/09/06tests.h22.html, archived at http://perma.cc/S9CM-SFGG; Diana Jean Schemo, Sidestepping of New School Standards Is Seen, N.Y. TIMES, Oct. 15,
to the top”—where states competed with one another for higher proficiency thresholds—into a “race to the bottom.” As a consequence, schools and districts today who fail to achieve AYP under NCLB increasingly fail to do so in a context of diluted state standards.

A relevant and helpful summary of data on urban public school student achievement, some of which is now required under NCLB, comes from the annual reports from the Council of Great City Schools, a consortium of the nation’s 67 largest urban public school districts. The 2010–2011 school-year student achievement data presented in a recent Council report convey grim news. Specifically, for fourth grade students, only 29% of the urban districts performed at (or above) state proficiency levels in math. The percentage drops to 15% for eighth graders. Results for reading are slightly worse, with only 17% of the urban districts performing at state proficiency levels for fourth graders. The percentage rises to 19% for the eighth graders. Moreover, National Assessment of Educational Progress test data from the 2009–2010 school year convey a similarly unsatisfactory picture of student achievement and illustrate the degree of the achievement gap that separates many urban public schools from national public school averages.

If questions about urban public schools’ academic performance were not damaging enough, that they are more expensive than their Catholic school counterparts only makes a challenging situation even more so. As Table 1 makes quite clear, public school (not just urban public schools) per pupil spending since just after World War I, adjusted to constant 2012 dollars, reveals a virtually unbroken upward trend. While per pupil spending for urban public schools, relative to the national trend, may have evolved over the years, it is highly unlikely that urban public schools departed too

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18. Heise, supra note 17, at 144.
19. See, e.g., id. at 144–45 (illustrating the trend by describing Connecticut’s dilution of its student-performance standards to a level lower than its “own definition of ‘goal level’”).
21. Id. at 6 fig.6.
22. Id.
23. Id. at 5 fig.5.
24. Id.
26. Perhaps reflecting the recent economic downturn, public school per pupil spending dropped slightly in the most recent two years.
dramatically from the national average. If anything, during the past few decades per pupil spending in most urban public schools likely exceeded the national average.

Regardless of how urban public school per pupil spending fared compared with the national average, more germane to Brinig and Garnett’s book is its relation to Catholic schools. National data on Catholic school annual tuition prove elusive, but Table 1 includes what little data are readily (and publicly) available. Even a paucity of Catholic school annual tuition data cannot obscure one obvious point: Catholic schools operate less expensively than public schools. To observe only that Catholic schools operate less expensively than public schools, however, misses the main, larger point: Catholic school students report higher levels of academic achievement *despite* lower per pupil spending levels.
Table 1: Total Public School Per Pupil Spending & Average Catholic School Tuition

![Graph showing public school vs. Catholic school spending over time]

NOTE: Constant 2012 dollars.

B. Catholic Schools

While it certainly remains the case that many wonderful public schools, including urban public schools, exist, it also remains painfully clear that far too many struggle. Similarly (and not surprisingly), Catholic schools range in quality and efficacy. Nonetheless, for an array of reasons, including self-selection, the general descriptive claim that, on average, Catholic schools outperform their public school counterparts is no longer controversial. Indeed, such a claim is now remarkably unremarkable and well understood. Core findings from James Coleman’s path-breaking research beginning in the 1970s generally withstood the test of time. Even more salient, of course, is


28. James S. Coleman et al., High School Achievement: Public, Catholic, and Private Schools 176, 178 (1982) (observing that there are multiple ways in which “private schools produce higher achievement outcomes than public schools”); see also, e.g., Anthony S. Bryk et al., Catholic Schools and the Common Good 297 (1993) (finding four foundational characteristics to explain the efficacy of Catholic high schools); Andrew M. Greeley, Catholic High Schools and Minority Students 107 (1982) (identifying the “apparently superior performance of young people . . . in Catholic schools”); Timothy Walch, Parish School: American Catholic Parochial Education from Colonial Times to the Present 241 (1996) (highlighting the fact that “[s]tudents in parish schools outperform their friends in public
that many Catholic schools succeed amid “urban chaos.” As Brinig and Garnett note, Nicolas Lemann’s *Atlantic Monthly* essay in 1986 advanced the strong form of this point by observing that in many American cities today, “the only institutions with a record of consistently getting people out of the underclass are the parochial schools.” Moreover, as Richard Kahlenberg observes paradoxically, many Catholic schools “better approached the middle-class culture of the common school than high-poverty public schools themselves.”

Assuming a key and, by this point, largely uncontested premise—that many Catholic schools serving urban areas outperform their public school counterparts in critical ways, including student achievement—frames a devastating conclusion: The acceleration of Catholic school closures in urban areas reduces the supply of efficacious educational institutions that serve a disproportionate number of children most in need of quality educational services.

While evidence of an acceleration of Catholic school closures is obvious to most who live in an urban area—particularly those with school-age children—a brief summary of the salient macrotrends warrants attention. As Table 2 illustrates, the raw number of Catholic schools has slowly but steadily declined since the early 1960s. The rate of decline, while generally stable since approximately 1975, steepened since approximately 2002. The trend line in Table 2 comports with Brinig and Garnett’s claim that “[n]ationwide, over 1,600 Catholic schools have closed in the past two decades.”

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29. Brinig & Garnett, supra note 1, at 28.
32. Brinig & Garnett, supra note 1, at ix.
Notwithstanding evidence of a long-standing, steady, and recently accelerating trend of Catholic school closures, optimists might try to grasp at the hypothetical possibility that an accelerating decline in the total number of Catholic schools over time need not necessarily translate into a decline in the number of students served by the diminishing supply of Catholic schools. That is, perhaps the data in Table 2 merely reflect school consolidations or a shift from smaller to larger Catholic schools. Data in Table 3, however, dampen any optimism and reveal that, as one would more realistically expect, the magnitude in the decline in the total number of Catholic schools parallels a similar decline in the number of Catholic school students. Indeed, the trend line in Table 3 more or less mirrors the trend line in Table 2. Thus, in the context of Catholic education, fewer Catholic schools results in fewer Catholic school students. As Brinig and Garnett note, during the past two decades alone, Catholic school closures have displaced more than 300,000 students.

34. BRINIG & GARNETT, supra note 1, at ix.
III. From Lost Classrooms to Lost Communities

Lost Classroom advances two central claims that flow from subtle interactions. The first claim, while easier to support, poses immediate consequences for urban school reform. Specifically, the very student population most in need of quality educational service—students from low-income homes that include a disproportionate number of nonwhite students—are most exposed to the acceleration of Catholic school closures. Lost Classroom’s second and more far-reaching claim flows from the first. Catholic school closures—triggering a net reduction in social capital-building institutions—contribute to a broader destabilization of many urban neighborhoods. To support their claims, the authors turn to data and empirical methods, which are entirely appropriate to their research questions and which reflect and contribute to broader trends in legal academic research.

A. Lost Classrooms and the Impact on Urban School Reform

A diminishing number of urban Catholic schools implicates school reform efforts at both the individual and institutional levels. Obviously, those students (and their families) literally displaced by Catholic school closures confront immediate challenges associated with the need to transfer schools. More broadly, however, individual students—and households—now confronting reduced opportunities to access urban Catholic schools, and their

35. Data obtained from the National Catholic Educational Association’s annual statistical reports on Catholic schools. Data & Information, supra note 33.
enviable record of success, must navigate among more limited educational opportunities. One irony, of course, is that the decrease in urban Catholic schools coincides with an increase in the demand for school choice, as well as publicly and privately funded programs supporting greater school choice.36

A diminishing supply of urban Catholic schools also poses threats to school reform at the institutional level. As Kahlenberg correctly underscores, the “social capital of the sort found in Catholic schools is vital to improving our educational system.”37 As Lost Classroom makes clear, the supply of schools that serves as one obvious model of how to better serve those students most in need of quality schooling, Catholic schools, is decreasing (rather than increasing) over time. Even more alarming is that the diminishing number of Catholic schools hits urban America with disproportionate force.

To illustrate one important way in which urban Catholic school closures intersects with system-wide urban school reform efforts, one need only look to Cleveland, Ohio. The very program that gave rise to the litigation that culminated in the U.S. Supreme Court’s Zelman decision began as a statewide effort to address Cleveland’s struggling public schools. Confronting decades of underperforming public schools in Cleveland, the Ohio General Assembly responded in 1995 by passing the Ohio Scholarship and Tuition Program.38 While the statute was drafted to benefit any family in any Ohio school district “under federal court order requiring supervision,” when the program began only the Cleveland public school district fell into that category.39 The program permitted eligible families to direct a limited amount of public funds for tuition aid at eligible and participating public and private (including religious) schools.40

Similar to Chicago, the focus of Brinig and Garnett’s study,41 Cleveland’s private school landscape is noted for successful Catholic schools. As the Justices dissenting in Zelman emphasized, many (82%)42 of the private schools that participated in the Ohio voucher program were religiously affiliated,43 and these religiously affiliated schools served most (96%) of the participating students during the 1999–2000 school year.44 While scholars may debate the educational efficacy of Ohio’s voucher program, few contest the important role that religiously affiliated schools, notably Cleveland’s Catholic schools, played in the implementation and

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36. For a summary of the various voucher programs, many (but not all) of which include Catholic schools, see generally CLINT BOLICK, VOUCHER WARS: WAGING THE LEGAL BATTLE OVER SCHOOL CHOICE (2003); PRIVATE VOUCHERS (Terry M. Moe ed., 1995).
37. Kahlenberg, supra note 31, at 55.
39. Id. § 3313.975(A); Zelman v. Simmons-Harris, 536 U.S. 639, 644–45 (2002).
41. BRINIG & GARNETT, supra note 1, at 57.
42. Zelman, 536 U.S. at 647.
43. Id. at 687 (Souter, J., dissenting).
44. Id. at 647 (majority opinion).
execution of the Ohio program. (Paradoxically, Cleveland religious schools’
relative and absolute success in competing for students benefitting from
publicly funded vouchers was pointed to by dissenting Justices as a reason to
strike down the Ohio program as a constitutionally impermissible
establishment of religion.)45 Indeed, to the extent that the program succeeded
by offering more low-income Cleveland families access to Catholic schools,
the viability (and replicability) of such success is directly challenged by Lost
Classroom’s findings. That is, ironically, as new voucher programs (in
various forms) continue to emerge and existing programs expand, they now
do so in an environment with fewer urban Catholic schools.46

B. Losing Classrooms and Communities

Catholic school closures’ deleterious impact on urban school reform
efforts, however, are only part of Brinig and Garnett’s story, as they extend
their thesis from classrooms and urban school reform efforts to the
neighborhoods and communities that lose Catholic schools. In so doing, Lost
Classroom broadens the conceptual focus of Catholic schools from
educational institutions to community institutions. Once reconceptualized
more broadly as community institutions, the risk to neighborhood stability
posed by Catholic school closures comes into sharper focus. Specifically,
the authors argue that Catholic school closures trigger a net reduction in
social capital-building neighborhood institutions and, as a result, contribute
to the further destabilization of many already stressed urban areas.47
Brinig and Garnett’s broader claim is both ambitious and difficult to sustain,
particularly given limited data and especially knotty and complex causation
and endogeneity issues. This is not to say that Brinig and Garnett’s instincts
are incorrect. Rather, readers’ views may differ on the sufficiency of the
evidence upon which the authors base their claims.

Lost Classroom construes the threat to neighborhood destabilization in
terms of social cohesion measures (drawn from survey data) and data on
major crimes at the police-beat level from 1999 to 2005 (in Chicago) and at
the census-tract level in Philadelphia and Los Angeles.48 The authors set out
to exploit the comparative potential offered in Chicago by the existence of
Catholic and public charter schools. They find that the presence of a Catholic

45. Id. at 727 (Breyer, J., dissenting) (arguing that the “considerable shift” of taxpayer dollars
to private religious schools exacerbates constitutional problems).
46. See BRINIG & GARNETT, supra note 1, at 9–32 (detailing the increasing number of closures
of Catholic schools). For an inventory of school choice programs, see, for example, ALLIANCE FOR
program expanded, see EdChoice Scholarship Program, OHIO DEPARTMENT EDUC.,
http://education.ohio.gov/Topics/Other-Resources/Scholarships/EdChoice-Scholarship-Program,
archived at http://perma.cc/6Z2K-4AJQ.
47. BRINIG & GARNETT, supra note 1, at 3–4.
48. Id. at 3, 5 (Chicago); id. at 103 tbl.6.3 (Philadelphia); id. at 106 (Los Angeles).
school correlates with a higher level of social cohesion and comparatively less crime. In Chapter 5, the authors endeavor to refine their analyses further by comparing Catholic and public charter schools’ independent influence on neighborhood crime rates. Notably, Brinig and Garnett report that while the presence of a Catholic school correlated with a lower neighborhood crime rate, the presence of a public charter school did not influence neighborhoods at any statistically significant level. While the authors convey confidence in their causal claims regarding Catholic school closures and increased crime rates, the authors soften their conclusions relating to public charter schools due to technical and complex causation issues.

While *Lost Classroom* admittedly focuses on Catholic school closures and their implication for neighborhood cohesion, greater attention to research on school closures in other contexts might be warranted and helpful. For example, a nod to the public school consolidation literature may have assisted (and supplemented) the authors’ difficult task of tethering Catholic school closures and neighborhood degradation. Economies of scale, shifting demographic patterns, birth rates, and an array of other factors have increased stress on many public school districts and individual schools. The public school consolidation trend, which gained steam during the 1980s, was (and continues to be) acutely felt in many rural communities. Setting aside more mundane concerns—such as the implications for athletic team rosters that inevitably arise when a school closes and its students are absorbed into another school—more troubling are the broader and deeper threats to a rural community’s very economic existence following the closure of its local high school. Scholars have noted that rural school consolidations generate harms that implicate students and threaten rural communities’ vitality.

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49. *Id.* at 75.
50. *Id.* at 82–83.
51. *Id.* at 95–98 & fig.5.1, tbl.5.2.
52. *Id.* at 98 (“[W]e strongly suspect that the link between open Catholic schools and reduced crime is a causal one. We have no similar hunches about [public] charter schools.”).
53. See generally Amy Stuart Wells et al., Ctr. For Understanding Race and Educ., *Divided We Fall: The Story of Separate and Unequal Suburban Schools 60 Years After Brown v. Board of Education* (2014) (describing how “changing racial and ethnic demographics” are impacting school districts); Robert J. Tholkes & Charles H. Sederberg, *Economics of Scale and Rural Schools*, RES. RURAL EDUC., Fall 1990, at 9 (analyzing the potential impact of economics of scale on rural school districts).
The complex research design and data demands that complicate Brinig and Garnett’s analyses (discussed below) reveal a broader question with potentially important educational policy ramifications: Assuming Brinig and Garnett’s findings are correct, what explains why Catholic and public charter schools may differ when it comes to generating neighborhood-level social capital? To be sure, as the authors correctly note, given the political dynamics incident to any public school system and institution, charter schools can vary, sometimes tremendously. And some—perhaps much—of this variation is intended.\(^\text{56}\)

Despite critical variation, public charter and Catholic (and other non-public) schools share certain key attributes. For example, charter and Catholic schools share some amount of entrepreneurial activity. Catholic schools, of course, are private religious organizations nested within the Catholic Church. While charter school laws vary across states, charter schools, similar to Catholic schools, are typically “created as the result of private, entrepreneurial action—that is, at the request of a private entity (the charter ‘operator’) for permission to open a school made to a governmental entity (the charter ‘sponsor’) . . . [and] operate more or less independently of local school authorities.”\(^\text{57}\) Another—or, perhaps, the—key shared ingredient, of course, is that parents and students choose to attend charter and Catholic schools rather than schools assigned to them by the government.

Thus, for the narrow purpose of comparing schools’ potential for social capital building, theory (and common sense) suggests that owing to key shared attributes charter and Catholic schools would also share a similar potential and capacity for generating desirable social capital. If so, then there is no particular reason to assume, ex ante, that public charter and Catholic schools would behave differently when it comes to social capital building.

Yet this is precisely what Lost Classroom implies. Specifically, to help build their case that Catholic schools contribute important social capital to their neighborhoods and communities (in terms of crime reduction), Brinig and Garnett suggest that Catholic schools generated social capital that Chicago’s public charter schools did not.\(^\text{58}\) The empirics behind their claim are complicated and limited by existing data. Assuming Brinig and Garnett are correct, we need to explore possible explanations for their findings.

Prior work by one of the authors, developed further in Lost Classroom, provides one possible explanation. Garnett has previously argued (relying

\(^{56}\) See generally Chester E. Finn, Jr. et al., Charter Schools in Action (2000) (noting the different types of schools that constitute the charter movement).


\(^{58}\) See infra notes 59–60 and accompanying text.
on prior work by James Coleman and Anthony Bryk\(^\text{59}\) that what distinguishes charter and Catholic schools is the latter’s capacity for developing “intentional communities,” which, in turn, make unique social capital contributions.\(^\text{60}\) If correct, and if a “Catholic school effect” in fact exists, this would at the very least explain why charter and Catholic schools—both of which share the critical aspect of choice—may not share similar outcomes when it comes to social capital production. But even if this account is correct, it does not explain the potentially more troubling point about why other educational institutions, including some public educational institutions, seem unable to replicate the Catholic school effect.

While Chapter 7 helpfully explores this precise question,\(^\text{61}\) in the end readers are left still grasping at some straws on two key points. First, whether Catholic schools possess something of a monopolistic lock on generating both the educational and more general social capital building (or “positive externalities”) consistent with the authors’ main empirical findings. Second, if a Catholic school effect exists, is it replicable by public (or other) educational institutions that benefit from some level of parental choice? That is to say, in the popular policy parlance, can policymakers replicate and “scale-up” the traditional successes enjoyed by urban Catholic schools? As more and more Catholic schools close and depart urban areas, answers to these questions become increasingly important.

While Brinig and Garnett dutifully catalogue an array of possible explanations for urban Catholic schools’ comparative successes, ranging from Jane Jacobs’s defense of urban life to Professor William Fischel’s neighborhood networks thesis,\(^\text{62}\) one critical explanation—selection effects\(^\text{63}\)—injects itself once again as plausibly salient. The very household characteristics that prompt families to select into urban Catholic schools may also spill over into the observed neighborhood-level positive externalities. That is, the positive externalities may be a function of the students and their

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59. See generally BRYK ET AL., supra note 28, at 272–76 (discussing the “impact of communal organization” affected by Catholic schools); COLEMAN ET AL., supra note 28 (applying statistical analysis to determine both the individual student and community outcome difference between public, Catholic, and private schools).

60. Garnett, supra note 57, at 1908.

61. BRINIG & GARNETT, supra note 1, at 112–36.

62. Id. at 119–23; see also WILLIAM A. FISCHEL, THE HOMEVOTER HYPOTHESIS 142–43, 154–55 (2001) (explaining that neighborhoods with better performing schools have higher property values and that efforts to equalize spending in school districts with high and low property values have not caused “measurable academic improvement”); JANE JACOBS, THE DEATH AND LIFE OF GREAT AMERICAN CITIES 119–21 (1961) (highlighting the importance of the “self-government functions of streets” in creating successful neighborhoods); William A. Fischel, Why Voters Veto Vouchers: Public Schools and Community-Specific Social Capital, 7 ECON. GOVERNANCE 109, 112–17 (2006) (emphasizing the role of local public schools in generating “social capital” between parents and stating that this does not happen in private schools because the parents all live in different communities).

63. See Garnett, supra note 57, at 1908 (describing selection bias as “the possibility that Catholic schools attract better students with more highly motivated parents than public schools”).
families—indeed of the Catholic schools that they attend. For the technical reasons described below, efforts to “control” for this aspect through comparisons to public charter schools—while helpful—remain underdeveloped.64 Again, this is not to say that the authors’ articulation of a theoretical foundation is wrong; rather, it is only that, owing to data and research design limitations, tests of the competing theoretical explanations remain inconclusive. And, candidly, perhaps social science is only capable of “inconclusiveness” in this context.

D. An Empirical Lens

If what Lost Classroom says is not important enough, how it seeks to persuade readers also warrants attention. Data and research design contribute to the foundation upon which Brinig and Garnett’s argument rests. Lost Classroom’s adoption of an empirical lens is welcome, appropriate, and helpful. Indeed, the nature of the authors’ claims lends them to empirical exploration and testing. The book’s empirical turn both reflects—and contributes to—a broader trend in legal scholarship. Finally, Lost Classroom’s empirical lens identifies strains of research that warrant further scholarly attention. At some risk of getting bogged down in the technical, arcane thicket, however, this subpart briefly places the authors’ decision to approach their topic from an empirical perspective into some context and focuses on the Lost Classroom’s core empirical chapters.

1. Lost Classroom’s Quantitative Turn.—While Lost Classroom’s turn toward the empirical is both noteworthy and adds to the work’s contribution, it is not without technical difficulty and some peril. Empirical accounts, such as Lost Classroom, must squarely address, among other issues, data limitations and causation questions.

Chapter 3’s focus on a neighborhood’s social cohesion and order culminates with the conclusion: “All of these results suggest that Catholic schools are important, stabilizing forces in urban neighborhoods and that [Catholic] school closures lead to less socially cohesive, more disorderly, neighborhoods.”65 The authors’ support for this empirical proposition involves survey data from the Project on Human Development in Chicago Neighborhoods (PHDCN).66 Not surprisingly, the authors make much of these admittedly troubling findings.

Less well developed in the analysis, however, are questions concerning potential data limitations. Understood more narrowly, the PHDCN data permit conclusions about respondents’ perceptions of social cohesion and neighborhood disorder.67 To be sure, while perceptions can accurately reflect

64. See infra section III(D)(1).
65. BRINIG & GARNETT, supra note 1, at 71.
66. Id. at 57–58.
67. Id. at 68–70.
objective reality, sometimes perceptions do not. Moreover, it may well be
that in certain contexts perceptions are more important than objective reality,
particularly as it relates to parental decisions about residential and school
options for their kids. Finally, the PHDCN data used in this study are cross-
sectional rather than the preferred longitudinal. In any event, even if the
survey results mean what they say, they nonetheless remain just that:
respondents’ perceptions rather than observable conduct.

The book’s transition from Chapter 3 to 4 moves readers from survey to
observational (here, crime) data. In so doing, Brinig and Garnett move from
one set of methodological issues to another. On the one hand, the reliance
on Chicago crime data speaks directly to the question addressed: Namely, did
evidence of increased social disorder presented in Chapter 3, triggered by
Catholic school closures (between 1990 and 1996), correlate with increased
crime (between 1999 and 2005)? The reliance on standard crime data (here,
the authors exploit police beat-level data on six major crimes for six years)
benefits from a growing scholarly lineage.

On the other hand, however, because the authors seek to isolate the
unique contribution to crime rates from Catholic school closures, if any,
complex causation problems lurk. One standard confounder, the independent
influence of crime trends over the course of the relevant time period, warrants
attention. As the authors note, crime declined nationally and in Chicago
between 1999 and 2005. Thus, any independent Catholic school effect must
be assessed within a dynamic environment noted for decreased crime. The
authors’ effort to do just this warrants praise for ingenuity and creativity and
reveals the authors’ deep granular understanding of Catholic schools and the
parochial school setting. Specifically, the authors crafted pastor-level
instrumental variables, including “irregular” pastor leadership signals,
designed to coherently predict Catholic school closures. Analytically, the
key assumption is that these pastor-level instrumental variables predict
Catholic school closures independent of surrounding demographic variables.
After endeavoring to adjust for the relevant, likely confounding
time trends, the authors find the rates of decline in crime in neighborhoods
that include Catholic schools were systematically steeper than the rates of
decline in neighborhoods that experienced a Catholic school closure between
1990 and 1996.

A randomization strategy (the proverbial “gold standard” research
design) assists in identifying possible causation between or among frequently

68. See id. at 66 (noting that the PHDCN data could not be used “to analyze how school closures
affect disorder and social cohesion over time”).
69. Id. at 67–75.
70. Id. at 78.
71. Id. at 79 & fig.4.1.
72. Id. at 59–68 & tbl.3.1.
73. Id. at 67–68.
74. Id. at 80–81 & tbl.4.1.
interacting variables. Obviously, randomization efforts are both more common and possible in sterile laboratory environments. In the real world of social science, however, research design possibilities for those seeking to study legal rules and educational institutions—contexts that involve human beings—are frequently more limited and randomization strategies prove far more difficult. Similarly, the data used by Brinig and Garnett are, by definition, not purely randomized. From a research design perspective, one would want a pool of literally identical neighborhoods in which a random draw of Catholic schools would close. Real-world constraints (as well as increasingly aggressive university institutional review boards) render ideal research designs in most studies virtually impossible. As it stands, Lost Classroom draws heavily from one city (Chicago) and exploits more limited data from Philadelphia and Los Angeles that, perhaps unsurprisingly, introduce further complicating wrinkles into the results. While I am persuaded that Brinig and Garnett’s general intuition is generally correct, as it stands now it remains just that—largely intuition. Greater clarity on whether Lost Classroom’s core findings are generalizable to other cities and contexts will remain for future research.

An even more nuanced empirical concern involves causal direction. In an effort to tether this work to James Wilson and George Kelling’s influential “Broken Windows” thesis, not surprisingly Brinig and Garnett would like to tell a neat, tidy, and concise story about how Catholic school closures trigger social disorder, which, in turn, contributes to increased crime rates. Indeed, they go on to note that they “strongly suspect” such a causal link. Given the enormous complexities incident to such issues and mindful of data limitations, Brinig and Garnett prudently (and correctly) push away from any strong claims on this front and concede that they cannot “know the order of the causal chain linking school closures, disorder, social cohesion, and crime.” Many readers, of course, may wish for greater clarity on such a core question. Whether satisfactory clarity is possible, however, is unclear given inherent data and methodological limitations.


76. For examples of empirical legal studies that exploit a randomization strategy, see generally D. James Greiner et al., The Limits of Unbundled Legal Assistance: A Randomized Study in a Massachusetts District Court and Prospects for the Future, 126 HARV. L. REV. 901 (2013); D. James Greiner & Cassandra Wolos Pattanayak, Randomized Evaluation in Legal Assistance: What Difference Does Representation (Offer and Actual Use) Make?, 121 YALE L.J. 2118 (2012).

77. See supra note 48 and accompanying text.

78. See generally James Q. Wilson & George L. Kelling, Broken Windows: The Police and Neighborhood Safety, ATLANTIC MONTHLY, Mar. 1982, at 29 (illustrating that increases in “untended” property cause many residents to think that crime is increasing and to “modify their behavior accordingly”).

79. BRINIG & GARNETT, supra note 1, at 98.

80. Id. at 88.
In Chapter 5, the authors describe their efforts to exploit (admittedly limited) charter school data in Chicago in an effort to better distill a potential Catholic school effect.81 The effort—while certainly creative and holding promise—also introduces additional causal questions as the interactions involving the array of motivations—political, social, educational, and economic—behind decisions to open charter schools and background demographic factors inject further methodological complications. Unlike their efforts involving the Catholic schools, the authors decline to opine about the complex causation issues incident to the need to disentangle the location of charter schools (principally, K–6 or K–8 schools) from the surrounding neighborhood demographics.82 By not doing so the analysis cannot statistically cabin various neighborhood-level demographic factors that plausibly influence crime rates. Data and methodological limitations notwithstanding, the authors report that while the presence of a Catholic school correlates with reduced crime, the presence of a Chicago charter school did not materially influence crime rates in any direction.83 To their credit, the authors recognize the data and modeling limitations they confront, and when it comes to Chicago’s charter schools, the authors appropriately limit their causal claims.84

Just as Lost Classroom identifies serious challenges for students, their parents, and urban neighborhoods that, given current trends, will likely only increase in scope over time, the complex nature of the research challenge imposes serious methodological difficulties on researchers seeking to understand with precision relations among highly complicated variables that can interact in unanticipated ways. By standing down a bit, resisting an impulse for full-throttled claims, and recognizing important boundaries beyond which empirical data and research designs cannot reasonably sustain desired conclusions, Lost Classroom gains more than it loses. The authors’ modest and cautious tone increases readers’ confidence in their analyses and more accurately reflects the technical degree of difficulty associated with their research project.

2. Empirical Legal Studies.—Technical and methodological difficulties notwithstanding, Lost Classroom’s empirical turn both reflects—and contributes to—a broader trend in legal scholarship. The growth of empirical legal scholarship is, for better or worse, undeniable.85 While fuller accounts of empirical legal study’s intellectual development reveal various false starts in the past, over the past few decades an increase in sophisticated empirical

81. Id. at 90–98.
82. Id. at 92–94.
83. Id. at 96–97 & tbl.5.2.
84. Id. at 97–98.
85. In the interest of full disclosure, I co-edit the Journal of Empirical Legal Studies and, as such, am especially partial to this particular field and its growth.
legal studies became increasingly difficult to ignore. For example, Professor Robert Ellickson’s citation study of legal scholarship trends included an assessment of empirical legal scholarship’s growth in law reviews between 1982 and 1996. Professor Ellickson’s conclusion—that the data only “hint that law professors and students have become more inclined to produce (although not consume) quantitative analyses”—generally comported with prevailing wisdom grounded in growing anecdotal evidence. Five years later, Professor Tracey George updated Ellickson’s study and analyzed a more recent cohort of publications (1994–2004). Professor George concluded that empirical legal scholarship, or more accurately the number of references to it, “continues to grow.” Writing in 2006, Professor George described empirical legal scholarship as “arguably the next big thing in legal intellectual thought.” Five years later in 2011, I updated the Ellickson (and George) study once again and found that what Ellickson described as a “hint” one decade earlier had emerged into a palpable trend that has sustained over time. Even more recently Professor Joshua Fischman declared: “Today, empirical legal scholarship is flourishing again . . . .” Lost Classroom both reflects and contributes to this flourishment.

3. Next Steps.—Having successfully carved new scholarly terrain, one important test of any piece of original scholarship, such as Lost Classroom, is the degree to which it stimulates future research that builds upon or expands it. While only time will tell whether Lost Classroom will succeed in this manner, one must certainly hope for such success. In this regard, two particular areas that Lost Classroom identifies strike me as unusually ripe for further research attention. First, in an effort to consider whether their core findings in Chicago are present elsewhere, Brinig and Garnett incorporate two other large American cities, Los Angeles and Philadelphia, into their study. Los Angeles and Philadelphia receive comparatively less attention, however, as the authors correctly note. By making their analytic template

86. For a fuller account of several examples of empirical research, see generally JOHN HENRY SCHLEGEL, AMERICAN LEGAL REALISM AND EMPIRICAL SOCIAL SCIENCE (1995).
88. Id. at 528–29 & tbl.4.
90. Id. at 147.
91. Id. at 141.
94. See BRINIG & GARNETT, supra note 1, at 99 (describing their attention to Los Angeles and Philadelphia as “more summary”).
clear and preliminarily extending their research on Chicago to other cities, *Lost Classroom* serves as a virtual research roadmap for future scholars seeking to expand upon Brinig and Garnett’s work into other cities.

Second, as previously discussed, *Lost Classroom*’s effort to press Chicago’s public charter school data into the service of identifying the unique influence of Catholic school closures introduced necessary methodological complexity. While the causal issues are knotty, further work developing (in Chicago and elsewhere) this aspect may yield important insights. Moreover, an intriguing theoretical anomaly persists: Insofar as both public charter schools and Catholic schools share the parental choice variable, why only Catholic schools are capable of generating “intentional communities” warrants further attention.

IV. Conclusion

Imagining cities without Catholic schools (or far fewer of them), as the authors expressly do in Chapter 9,95 is not for the faint of heart. The acceleration of Catholic school closures threatens a two-part punch to many urban areas. First, Catholic school closures reduce access to what for many is a more efficacious educational opportunity in areas (urban centers) that desperately need higher performing educational institutions. The second punch, while perhaps more subtle or diffused, nonetheless damages as well. To fully thrive, many urban centers would benefit from a higher proportion of middle-income households, especially those with school-age children. To either attract or retain such middle-income households, however, stable, successful schools remain critical. For generations, urban Catholic schools contributed mightily toward both deflecting middle-income families from departing urban areas for suburban areas and, most prominently, suburban schools. Catholic school closures, then, will likely accelerate the migration out of urban areas of those families (regardless of race or ethnicity) who benefit from the economic means to move to suburbs. And, of course, these families are among those particularly well positioned to add social capital to many urban neighborhoods. If such a migration reaches a tipping point, previously functioning urban neighborhoods could destabilize.

Discomfort aside, we must remain unflinchingly frank—what *Lost Classroom* tells us is not good. It is certainly not good for many urban households with school-age children, education reform efforts, and, as well, though perhaps with less empirical certainty, for many American cities. What makes this bad news even more difficult to digest is that we understand with acute clarity the many negative consequences that flow from an acceleration of Catholic school closures. What helps transform this bad news into tragic news is that the consequences are—and will continue to be—borne

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95. *Id.* at 157–66.
by those most in need of high-quality educational services: low-income urban families, principally of color.

Also contributing to public unease over the implications flowing from Brinig and Garnett’s important research is that, to some degree, this problem could have been abated. Counterfactuals remain difficult, as we will never truly know whether the Chicago Archdiocese (and archdioceses in Los Angeles, Philadelphia, and elsewhere) would have continued to cross-subsidize parochial schools and, if so, for how long into the future. Also, while an array of macroeconomic and demographic trends can easily overwhelm, that “irregular” parish-level pastoral leadership helped facilitate Catholic school closures, and that some of these irregularities relate to the devastating sexual abuse scandals, convey an unpleasant level of self-infliction at play as well.96

In the end, however, this is a sad and important story for an array of reasons. If the Catholic school closure trend does not look like it will abate anytime soon, perhaps policymakers can learn from Lost Classroom and devise policies that will help to better preserve effective classrooms and urban neighborhoods. That is, perhaps Catholic schools’ legacy can at least partially offset the consequences from Catholic school closures in American cities.

96. Id. at 64.