MARK D. ROSENBAUM (BAR NO. 59940)
CATHERINE E. LHAMON (BAR NO. 192751)
PETER J. ELIASBERG (BAR NO. 189110)
ACLU Foundation of Southern California
1616 Beverly Boulevard
Los Angeles, California 90026
Telephone: (213) 977-9500

JACK W. LONDEN (BAR NO. 85776)
MICHAEL A. JACOBS (BAR NO. 111664)
MATTHEW I. KREEGER (BAR NO. 153793)
LEECIA WELCH (BAR NO. 208741)
J. GREGORY GROSSMAN (BAR NO. 209628)
Morrison & Foerster LLP
425 Market Street
San Francisco, California 94105-2482
Telephone: (415) 268-7000

ALAN SCHLOSSER (BAR NO. 49957)
KATAYOON MAJD (BAR NO. 211756)
ACLU Foundation of Northern California
1663 Mission Street, Suite 460
San Francisco, California 94103
Telephone: (415) 621-2493

JOHN T. AFFELDT (BAR NO. 154430)
Public Advocates, Inc.
1535 Mission Street
San Francisco, California 94103
Telephone: (415) 431-7430

Attorneys for Plaintiffs
ELIEZER WILLIAMS, etc., *et al.*

SUPERIOR COURT OF THE STATE OF CALIFORNIA

COUNTY OF SAN FRANCISCO

ELIEZER WILLIAMS, a minor, by Sweetie Williams, his guardian ad litem, *et al.*, each individually and on behalf of all others similarly situated, Plaintiffs, v. STATE OF CALIFORNIA, DELAINE EASTIN, State Superintendent of Public Instruction, STATE DEPARTMENT OF EDUCATION, STATE BOARD OF EDUCATION, Defendants.	No. 312236 **DECLARATION OF LEECIA WELCH IN SUPPORT OF PLAINTIFFS' DESIGNATION OF REBUTTAL WITNESS SAMUEL R. LUCAS** Date Action Filed: May 17, 2000

I, LEECIA WELCH, hereby declare as follows:

1. I am an attorney licensed to practice law in the State of California. I am an associate at the law firm of Morrison & Foerster LLP, counsel of record for plaintiffs Eliezer Williams, et al. ("plaintiffs") in this action. I have personal knowledge of the facts stated herein and could testify competently to them if called to do so.

2. Plaintiffs have provided a list of the persons whose expert opinion testimony the plaintiffs intend to offer on rebuttal at trial of this action, either orally or by deposition testimony. The list includes Samuel R. Lucas, to whom this declaration refers.

3. Dr. Lucas has agreed to testify at trial.

4. Dr. Lucas will be sufficiently familiar with the pending action to submit to a meaningful oral deposition concerning the specific testimony, including any opinions and their bases, that he is expected to give at trial.

5. Dr. Lucas's fee for providing deposition testimony, consulting with the attorneys for plaintiffs, and researching and related activities undertaken in preparation of the attached rebuttal expert report is $200 per hour.

6. Attached to my declaration as Exhibit A and incorporated by this reference is a *curriculum vitae* providing Dr. Lucas's professional qualifications, pursuant to section 2034(f)(2)(A) of the California Code of Civil Procedure.

7. Attached to my declaration as Exhibit B and incorporated by this reference is Dr. Lucas's rebuttal expert report. The following is a brief narrative statement of the general substance of the testimony that Dr. Lucas is expected to give at trial, pursuant to section 2034(f)(2)(B) of the California Code of Civil Procedure. Dr. Samuel R. Lucas rebuts opinions offered in the expert reports of State experts Eric Hanushek, Caroline Hoxby, and Christine Rossell. In particular, Dr. Lucas identifies a series of methodological defects evident in the State experts' analyses and explains that once these defects are corrected, the very evidence these State experts rely on supplies solid evidence against the State's claims. In addition, Dr. Lucas shows that because the State experts test only linear effects, they reduce the chance that the threshold effects that are more relevant to this case will be found. Dr. Lucas concludes that the sum total of

the corrected and extended analysis of the State experts' data is that school resources do matter. Finally, Dr. Lucas evaluates the role social science research plays in deciding the fundamental equal protection principles at stake in this litigation. The foregoing statements are only a general summary of the issues and conclusions discussed and documented more fully in Dr. Lucas's rebuttal expert report, attached as Exhibit B.

I declare under penalty of perjury under the laws of the State of California that the foregoing is true and correct.

Executed at San Francisco, California, this 15th day of September, 2003.

Leecia Welch

EXHIBIT A

Addresses: Sociology Department
University of California-Berkeley
410 Barrows Hall # 1980
Berkeley, California 94720-1980
Telephone: (510) 642-9564
e-mail: Lucas@demog.berkeley.edu
home-page: http://sociology.berkeley.edu/faculty/lucas

3 Embarcadero West #148
Oakland, California 94607

Education

B.A. 1986 Haverford College, Major: Religion

M.S. 1990 University of Wisconsin-Madison, Major: Sociology

Ph. D. 1994 University of Wisconsin-Madison, Major: Sociology (Preliminary Examinations: Sociology of Education, Social Stratification, and Methods and Statistics), Minor: Econometrics and Statistics

Dissertation

Title: *Effects of Race and Gender Discrimination in the United States, 1940-1980*

Committee: Robert M. Hauser (Chair), Robert D. Mare, Adam Gamoran, Charles N. Halaby, Lauren Edelman, and Glen Cain

Honors and Awards

2002	Elected to Sociological Research Association
2000	Willard Waller Award for Most Outstanding Book in the Sociology of Education published 1997-1999 (for *Tracking Inequality*)
1997	The Gustavus Myers Center Award for the Study of Human Rights in North America (for *Inequality by Design*)
1992	Ford Foundation Minority Doctoral Dissertation Fellowship
1991	Passed with Distinction, Preliminary Examination in Social Stratification
1988	American Sociological Association Minority Graduate Fellowship (declined)
1988	Council on Institutional Cooperation Minority Graduate Fellowship (declined)
1988	University Fellowship, University of Wisconsin-Madison
1988	National Science Foundation Minority Graduate Fellowship

Major Grants

1997	Spencer Foundation Grant, $478,000, with Co-Principal Investigator Mark Berends, RAND
1996	United States Department of Education Field Initiated Studies Grant, $436,000, with Co-Principal Investigator Mark Berends, RAND

Books

Lucas, Samuel Roundfield. 1999. *Tracking Inequality: Stratification and Mobility in American High Schools.* New York: Teachers' College Press.

Fischer, Claude, Michael Hout, Martín Sánchez Jankowski, Samuel R. Lucas, Ann Swidler, and Kim Voss. 1996. *Inequality by Design: Cracking the Bell Curve Myth.* Princeton, New Jersey: Princeton University Press.

Refereed Articles

Lucas, Samuel R., and Mark Berends. 2002. "Sociodemographic Diversity, Correlated Achievement, and *De Facto* Tracking". *Sociology of Education* 75: 328-348.

Lucas, Samuel R. 2001. "Effectively Maintained Inequality: Education Transitions, Track Mobility, and Social Background Effects." *American Journal of Sociology* 106: 1642-1690.

Lucas, Samuel R., and Aaron D. Good. 2001. "Race, Class, and Tournament Track Mobility." *Sociology of Education* 74: 139-156.

Lucas, Samuel R. 1996. "Selective Attrition in a Newly Hostile Regime: The Case of 1980 Sophomores." *Social Forces* 75:511-533.

Review Essays

Lucas, Samuel R. 2000. "Hope, Anguish, and the Problem of Our Time: An Essay on Publication of *The Black-White Test Score Gap.*" *Teachers College Record.* 102: 463-475.

Keynote Addresses

"Re-Visioning Policy in an Effectively Maintained Inequality Regime," address at the Sociology of Education Association Annual Meeting, Pacific Grove, California, February 22, 2002.

Invited Lectures

"The Unremarked Revolution in Tracking: Implications for High School Outcomes." Presentation at The Virginia Center for Educational Policy Studies, Curry School of Education, University of Virginia, November 16, 2001.

"The United States as an Effectively Maintained Inequality Regime" 2001. Presentation at RAND in Washington, DC (Also presented at RAND in Santa Monica; the Sociology Department at Princeton University; and the Economy, Justice, and Society Series on Social and Economic Inequality, University of California-Davis).

"Track Assignment and the Black-White Test Score Gap: Divergent Evidence from Analyses of 1980 and 1990 Sophomores." Presentation at the Closing the Gap: Promising Strategies for Narrowing the Achievement Gap Between White and Minority Students," at the Brookings Institution, February 2, 2001. (Paper authored by Lucas, Samuel R., and Adam Gamoran).

"Black-White Differences in Course-Taking, 1980 and 1990." Presentation at the Millennium Conference: Achieving High Educational Standards for All, sponsored by The National Academies, Washington, DC, September 21-22, 2000.

"Prominent Explanations and Potential Prominent Factors in The Black/White Test Score Gap." Presentation at the Workshop on The Role of Tests in Higher Education Admissions, a National Research Council Workshop sponsored by the Board on Testing and Assessment and the Office of Scientific and Engineering Personnel, Washington, D.C., December 17-18, 1998.

"Why Intelligence Tests are of Limited Use as Measures of Skill." Presentation at The Interdisciplinary Workshop on Skills, Test Scores, and Inequality. Roy Wilkins Center for Human Relations and Social Justice, Hubert H. Humphrey Institute of Public Affairs, University of Minnesota, Minneapolis, Minnesota, June 2, 1997.

"Selective Attrition in a Newly Hostile Regime". Presentation to the Planning Policy and Leadership Studies Program, College of Education, The University of Iowa, May 8, 1997.

"A Matter of Life and Death: Race Discrimination, Sex Discrimination, and Mortality in the United States." Presentation to the Ford Foundation Conference of Fellows Work-in-Progress Session, Irvine, California, October 1994

Other Articles, Chapters, and Presentations

Lucas, Samuel R., and Mark Berends. 2003. "Race and Track Assignment in Public School." Presentation at the International Sociological Association Research Committee Number 28 Meeting, Tokyo, Japan, March 2003. (An earlier version was presented at the 40^{th} Anniversary Symposium at the Center for Demography and Ecology, University of Wisconsin, Madison, October 2002).

Lucas, Samuel R., and Adam Gamoran. 2002. "Tracking and the Achievement Gap," pp. 171-198 in *Bridging the Gap*, edited by John E. Chubb and Tom Loveless. Washington, DC: Brookings Institution Press.

Lucas, Samuel R. 2002. "Invited Commentary: Transitioning to Adulthood in a Turbulent Time." *Education Statistics Quarterly* Summer: 17-20

Lucas, Samuel R. 2002. "Using National Data to Interrogate Discrepant Findings on Race/Ethnicity and Tracking.." Presentation at the American Educational Research Association Annual Meeting, New Orleans, Louisiana, April.

Lucas, Samuel R., and Mark Berends. 2002. "Finding and Explaining School to School Variation in Race and Track Assignment". Presentation at the American Educational Research Association Annual Meeting, New Orleans, Louisiana, April.

Berends, Mark, Samuel R. Lucas, and R.J. Briggs. 2002. "Effects of Curricular Differentiation on Student Achievement: Longitudinal Analyses of High School Students." Presentation at Research Seminar II: Instructional and Performance Consequences of High-poverty Schooling, The Charles Sumner School Museum and Archives, March 11, 2002.

Lucas, Samuel R., Phillip Fucella, and Mark Berends. 2001. "Neo-Classical Education Transitions of Boomers and Post-Boomers in the United States: A Corrected Tale for Three Cohorts." Presentation at the International Sociological Association Research Committee Number 28 Meeting, Berkeley, California, August 2001.

Lucas, Samuel R. 2001. "Explaining the Dominance of Downward Track Mobility for the Mobile." Presentation at the Population Association of America Annual Meeting, Washington, DC, April 2001. (An earlier version was presented in August 1999 at the American Sociological Association Annual Meeting in Chicago, Illinois).

Berends, Mark, Samuel R. Lucas, and Thomas Sullivan. 1999. "Effects of Changing Family Background Characteristics on Black-White Test Score Trends, 1972-1992." Presentation at the International Sociological Association Research Committee Number 28 Meeting, Madison, Wisconsin, August 1999.

Lucas, Samuel R., and Aaron D. Good. 1999. "Race, Class, and Tournament Track Mobility" Presentation at the American Educational Research Association Annual Meeting, Montréal, Quebec, April 1999.

Lucas, Samuel R. 1998. "Bringing the Tracks (All the Way) Back In: Education Transitions, Track Mobility, and Waning Effects of Social Background." Presentation at the American Sociological Association Annual Meeting, San Francisco, California, August 1998.

Lucas, Samuel R. 1998. "Contesting Tournament Track Mobility in the United States" Presentation at the International Sociological Association Meetings, International Sociological Association Meetings, Research Committee Number 28 (Joint with Research Committee 04), Montréal, Quebec, July 1998.

Lucas, Samuel R. 1998. "A Review of *Readings on Equal Education, volume 14: Forty Years after the Brown Decision: Social and Cultural Effects of School Desegregation* edited by Charles Teddlie and Kofi Lomotey." *Contemporary Sociology* 27:352-353.

Lucas, Samuel R. 1997. "Context and College Entry." Presentation at the American Sociological Association Annual Meeting, Toronto, Ontario, August 1997.

Lucas, Samuel R. 1996. "Causes and Consequences of Scope: Ethnic Diversity, Class Heterogeneity, and Political Action." Presentation at the American Sociological Association Annual Meeting, New York, N.Y., August 1996.

Hout, Michael, and Samuel R. Lucas. 1996. "Narrowing the Gap Between Rich and Poor." *Chronicle of Higher Education* 42;49: B1-B2. (also excerpted in *Education Digest*)

Lucas, Samuel R. 1995. "Educational Transitions of 1980 Sophomores: Background, Achievement, and Delinquency" Presentation at the American Sociological Association Annual Meeting, Washington, D.C., August 1995.

Lucas, Samuel R. 1995. "Socioeconomic Conditions, Discriminatory Climate, and Mortality." Presentation at the Population Association of America Annual Meeting, San Francisco, California, April 1995.

Lucas, Samuel R. 1995. "A Review of *African-Americans: Essential Perspectives*, by Wornie L. Reed." *Contemporary Sociology* 24:193-194.

Lucas, Samuel R. 1992. "Secondary School Track Rigidity in the United States: Existence, Extension, and Equity." Presentation at the American Sociological Association Annual Meeting, Pittsburgh, Pennsylvania, August 1992.

Lucas, Samuel R., and Adam Gamoran. 1991. "Race and Track Assignment: A Reconsideration with Course-Based Indicators of Track Locations". Presentation at the American Sociological Association Annual Meeting, Cincinnati, Ohio, August 1991.

Lucas, Samuel R. 1988. "The NELS:88 Student Survey". Presentation at the American Education Research Association Annual Meeting, New Orleans, Louisiana, April 1988.

Lucas, Samuel R., Steven Ingels, and Louise Little. 1987. "Analysis of Field Test Student Data, Section 3.2: Student Questionnaire Data" in Steven Ingels, et al., *Field Test Report: National Education Longitudinal Study of 1988 (NELS:88)*, prepared for Center for Education Statistics, U.S. Department of Education.

Lucas, Samuel R., Steven Ingels, Harrison Greene, and Louise Little. 1987. "Student Data Collection" in Steven Ingels, et al., *Field Test Report: National Education Longitudinal Study of 1988 (NELS:88)*, prepared for Center for Education Statistics, U.S. Department of Education.

Work Under Review

Lucas, Samuel R., and Mark Berends. "Race and Track Location in U.S. Public Schools"

Teaching Interests

Sociology of Education
Social Stratification
Research Methods
Statistics

Administrative, Committee and Professional Service:

National

Member, Committee on Measuring and Assessing Discrimination, National Academy of Sciences, September 2001-

Member, Technical Review Panel, Education Longitudinal Study, July 2000-

Member, Committee on Representation of Minority Children in Special Education, National Academy of Sciences, May 1999-August 2001 (which published the report, *Minority Students in Special and Gifted Education*, edited by Suzanne Donovan and Christopher T. Cross).

Disciplinary

Member, American Sociological Association Task Force on the Amicus Brief for *Grutter v. Bollinger*, December 2002-February 2003

Editorial Board, *Contexts,* Jan 2003-Aug 2003

Chair, Ad Hoc Showcase Session Committee, Sociology of Education Section, American Sociological Association, September 2001-

Member, Sociology Advisory Panel, National Science Foundation, October 1999-September 2001

Consulting Editor, *American Journal of Sociology*, September 1999-August 2001

Nominations Committee, Section on Sociology of Education, the American Sociological Association, 1999-

Council Member, Section on Sociology of Education, the American Sociological Association, 1999-

Editorial Board, *Sociology of Education*, 1995-1997

Intern, *American Sociological Review*, 1991-92

University

Educational Technology Committee, University of California-Berkeley, January 2003-

Committee on Educational Policy, University of California-Berkeley, July 2002-

Committee for the Protection of Human Subjects, University of California-Berkeley, January 2001-June 2001

Chancellor's Committee to Draft a Proposal for an Organized Research Unit in Race and Gender, University of California-Berkeley, 1999-2000

Departmental

Chair, Affirmative Action Committee, Sociology Department, University of California-Berkeley, January 2001-June 2001

Awards Committee, Sociology Department, University of California-Berkeley, 1999-2000

Admissions Committee, Sociology Department, University of California-Berkeley, 1998-99

Affirmative Action Committee, Sociology Department, University of California-Berkeley, 1998-2000

Graduate Curriculum Committee/Committee on Academic Progress, Sociology Department, University of California-Berkeley, 1994-95

Faculty Computing Coordinator, Sociology Department, University of California-Berkeley, 1993-2000

Admissions Committee, Sociology Department, University of California-Berkeley, 1993-94

Curriculum Committee, Sociology Department, University of Wisconsin-Madison, 1989-90

Discussant at Professional Meetings

Discussant, "Educational Stratification I Session," International Sociological Association Research Committee 28 on Social Stratification and Mobility Winter Meeting, Tokyo, Japan, March 2003.

Discussant, "William H. Sewell Memorial Session," International Sociological Association Research Committee 28 on Social Stratification and Mobility Summer Meeting, Berkeley, CA, August 2001.

Discussant, "Achievement Studies in the Sociology of Education," American Sociological Association Annual Meeting, Washington, DC, August 2000.

Discussant, "Occupations and Inequality," International Sociological Association Research Committee 28 on Social Stratification and Mobility Summer Meeting, Madison, WI, August 1999.

Discussant, "Minority Labor Markets," Population Association of America Annual Meeting, San Francisco, CA, April 1995

Discussant, "Curricular Tracking Policies and Practices in American Public Schools," American Education Research Association, San Francisco, CA, April 1995

Discussant, "Race/Ethnicity and Educational Attainment: Multi-Ethnic Comparisons," American Sociological Association Annual Meeting, Los Angeles, CA, August 1994.

Organizer for Professional Meetings

Organizer, "Demography of Inequality" Session, Population Association of America Annual Meeting, Washington, DC, March 2001.

Professional Affiliations

American Sociological Association
International Sociological Association Research Committee 28
Population Association of America
American Educational Research Association

Reviewing

Reviewer for *American Journal of Sociology*, *American Sociological Review*, *Sociology of Education*, *Social Forces*, *Demography*, National Science Foundation, Spencer Foundation, Russell Sage Foundation, Office of Educational Research and Improvement, Ford Foundation

Positions Held

Visiting Scholar, Sociology Department, Harvard University, August 2001 - July 2002

Associate Professor of Sociology, University of California-Berkeley, July 1999 -

Assistant Professor of Sociology, University of California-Berkeley, July 1994 - June 1999

Acting Assistant Professor of Sociology, University of California-Berkeley, July 1993 - June 1994

Lecturer, Sociology 357: Methods of Sociological Inquiry, University of Wisconsin-Madison, September 1991 - May 1992

Research Assistant, Wisconsin Longitudinal Study, August 1991 - June 1992

Research Intern, Stratification of Learning Opportunities Project, August 1988 - August 1991

Assistant Survey Director, NORC, August 1986 - August 1988

Research Assistant, Women Against Abuse, Philadelphia, Pennsylvania, January 1986 - August 1986

Resident Director, A Better Chance in Ardmore, May 1985 - May 1986

Teaching Assistant, Religion 102: Introduction to Modern Theology, Haverford College January 1985 - May 1985

Resident Tutor, A Better Chance in Ardmore, January 1985 - May 1985

Supervisor, National Research, Incorporated, August 1983 - January 1984

Telephone Interviewer, National Research, Incorporated, May 1983 - August 1983

EXHIBIT B

Implications of the Stratification of Structural Learning Opportunities in California Schools:
Re-Analyses of Evidence on School Resource Effects in *Williams v. California*

Samuel R. Lucas
University of California-Berkeley*

*All analyses were conducted with the assistance of the Demography Department of the University of California-Berkeley. I thank Denis Trapido for research assistance. All errors are of course those of the author. Please direct all correspondence to Samuel R. Lucas / Sociology Department / University of California-Berkeley / 410 Barrows Hall #1980 / Berkeley, CA 94720-1980 or via e-mail to Lucas@demog.berkeley.edu

Introduction

Several experts have been brought in to address issues that might be relevant for judicial consideration in deciding *Williams v. California.* The analysis herein speaks to research provided by three experts for the State of California, defendants in this litigation--Professors Caroline Hoxby and Christine Rossell, and Dr. Eric Hanushek.

A key question Hoxby, Rossell, and Hanushek address is whether school resources matter. They address this issue using data drawn from nationally-representative datasets, data from one selected district in Georgia, and data covering California in slightly more depth. Yet, a series of methodological defects are evident in their analyses. Once those defects are corrected, the very data defendants' experts provided supplies solid evidence against the defendants' experts claims. Further, the question defendants' experts address is distinct from the question one would ask were one to use appropriate data to reflect upon the concerns raised by the plaintiffs in *Williams v. California.* Once we focus on a question more relevant for plaintiffs' articulated concerns, we can re-analyze defendants' experts data. And, once we re-analyze defendants' experts' data, focused on this question, we find powerful evidence in support of plaintiffs' claims.

We proceed by first re-assessing the analyses provided by Rossell, Hanushek, and Hoxby, in order to correct some of the larger methodological weaknesses in their analyses. We then replicate their key analyses, while correcting for those methodological limitations, in order to determine what one would learn were one to develop the same analyses the defendants' experts presented, save for the correction of major methodological weaknesses. Next, we re-analyze the data these scholars provided, in an effort to test claims more consistent with plaintiffs' concerns in this case. In doing so we demonstrate how one would proceed were one interested in gleaning from the statistical evidence information that might be most pertinent to the issues raised in *Williams v. California*. Finally, we assess the comprehensiveness of the analyses presented by Professors Hoxby, Rossell, and Dr. Hanushek, in light of additional complexities of schooling and school outcomes.

Do School Resources Matter on Average?

Hoxby, Hanushek, and Rossell all take up the question of whether school resources matter on average. Hanushek references changes in funding and student achievement and a selection of existing studies of the resource-achievement relation to assess the role of school resources on student outcomes. Hoxby uses National Education Longitudinal Study (NELS), California Standardized Testing and Reporting Program (STAR) data, and National Longitudinal Survey of Youth (NLSY) data to investigate whether school management matters and to decompose the effect of families, neighborhoods, and schools on outcomes of interest. Rossell uses California STAR data, state comparative data, and data on schools in one Georgia school district to investigate the impact of resources on student achievement.

There are different methodological weaknesses in all three efforts. It will pay to attend to each analysis in turn; hence, we will describe the problems with each and the likely consequences of the problem, repair the problem, and then consider the corrected findings which reflect what one would obtain had one conducted an appropriate analysis.

The Hanushek Report Perspective on the Resource-Outcome Relation

Hanushek presents two main types of evidence. First, he presents evidence on the time trend between school resources, indexed by student-teacher ratio, median years of teaching experience, percent of teachers with a master's degree or more, and per pupil spending, and compares these trends to trends in National Assessment of Educational Progress (NAEP) test scores. The gross trend is toward greater school resources. Yet, upon perusing the time-trend for NAEP test scores, Hanushek contends that mean NAEP test scores for 17 year olds are not associated with changes in school resources.

The first problem is that unsystematic study of a series of time-trends has been rejected as a serious method of investigation. All Hanushek does in this analysis is array a few time trends together, and speculate as to the non-effects of some trends for the other. Scholars have known for decades that such efforts are prone to mis-lead (e.g., Campbell and Stanley 1963). There are many reasons that the approach has been rejected; for example, it does not allow one to account for other variables that might conceal the effect that would be discerned in a systematically analyzed time trend, and it does not allow one to estimate the magnitude of an effect and a standard error on that magnitude estimate simultaneously. Hence, attempting to discern a relationship between factors by looking at a table of time trends has been discredited as a method. Although there may be no harm in perusing tables of trends, such an effort can only be exploratory, and should provide no more than a point of departure for a rigorous, systematic approach.

The second problem is one that Hanushek notes but then seems to dismiss. Hanushek recognizes that scholars have proposed two particular types of factors that may problematize the kind of time-trend comparison he presents: 1)the existence of changes in the student population and, 2)the existence of legislated increases in the demands on schools that would be expected to increase the cost of school and the level of training of personnel, but that would not be expected to positively affect measured achievement (e.g., national legislation mandating the provision of special education).

Hanushek acknowledges the existence of many trends in the student population, some that probably make matters better for children (e.g., increases in levels of parents' education) and some that probably make matters worse for children (e.g., increases in single parent household). Of these known trends Hanushek correctly notes that "[I]t is difficult to know how to net out these opposing trends with any accuracy" (Hanushek 2003, p. 5).

In contrast, when it comes to the issue of increasing demands placed on schools, demands such as the reasonable and legislated mandate that schools provide special education to mentally retarded or learning disabled students rather than expel them, Hanushek appears less cautious. He writes:

> [T]he magnitude of special education spending and its growth, however, are insufficient to reconcile the cost and performance dilemma. Using the best available estimate of the cost differential for special education--2.3 times the cost of regular education, the growth in special education students between 1980 and 1990 can explain less than 20 percent of the expenditure growth. In other words, while special education programs have undoubtedly influenced overall expenditures, they remain a relatively small portion of the total spending on schools. (Hanushek 2003, p.6, citations omitted).

A problem here is that the statement reflects only analysts' claims' as to the amount of the cost increase going to special education, but it does not reflect how other indices of school quality (e.g., teacher preparation, student-teacher ratios) may have been affected by the way in which spending was allocated. It is impossible to obtain data on the indices of school quality by special education status; that is, national data collection is insufficient to calculate measures of, for example, student-teacher ratios for special education students/classrooms and regular education students/classrooms. Such a calculation and others would be helpful in discerning whether and how changes in the overall inputs to education have been allocated differentially to different categories of students.

It is evident, however, that if one considers the very few resources that *are* broken out by special education status, one will find evidence suggesting that special education students have better resources at their disposal than do general education students. Results from the 1993-94 Schools and Staffing Survey indicate that during the 1993-94 academic year, 44.4 percent of all public high school teachers had Master's degrees, while 49.1 percent of public high school teachers teaching special education primarily had Master's degrees. Similarly, 46.9 percent of public elementary school teachers teaching special education primarily had a Master's degree, while only 39.7 percent of all public elementary school teachers, including special education teachers, had Master's degrees (Snyder, Hoffman, and Geddes 1999, Table 68). Hence, even if one accepts the ad hoc approach to time series analysis Hanushek offers in his report, the disparities in documented resources suggest that the allocation of resources to special education students, while laudable, did mean that the vast expansion in overall resources may have over-stated the amount of change in the quality of education for general education students. If so, comparing the gross trends in school inputs to the gross trend in test scores is difficult to justify even if one were to use rigorous time series methods. Thus, the limited data available, with the long distance from the classroom on which the comparisons are based, make this a fairly unpromising line of inquiry for discerning the impact of school resources on student achievement. Hence, the first kind of evidence Hanushek provides should be set aside, because it is analyzed in an unsystematic manner using an approach that is no longer accepted as appropriate, and because the data are not sufficient to the task.

Hanushek also provides what appears to be the results of a meta-analysis of several studies of the impact of various school resources on student achievement. Meta-analyses allow researchers to combine the information from dozens or hundreds of studies to ascertain whether the studies, taken together, reveal any systematic pattern of results. Meta-analyses can be a useful tool for establishing exactly where the weight of the evidence falls on questions of sufficient import to have generated a variety of studies on appropriate populations and samples. Using some 376 "studies" of the impact of school resources, Hanushek purports to show no consistent effect of school resources. Yet, methodological flaws in his meta-analysis render Hanushek's work erroneous.

Princeton economist Alan B. Krueger and colleague Diane M. Whitmore of the University of California-Berkeley have identified important flaws in Hanushek's analyses. Krueger and Whitmore (2002) note that in Hanushek's meta-analysis of class size research, he drew varying numbers of "studies" from different papers, where a "study" is defined as one statistical model. From some papers Hanushek drew only one or two "studies" or, more accurately, cases; from other papers Hanushek drew up to 24 different "cases." Hanushek followed the same procedure in studying research on expenditures per student. Hanushek summarizes these and other meta-analyses he has conducted in the expert report he submitted to this court.

Table 1 replicates a table drawn from a published paper by Krueger and Whitmore (2002) which corrects the errors in Hanushek's analysis. Column 4 in Table 1 re-presents the results Hanushek provides to the court in Table 2 of his report. Hanushek found that about one quarter of the estimates were positive and statistically significant, while approximately 7 percent were negative and statistically significant--the remainder, nearly two-thirds of the estimates, were not statistically significant.

Table 1 -- Krueger and Whitmore (2002) Re-Analysis of Hanushek Meta-Analyses of Class Size Effects Studies and Per Pupil Expenditure Effects Studies

	Weighting of Estimates/Data in Meta-Analyses					
	Class Size			Expenditure per Student		
Column Number	1	2	3	4	5	6
Results	# of Extracted Estimates	Equal	Journal Impact Factor	# of Extracted Estimates	Equal	Journal Impact Factor
Positive and Statistically Significant	14.8 %	25.5%	34.5%	27.0%	38.0%	28.0%
Positive and Statistically Insignificant	26.7%	27.1%	21.2%	34.3%	32.2%	30.0%
Negative and Statistically Significant	13.4%	10.3%	6.9%	6.7%	6.4%	10.0%
Negative and Statistically Insignificant	25.3%	23.1%	25.4%	19.0%	12.7%	10.0%
Unknown sign and Statistically Insignificant	19.9%	14.0%	12.0%	12.9%	10.7%	21.0%
Positive to Negative Ratio	1.07	1.57	1.72	2.39	3.68	2.90
p-value	0.500	0.059	0.034	0.0138	0.0002	0.0010

Note: table drawn from Krueger and Whitmore, 2002, p. 15.

The first point to remark is that Hanushek did not provide for the court the ratio of positive to negative estimates (the +/- ratio), nor did he provide a p-value for the test of that ratio. Krueger and Whitmore provide those ratios and the p-value. The p-value is important, for it allows us to assess the likelihood that the ratio we observe is a chance occurrence or, instead, reflective of something systematic. For example, we might want to discover whether a proprietor is cheating customers. We know that over the course of a day there are dozens of transactions, and errors do occur. We know that error may lead customers to sometimes over-pay (because they receive back insufficient change) and sometimes under-pay (because they receive back too much change). Yet, if the only factor driving over-paying and under-paying--over-charging and under-charging--is error at the cash register, the errors should on balance cancel out.

However, if we were to observe dozens of interactions between a proprietor and customers, and observed that customers were over-charged twice as many times as customers were under-charged, we could obtain a p-value on the over-charge/under-charge ratio. The p-value would indicate how likely it is that given the number of interactions we observed, we would find a 2/1 over-charge/under-charge ratio were the mis-charges purely the result of random error. The lower the p-value, the less likely we would expect the ratio we observe to be the result of random fluctuations across transactions. In other words, the lower the ratio, the more likely it is that some systematic factor is pushing our observation away from the 1/1 ratio one would expect were the process truly random. In the case of the proprietor, we would conclude that some factor was systematically tipping the balance in favor of more over-charges. And, owing to the interest of the proprietor in obtaining more money, we might suspect conscious cheating.

In the case of the expenditures per student meta-analysis, the ratio of positive to negative estimates, even with Hanushek's flawed analytic design, is 2.39--for every one estimate that is negative, there are more than two and one-third estimates that are positive. The p-value of .0138 indicates that it is very likely that some systematic factor is tipping the balance toward a positive effect conclusion. And, the most likely systematic factor in the case of school expenditures research is reality--the true effect of expenditures per student is unlikely to be zero. In sum, the p-value indicates that it is unlikely that the effect of per pupil expenditure equals zero, because a +/- ratio that large, in that many instances of research, is unlikely to be observed in a world with no effect of per pupil expenditure.

As one corrects the errors of Hanushek's design, one obtains even larger +/- ratios, that are even more statistically significant, that is, one obtains even stronger evidence that it is unlikely that there is no effect of per pupil expenditure. Krueger and Whitmore correct the errors in Hanushek's analyses in two ways, serially. First, they use the many estimates Hanushek used, but they count each study only once. Consider two studies that might have been conducted. One researcher may have gathered data in Philadelphia, and may have made two estimates of the effect of per pupil expenditure on student achievement, using different regression models with different sets of independent variables. Another researcher might have obtained data from rural Maine, and might have estimated six models studying the effects of interest, again with different model specifications. In Hanushek's design, the Maine study counts three times as much as the Philadelphia study. Yet, there is no statistical, theoretical, or methodological justification for triple-weighting one study vis à vis another. Indeed, counting each estimate *from* a study, rather than each *study*, as a separate case is statistically problematic because for many of the studies the same data is used to obtain each estimate; counting each estimate from the same study as if it were an independent estimate violates the assumption of independence (Krueger and Whitmore 2002, p. 17).

Columns 2 and 5 of Table 1 correct for this problem by making each study count as one study, i.e., all count equally. This correction greatly increases the +/- ratios reported in columns 2 and 5 compared to columns 1 and 4 respectively. The second correction is to adjust the estimates on the basis of the "journal impact factor." Krueger and Whitmore describe their procedure as follows:

> To crudely (but objectively) assign more weight to higher quality studies, the studies are assigned a weight equal to the 1998 impact factor of the journal that published the article, using

> data from the Institute for Scientific Information. The impact factors are based on the average number of citations to articles published in the journals in 1998. Impact factors are available for forty-four of the fifty-nine class size studies in the sample; the other fifteen studies were published in books, conference volumes, or unpublished monographs. Studies not published in journals were assigned the impact factor of the lowest ranked journal. . . . Although obvious problems arise with using journal impact factors as an index of study quality (for example, norms and professional practices influence the number of citations), citation counts are a widely used indicator of quality, and the impact factor should be a more reliable measure of study quality than the number of estimates Hanushek extracted. (Krueger and Whitmore 2002, pp. 17-18).

The meta-analysis that takes account of the quality of the journal in which the paper was published (to account for higher quality studies) also reveals a clear pattern of positive effects of lower class size and greater expenditures per student. Krueger and Whitmore provide a compelling response to Hanushek's meta-analyses, which can be summarized as follows: the meta-analytic evidence suggests there is an effect of school resources on student outcomes of interest.

Hanushek's report uses out-moded ad hoc time series analytic techniques and a patently flawed meta-analytic strategy. With respect to the time series analysis, the data are insufficient to even pursue the time trend analysis in any depth. And, the problem is compounded by the use of unsystematic techniques of analysis.

With respect to the meta-analysis, when other scholars have corrected the errors in the meta-analysis, they reveal evidence of an effect of school resources on student outcomes. Hence, even though the report Hanushek submitted variously presents weak data and uses inappropriate analytic approaches, it appears that an appropriate analysis of the subset of appropriate data Hanushek presented ends up undermining the defendants' expert's claim and providing potent evidence in favor of the plaintiffs' position. Resources matter.

The Hoxby Report Perspective on the Resource-Outcome Relation

Hoxby uses NELS data to study the alleged "school management" effect in California. The first methodological problem with Hoxby's analysis is simple: statistical inference requires the availability of a representative sample, but Hoxby does not have a representative sample of California schools.

The accepted method of obtaining a representative sample is to use probability sampling. If one uses probability sampling one will obtain a sample for which every member of the population has an in principle 1)knowable and 2)non-zero probability of selection into the sample--a probability sample (e.g., Kalton 1983).[1] The entire edifice of statistical inference--the use of standard errors, the calculation of p-values, the reporting of z-tests/t-tests of coefficients, the calculation of confidence intervals, the assessment of statistical significance--depends crucially upon the use of probability samples. If a researcher has a non-probability sample, then the use of statistical inference techniques (e.g., reports of statistical significance) are not defensible.

Hoxby uses National Education Longitudinal Study (NELS) data, a nationally representative dataset, to study how school-level resources correlate with student achievement after individual-level factors are controlled, *in California* (Hoxby 2003, p 6). Because the focus of the analysis is on determining whether school-level factors matter, her use of the NELS dataset is inappropriate given its sample design.

There are two reasons that make the NELS data inappropriate. First, schools in each state in the NELS sample do not provide a probability sample of the schools in the state. In other words, the sample design allowed the survey organization to have some schools in California, but those schools were selected to represent schools all across the country, not to represent the schools in California. What this means is that the sample of California schools will not

---

[1]Certainly, some research uses samples that do not completely meet this standard. For example, some public opinion polls for Presidential elections do not sample persons serving in the military overseas, although these persons are eligible to register and to vote in the election. Such studies *still* have a probability sampling design for the subset of persons of focus. And, owing to the small number of others removed from the population of focus, it is unlikely that the omissions imperil the effort to make predictions about the Presidential election.

serve as a probability sample for the state of California; the sample is only useful when combined with sampled students from all other states in the NELS dataset. Hence, the use of the California subset to generalize to California is unwarranted.

An analogy may clarify the matter. Some schools in NELS may be in Georgia, but the schools selected in Georgia were selected to allow researchers to assess the experience of students in locations prominent in Georgia, such as rural location, for example. But Georgia is not only rural, it also contains cities of some size (including Atlanta). Sampling statisticians may have completely ignored the presence of Atlanta, and designed the sample to pick up students in locations around the state so as to properly estimate the average *national* impact of, say, rural school location. The avoidance of some kinds of locations in a state is not a problem if the data is used to study the nation. But it becomes a serious problem if one wants to make inferences to the *particular* state of Georgia. Because the NELS sample design does not create state-level representative data, selecting the NELS students from one state for separate analysis, as Hoxby does, is inappropriate.[2]

The fore-going reason implies that any state-specific analysis of NELS

---

[2]Sampling statisticians are often trying to balance issues of monetary efficiency and parameter estimate efficiency. For this reason the aims of the study must be considered to determine whether a sample collected for one purpose may be used to study some other question. In the case of NELS, the data collection team sampled students in many states to produce a nationally-representative dataset. Cost considerations prohibited an effort to sample within states to produce state-representative data, unless the state augmented the sample with state funds to allow state-level representative data. California did not make such augmentations.

data need be rejected on its face, for the data collection design cannot sustain a state-specific analysis. Yet, a second reason suggests that one cannot answer the question Hoxby is attempting to answer using NELS, even if one were to use all of the schools in the sample. The second reason flows from the longitudinal nature of the NELS design, with data collected when members of the cohort were in grades 8, 10, and 12, with follow-ups thereafter. One of Hoxby's motivations for using NELS is to use 8th grade test scores as control variables in models of 10th grade achievement (Hoxby 2003, pp. 6-8). This strategy is fine for many analyses. But it is problematic if one aims to investigate school-level factors because of the sample design (see Ingels, et. al. 1994).

The base-year sample design called for schools with eighth grades to be sampled. Two years later survey personnel sought to follow students to their tenth-grade schools. This led to many problems for the kind of analysis Hoxby presents. First, because the vast majority of students were in two different schools in the two years of the study, and because Hoxby does not use an indicator that would proxy for how long the students attended each school (the 8th grade school and the 10th grade school), it is impossible for her analysis to identify any school-level effect using NELS data. The "school management effect" Hoxby reports for any school (Hoxby 2003, pp. 6-8) is at best an amalgam of factors operating at different schools--the 8th grade school and the 10th grade school.

Yet, even if one had such a proxy, problems would ensue, because, as the methodology report published by the data collection agency reveals, the students who are surveyed in the tenth grade at any particular school do not form a probability sample of students in the tenth grade *at that school* (Ingels et al. 1994). This means that it is inappropriate to use these data to estimate the relation between factors inside the school and student achievement. For example, perhaps a high school draws students from 3 or 4 middle schools. If so, and if only one of those middle schools were sampled in the base year, then the students who show up in NELS cannot represent the way in which the high school factors might matter for achievement, for they do not form a representative sample of their high school peers.

Analysts routinely include school-level factors in models of individual-level achievement, but for each of these analyses one must ask: "What is the focus of the analysis?" If the analyst aims to remove the possible biasing effect of those school-level factors *within* the particular sample being used, so as to clean up the assessment of matters that can be studied with NELS, i.e., if school-level factors are mere controls, then it is possible to proceed in the manner Hoxby does. But Hoxby's aim is to focus on the school-level coefficients (e.g., the alleged "school-management effect"), using the individual-level factors as controls, i.e., Hoxby's interest is the opposite of what is acceptable. Given the sample design one cannot follow the strategy Hoxby follows. Of course, statistical software will spit out regression coefficients, standard errors, and confidence intervals, but the analyst needs to assess whether these statistics are meaningful. Given the sample design as documented by NELS data collection personnel contracted by the Federal government to collect the data, one can only conclude that the standard errors, confidence intervals, and coefficients of interest of Hoxby's analysis are meaningless.

For these reasons Hoxby's use of NELS data produces results that are inappropriate to consider. And, because these problems would bedevil any analysis of California students of NELS, and any analysis of NELS that aims to focus on school-effects, analyses of the NELS dataset cannot speak to the issues raised in the *Williams v. California* litigation.

To address this same question Hoxby uses California STAR data to assess the "school management effect". Hoxby appears to use a fixed-effects model in this analysis. In this context a fixed effects model can essentially estimate an adjusted-mean for each school--I will henceforth refer to these estimates as "adjusted means". Hoxby graphs the distribution of adjusted means she obtains and asserts that these are "school management" effects. The first problem with this analysis is very simple--the label Hoxby proposes is merely an assertion. There is absolutely *no* evidence that the adjusted means are actually indicators of "school management." Indeed, it is quite possible that if we had a properly theorized indicator of school management we would find that many or even most schools with low-performing pupils were managed very well. Such a finding might suggest to us that schools are well-managed, just not managed well-enough to overcome the impact of limited resources. A direct indicator of school management, therefore, might lead us to conclude that the odds are so long, we may as well judge it *impossible* for school management to overcome the constraint imposed by, for example, having textbooks for only two-thirds of the students in a class--a situation that is quite problematic, for how should the teacher teach in such a situation: 1)use the textbooks for some but not all students, 2)don't use the textbooks for anyone, 3)develop a complicated rotation scheme? Hoxby offers no advice for teachers or schools in such a predicament but, instead, commits the error of asserting a finding that depends on the existence of a measure she does not have. This is inappropriate.

What Hoxby has found is that once many factors are controlled, there is still variation in schools' level of achievement. No one disputes this fact. Given Hoxby's model, any unmeasured factor that would affect all children at the school--over-crowding, pest infestations, unsanitary conditions or pollution that could lead to childhood sickness, insufficient numbers of textbooks, lack of pedagogical instruments, or even school management--that is not present in the model can account for the observed variation. To attribute the school-level variation to any *one* of the large and undetermined set of possible variables, as Hoxby does, is to go substantially beyond what the data will allow an analyst to say.

Hoxby also asks whether schools matter a great deal or not. Addressing this question seems necessary because Hoxby's analysis of school-level variation in achievement, especially the wide distribution of adjusted means in the figures on page 10, certainly suggests that schools matter. Hence, Hoxby seeks to calibrate the effect of schools, and to do so reports the percentage of explained variance that is explained by "family," "school," and "neighborhood," after analyzing NELS and National Longitudinal Survey of Youth (NLSY) data. Several methodological and theoretical comments are in order concerning these decompositions.

The NELS data is even more problematic for this analysis than for the analysis mentioned above. The same problems that make NELS inappropriate above apply here, and additional ones apply as well. First, although it is unfortunate that Hoxby did not provide the full model she estimates but, instead, presents only the decompositions of explained variance, upon reading her description of the variables used in the model (provided in Hoxby 2001) it appears she did not control for prior test score. Hence, the NELS decomposition is for a model that does not control for students' 10th grade achievement. This has two major implications for the analysis--1)the NELS data analysis is seriously compromised, and 2)the amount of explained variation assigned to different factors is likely seriously over-stated.

The NELS data analysis is compromised not only for the reasons that rendered the previous use of NELS data inappropriate, but also because students change schools between the eighth grade base-year (1988) and the twelfth grade second follow-up (1992). Had Hoxby controlled for the tenth grade test score, and had the NELS sample design been such that it would be appropriate for one to investigate this issue using NELS, it might be possible to ascertain how much of the gain in test scores from grade 10 to 12 could be assigned to families, neighborhoods, and schools, because there is a great deal of stability in school enrollments in those years. Indeed, variables available in the public use version of NELS would have allowed a researcher to identify which students changed schools between grades 10 and 12, further focusing the analysis on the school effect. Yet, by failing to control for tenth grade test score, it becomes impossible to defend the decomposition, because the effect of the child's family, a family which likely has been with the child for his or her *entire* life, is compared with the mis-measured effect of the school the child attended for perhaps two years or less, *without any kind of control to make these comparisons commensurate.* This is inappropriate. Had the tenth grade test score been included in the model, one could at least argue that prior factors had been wiped out, and only the last two years of family, neighborhood, and school are at issue. This would render the factors commensurate. Omitting the control for grade 10 test scores makes the comparison incommensurate.

Second, by failing to include previous achievement, the analysis overstates the role of any factor. Had Hoxby included prior test score, the explained variance in the model would increase, the coefficients for all other factors would likely decline, and the percentage of *explained* variance attributable to family factors would also decline. Hence, by omitting this key factor, Hoxby biases her report in a way that maximizes the amount that will appear to be explained by the family.

The bias is consequential. Policy levers cannot alter families as easily as they may alter schools. Hoxby (2001) notes that families matter a great deal, and that schools also matter. Indeed, a major basis for current federal law--the No Child Left Behind Act signed into law on January 8, 2002--is that schools affect test scores, and schools can be altered. Schools play a role in determining students' previous test score, as do families. Yet, by failing to control for previous test score in the model, allowing the effect of families to accrue for nearly two decades (ages 0 to 18), estimating (poorly) only a contemporaneous effect of school-level factors, and comparing the explained variance attributable to family and school factors directly without appropriate adjustment, Hoxby follows an unacceptable research strategy that differs markedly from the standards in sociology, economics, and the field of education.

Finally, Hoxby's reported decompositions are potentially *very* misleading. After seeing Hoxby's decompositions some readers may believe that 93.4 percent of the variation in test scores is explained by family variables. Yet, this is incorrect; Hoxby (correctly) reports that 93.4 percent of the *explained* variation in her model is attributable to family variables. But, as we do not know how much of the variation in test scores is actually explained by the model, we cannot really assess how important families and schools are. For example, assume Hoxby's model explains 55 percent of the variance in total; that would mean that families explain approximately 51.4 percent of the variance in test scores, while school and neighborhood factors explain 3.6 percent of the variance in test scores. Yet, such a model would also show that nearly *half* of the variance in test scores is not explained. This would allow room for unmeasured factors to kick-in, some of which might be at the level of the family, some of which might be at the level of the neighborhood, and some of which might be at the level of the school. By reporting the proportion of *explained variance explained* by each category, rather than following the more standard practice of relating the proportion of total variance attributable to each category, Hoxby conceals the explanatory power of the model and the amount of unexplained variance. By concealing the amount of unexplained variance, Hoxby mis-leads the reader as to the importance of the family. This is inappropriate.

In short, it is impossible to replicate Hoxby's analysis of NELS data because she uses restricted data and does not supply the variables used in her models. Yet, the impossibility of replicating Hoxby's NELS analyses is not significant for the questions Hoxby addressed, because it is inappropriate to analyze NELS for the questions Hoxby seeks to address. It is also impossible to replicate Hoxby's NLSY analyses.[3] Our confidence in the presented results is weakened, however, because Hoxby presents summary statistics that will over-state the important role of families, and fails to provide enough information to evaluate the models. Finally, Hoxby's interpretation of the adjusted school means, which she regards as the "school management" effect, are based on assertions, not actual measures of management. In short, Hoxby's evidence is not persuasive; it rests on assertion, obfuscation, and unrepresentative data.

The Rossell Report Perspective on the Resource-Outcome Relation

Rossell analyzes California STAR data from the 1999-2000 and 2000-2001 academic years, and presents an analysis of data from one school district in

---

[3]The NLSY data Hoxby provides are incomplete. The file nlsy.csv, which, according to Hoxby (end of p. 7 in her "Notes"), must contain the educational attainment, income, family, and school input variables, does not have all the variables. Of the 7 school input variables one is missing and another one is missing but can be computed. Of the 12 family variables listed on p. 8 of the "Notes" Hoxby provided, 6 are missing.

We proceeded to attempt to replicate her analysis as closely and expeditiously as possible, in order to demonstrate the mis-leading implications of providing incomplete information about model results. Thus, we downloaded the missing NLSY independent variables from the website that Hoxby mentions in the beginning of p. 8 of "Notes." We defined all negative values as missing. We discovered that the income variables for 1994 and 1996 are missing in Hoxby's file; thus, we used income at the age of 34 instead of income at 33 for the respondents born in 1961 and 1963. For the same reason we had to use educational attainment at the age of 34 for the respondents born in 1962 and 1964. Then we discovered that Hoxby's NLSY data is deficient in another way. She actually uses wage (wage is income divided by hours of work; it is misleadingly called 'incomes' in the title of the second figure on p. 13 of the report) as the dependent variable in her second regression. Wages reflect salary income; incomes could reflect investment, salary, or income from other sources.

Also, the hours of work variables are not in the dataset. We searched for those variables online and downloaded what it *appears* Hoxby used. The data file with those variables has two problems. Firstly, the data is not ready for use because several variables appear to be lumped together in one column. We could not figure out an effective procedure to separate them without additional information. Finally, most of the values of these variables (about 90%) are missing, so the variables are seriously compromised anyway.

In short, there were just too many impediments to the effort to replicate Hoxby's analyses.

Georgia, in order to assess the impact of school resources on student outcomes. The Georgia data contains a variable that is often unavailable--facility quality. And, the California STAR data is potentially most relevant for the concerns of the court, for the data pertain to California, the data can be regarded as reflecting a census of the institutions at issue in the case (i.e., perhaps better than a probability sample), the data allow measurement of at least some school inputs directly, and the data are relatively recent.

Unfortunately, a series of mis-steps weaken the ability of Rossell's analysis to address the question she poses. The first mis-step is to presume that one can generalize from the experience of one apparently atypical district as to whether school facilities matter. Rossell writes:

> I do not have data on the quality of school facilities in any California school district, but I do have data from a school district in Georgia that routinely surveys its facilities and gives them a quality rating. The state of Georgia does not do this. It is up to the individual districts as to whether they wish to do this. This particular school district is the first I have encountered in decades of research and consulting that has a systematic facilities rating program (Rossell 2003, p. 21).

Rossell essentially states that the district for which she has facilities data is atypical. It is easy to point to any of the myriad possible ways in which the district is unlike others with respect to facilities. For example, just having a program to monitor facilities may eventuate in a situation whereinwhich even the most sub-standard facilities in the district, relative to other schools in the district, would still be sufficient to support student learning. Or, having a program may reflect a zealousness on the part of the district officials charged with plant issues, a zealousness that may not be common. The problem with atypicality exists because we cannot observe every factor about the district. Hence, any of the possibly many unknown differences between this district and more typical ones may create conditions that make it impossible to make inferences beyond the particular district under study. This is the reason analysts prefer probability samples for statistical analysis.

Because the district is atypical--a fact Rossell notes in her report--it is appropriate to study the relationship between facilities in that district and school outcomes, but it is *inappropriate* to generalize from that study to other districts and schools. The unusual nature of this district makes it incorrect to regard relationships between inputs and outputs there as indicative of what the relationship is in typical districts. Thus, the analysis of the facilities data should be ignored, for the atypical nature of the setting indicates that there is no reason to suspect the analysis can shed light on the experience of students in California.

Turning to the California STAR data, the data is representative of California, and thus potentially very useful for the considerations of the court. However, one of Rossell's first mis-steps in analyzing the California data is to fail to properly respond to the presence of missing data. Missing data presents a major problem for statistical analysis of social science data. Of course, there are many types of missing data, and many ways missing data can cause problems. One may have missing data on the dependent variable, or one may have missing data on the independent variables, the covariates.

Missing data on covariates is relatively easy to address; California STAR data does have missing data on the covariates. Such a situation presents a problem for researchers because one must make some assumption about the missing data; using that assumption, the analyst makes research decisions on how to treat the data. Analysts have shown that some assumptions are better than others (e.g., Little 1983). A particularly damaging approach is to assume that the cases that have missing data on any independent variable contain *no* information of value to the analysis of the question under investigation. If a researcher makes this assumption, the researcher will delete cases that are missing on any variable. This is exactly the approach Rossell follows in her report; as Table 2 indicates, this led to the deletion of anywhere from 7.2 to 25.9 percent of the data that could be used in the primary analyses.[4] Deleting cases in this manner has several negative effects on the analysis.

---

[4]Rossell studies determinants of the mean achievement of all students, the mean achievement of minority students, and the mean achievement of English Language Learners. We define the primary analyses as analyses of all students' achievement in all five subjects, whereinwhich the independent variables are percentage of teachers with emergency permits and/or waivers, as well as the analyses of English Language Learners achievement with the key independent variable of percentage of teachers who are fully certified in ELL or bilingual education. We define these as the primary analyses because if effects of resources are revealed for these analyses, there is no reason to look further. Hence, these should form the first set of analyses to correct.

Table 2 -- Number and Percentage of Available Cases Deleted from Rossell's (2003) Primary Analyses

Variables			Rossell (2003) Method		
Outcome	Key Independent	Available Cases[a]	Used	Deleted	≈%
Reading	Emergency Permit	8079	7275	804	10.0
Mathematics	Emergency Permit	8077	7275	802	9.9
Language	Emergency Permit	8068	7271	797	9.9
Science	Emergency Permit	1669	1240	429	25.7
Social Studies	Emergency Permit	1672	1239	433	25.9
Reading	Waivers	8079	7280	799	9.9
Mathematics	Waivers	8077	7280	797	9.9
Language	Waivers	8068	7276	792	9.8
Science	Waivers	1669	1243	426	25.5
Social Studies	Waivers	1672	1242	430	25.7
Reading	Language Certification	5157	4769	388	7.5
Mathematics	Language Certification	5188	4813	375	7.2
Language	Language Certification	5170	4788	382	7.4

Table 2, Note a: "Available cases" are all cases with valid data on the outcome variable.

First, deleting cases is unwise because it makes the standard errors larger, all else equal. The standard errors are in part a function of sample size; by deleting cases one decreases the precision of the analysis, making it harder to discern effects. In other words, deleting cases biases the analysis toward a "no effects" conclusion. Second, by deleting a case just because it has missing data on one covariate, the researcher prevents the information about that case from being used in estimating other relationships for which the case *does* have data. It would be as if a detective decided to ignore *all* the statements of a witness to a shooting just because the witness did not see the face of the person who fired the gun. Ignoring other details the witness could provide would be unwise, for the witness might have information--the height of the shooter, the make of the gun, the direction the assailant fled, the characteristics of the getaway vehicle, and more--that might be *very* useful to the investigation. Just as a good detective will not throw away information under such conditions, a good social scientist will not delete cases that contain potentially useful information. Yet, in the analyses Rossell provides for the court she deletes all cases that are missing on any variable. This approach to missing data is unacceptable.

Analysts have developed several effective approaches for the treatment of missing data (e.g., Little and Rubin 1990). A particularly promising approach--mean substitution *with* control--allows one to proceed without invoking assumptions that might themselves become a matter of contestation in a legal proceeding, and thus is of great utility in the present context. This approach calls for one to code all the missing data on X_1 to the mean of the cases with valid data on X_1; at the same time, the researcher creates another variable called X_1^* which equals 0 if X_1 has valid data and 1 if X_1 has missing data (that has now been recoded to the mean). The value of this approach is that by using both X_1 and X_1^* in the regression model, one allows any cases that were missing on X_1 to still provide information useful for estimating the relationship between X_2 and the dependent variable. Yet, because one has included X_1^* in the model, this control will pick up any systematic deviation that is connected with cases that are missing on X_1, thus preventing the mean imputation from affecting the estimate of the X_1/dependent variable relationship. This approach allows one to use all cases with valid data on the dependent variable.

Correcting this simple over-sight begins to change the pattern of findings. Tables 3, 4, and 5 report the coefficients, standard errors, sample sizes, and R^2 (proportion of variance explained) for two sets of analyses: 1)Rossell's primary analyses and 2)a similar analysis that differs in that it retains all available cases by using mean substitution with imputation control.

The first observation to make concerning the re-analyses is that the percentage of explained variance is lower in all but one of the corrected analyses than in the analogous Rossell analysis, even though the corrected analyses have more independent variables (owing to the inclusion of controls for missing data). This suggests that the cases Rossell deleted do deviate systematically from those included in her analyses, because the variation in the deleted cases is not explained as well by the model as the subset of cases Rossell included in her analysis. Still, the amount of explained variance is very high in the corrected models.

The results for emergency permits change somewhat, but the most dramatic change concerns the findings as to the effect of waivers. In her primary analyses, Rossell found not a single instance in which waivers for teacher s matter. In contrast, by retaining cases that can be retained in the analysis, we find statistically significant effects of waivers in three central curricular domains--reading, mathematics, and language.

Table 3 -- Emergency Permits

Variable	Reading Rossell	Reading Corrected	Mathematics Rossell	Mathematics Corrected	Language Rossell	Language Corrected	Science Rossell	Science Corrected	Social Studies Rossell	Social Studies Corrected
%Eng Learner	0.007	-0.001	0.014	0.013	0.017	0.014	-0.006	-0.014	-0.000	-0.004
	0.003	0.005	0.004	0.005	0.004	0.005	0.010	0.012	0.009	0.012
%Biling00-01	-0.022	-0.020	-0.012	-0.009	-0.022	-0.021	-0.002	-0.026	0.014	0.004
	0.003	0.004	0.003	0.004	0.003	0.004	0.017	0.021	0.016	0.022
%free lunch	-0.010	-0.030	-0.017	-0.031	-0.013	-0.031	-0.015	-0.019	-0.009	-0.022
	0.003	0.003	0.003	0.004	0.003	0.004	0.007	0.007	0.006	0.007
%Min 00-01	-0.010	-0.038	-0.019	-0.041	-0.010	-0.035	-0.020	-0.042	-0.048	-0.071
	0.003	0.004	0.003	0.004	0.003	0.004	0.008	0.009	0.008	0.009
%ELL Spanish	-0.008	-0.022	-0.014	-0.027	-0.009	-0.022	-0.009	-0.015	-0.019	-0.027
	0.002	0.002	0.002	0.003	0.002	0.003	0.005	0.007	0.005	0.007
School Size	-0.000	0.000	0.000	0.001	0.000	0.001	0.001	0.001	0.001	0.001
	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Elementary	1.898	3.706	2.950	4.435	1.697	3.170	0.423	-6.621	-0.691	-0.251
	0.120	0.167	0.151	0.183	0.134	0.173	4.682	3.635	4.571	3.770
Read Ach 00	0.946	0.868								
	0.004	0.006								
Math Ach 00			0.905	0.847						
			0.005	0.006						
Lang Ach 00					0.938	0.873				
					0.005	0.006				
Scie Ach 00							0.918	0.857		
							0.014	0.016		
Soci Ach 00									0.890	0.825
									0.014	0.016
% Emergency	0.011	0.009	-0.008	-0.011	0.012	0.013	-0.003	-0.007	0.009	0.023
	0.005	0.006	0.006	0.007	0.005	0.006	0.013	0.012	0.012	0.013
Constant	4.035	9.814	7.875	12.456	4.815	9.982	4.998	9.069	8.582	13.086
	0.352	0.468	0.430	0.507	0.399	0.497	1.020	1.126	0.970	1.131
N	7275.00	8079.00	7275.00	8077.00	7271.00	8068.00	1240.00	1669.00	1239.00	1672.00
r2	0.96	0.92	0.94	0.90	0.95	0.91	0.91	0.85	0.91	0.84

legend: b/se

Note: Corrected Models contain controls for missing; see Appendix 3 for the Full Corrected models

Table 4 -- Waivers

Variable	Reading Rossell	Reading Corrected	Mathematics Rossell	Mathematics Corrected	Language Rossell	Language Corrected	Science Rossell	Science Corrected	Social Studies Rossell	Social Studies Corrected
%Eng Learner	0.007	-0.001	0.014	0.012	0.017	0.014	-0.004	-0.016	0.001	-0.007
	0.003	0.005	0.004	0.005	0.004	0.005	0.009	0.012	0.009	0.012
%Biling00-01	-0.022	-0.020	-0.012	-0.009	-0.022	-0.021	-0.003	-0.024	0.013	0.006
	0.003	0.004	0.003	0.004	0.003	0.004	0.017	0.021	0.016	0.022
%free lunch	-0.010	-0.030	-0.017	-0.031	-0.013	-0.031	-0.014	-0.018	-0.010	-0.021
	0.003	0.003	0.003	0.004	0.003	0.004	0.007	0.007	0.006	0.007
%Min 00-01	-0.008	-0.036	-0.020	-0.041	-0.008	-0.032	-0.020	-0.042	-0.048	-0.067
	0.003	0.004	0.003	0.004	0.003	0.004	0.008	0.008	0.007	0.009
%ELL Spanish	-0.008	-0.021	-0.015	-0.028	-0.009	-0.022	-0.009	-0.015	-0.019	-0.027
	0.002	0.002	0.002	0.003	0.002	0.003	0.005	0.007	0.005	0.007
School Size	-0.000	0.001	0.000	0.001	0.000	0.001	0.001	0.001	0.001	0.001
	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Elementary	1.855	3.658	2.962	4.452	1.637	3.108	0.446	-6.768	-0.795	-0.099
	0.121	0.167	0.152	0.183	0.134	0.173	4.700	3.628	4.582	3.766
Read Ach 00	0.946	0.869								
	0.004	0.006								
Math Ach 00			0.904	0.848						
			0.005	0.006						
Lang Ach 00					0.938	0.873				
					0.005	0.006				
Scie Ach 00							0.920	0.859		
							0.014	0.016		
Soci Ach 00									0.888	0.826
									0.014	0.016
% Waivers	-0.007	-0.064	-0.008	-0.053	-0.015	-0.050	-0.043	-0.060	0.019	-0.032
	0.020	0.023	0.025	0.025	0.023	0.024	0.055	0.042	0.053	0.044
Constant	4.013	9.776	7.926	12.421	4.867	9.957	4.881	8.981	8.696	13.041
	0.352	0.469	0.430	0.508	0.399	0.498	1.025	1.127	0.972	1.134
N	7280.00	8079.00	7280.00	8077.00	7276.00	8068.00	1243.00	1669.00	1242.00	1672.00
r2	0.96	0.92	0.94	0.90	0.95	0.91	0.91	0.85	0.91	0.84

legend: b/se

Note: Corrected Models contain controls for missing; see Appendix 3 for the Full Corrected models

Table 5 -- Language Certification

	Reading		Mathematics		Language	
Variable	Rossell	Corrected	Rossell	Corrected	Rossell	Corrected
%Eng Learner	0.025	0.017	0.007	0.029	0.036	0.033
	0.005	0.007	0.007	0.008	0.006	0.007
%Biling00-01	-0.041	-0.048	-0.071	-0.021	-0.044	-0.049
	0.004	0.005	0.005	0.006	0.005	0.006
%free lunch	-0.047	-0.068	-0.066	-0.088	-0.060	-0.082
	0.005	0.006	0.006	0.007	0.006	0.006
%Min 00-01	0.004	0.001	0.004	-0.000	0.009	0.005
	0.005	0.006	0.007	0.008	0.006	0.007
%ELL Spanish	-0.035	-0.066	-0.032	-0.126	-0.053	-0.087
	0.003	0.004	0.005	0.005	0.004	0.005
School Size	-0.001	-0.001	-0.003	-0.000	-0.001	-0.001
	0.000	0.000	0.000	0.000	0.000	0.000
Elementary	3.698	6.474	7.369	7.294	3.939	6.164
	0.233	0.271	0.283	0.317	0.268	0.293
Read Ach 00	0.756	0.651				
	0.010	0.013				
Math Ach 00			0.389	0.619		
			0.010	0.011		
Lang Ach 00					0.712	0.624
					0.011	0.012
% Cert Lang	-0.002	-0.001	-0.000	0.002	0.002	0.001
	0.003	0.004	0.004	0.005	0.004	0.004
Constant	10.393	14.844	14.288	25.630	14.594	19.808
	0.576	0.686	0.797	0.904	0.728	0.810
N	4769.00	5157.00	4798.00	5188.00	4788.00	5170.00
r2	0.78	0.70	0.65	0.72	0.74	0.68

legend: b/se

Note: Corrected Models contain controls for missing; see Appendix 3 for the Full Corrected models

Table 6 summarizes the comparison of the results Rossell obtained in her primary analyses to the results one obtains when one retains for analysis all schools that *can* be retained by mean substitution with control for imputation. This simple change doubles the number of statistically significant findings, from 2 to 4.

Table 6-- Comparison of Statistical Significance of School Resource Analyses of All Students (and the main ELL analysis of ELL students) when Useable Cases are Deleted (Rossell Listwise) or Retained (Mean ($\bar{x}$) Imputation w/ Control)

Variables		Deletion Method	
Outcome	Key Independent	Rossell Listwise	$\bar{x}$ Imputation w/ Control
Reading	Emergency Permit	Significant	Not Significant
Mathematics	Emergency Permit	Not Significant	Not Significant
Language	Emergency Permit	Significant	Significant
Science	Emergency Permit	Not Significant	Not Significant
Social Studies	Emergency Permit	Not Significant	Not Significant
Reading	Waivers	Not Significant	Significant
Mathematics	Waivers	Not Significant	Significant
Language	Waivers	Not Significant	Significant
Science	Waivers	Not Significant	Not Significant
Social Studies	Waivers	Not Significant	Not Significant
Reading	Language Certification	Not Significant	Not Significant
Mathematics	Language Certification	Not Significant	Not Significant
Language	Language Certification	Not Significant	Not Significant

Yet, additional adjustments are required. For one, Rossell's analyses have a curious feature. Each school input is studied separately, even when the inputs are arguably different variants of the same phenomenon. One example is provided by the treatment of the "Emergency Permit" and "Waiver" variables. Arguably, these two variables, which reflect the percentage of teachers in the school who are not certified teachers, should be studied together using any of the many appropriate tools for doing so. We have two bases for our claim that these two variables should be studied together. First, we contend that these two categories--those with an emergency permit, and those with a waiver--essentially serve the same function--they allow persons who are not formally prepared to enter the classroom into the classroom and allow them to teach. Hence, they should be studied together to tap the phenomenon of "non-teachers" in the classroom.

Second, our view that these variables should be studied together also seems to match the state's *own* understanding of the meaning of these two programs, and their and our understanding justifies combining these two variables into one analysis. Appendix 1, downloaded from the State of California Department of Education web-site on September 3, 2003 (http://www.cde.ca.gov/fiscal/categorical/title1/tchrshortage01.htm), is a document in which then Superintendent Delaine Eastin reports which subjects have been judged to be experiencing teacher shortages in 2001. Eastin notes that the criteria for identifying a teacher shortage uses "written objective standards developed for the California Assumption Program of Loans of Education (APLE). The shortage definition is the sum of the FTE [full-time-equivalent] teachers on emergency permits or waivers and the estimated FTE new hires by subject area divided by teacher FTE by subject area." It is clear from Eastin's letter that the State regards emergency permits and waivers as essentially the same, as both reflect a shortfall in the resources available to teach students.

We hasten to add that it is fine to estimate models that investigate the effect of emergency permits and waivers separately. But a thorough investigation would also combine these factors into one analysis. There are at least two ways to do so. A researcher might include both the emergency permit variable and the waiver variable in the same model. This approach would allow the researcher to test whether these two variables have discernibly different effects, and to test, statistically, whether the joint effect of both is discernibly different from zero (i.e., statistically significant). Moreover, this approach will allow the researcher to pull out the confounding effect of one variable while assessing the impact of the other variable on test scores.

Alternatively, a researcher may construct a new variable that is the sum of these two variables. The advantage of this approach is parsimony. The disadvantage is that it does not allow one to test whether the impact of each factor is the same in magnitude and direction.

Hence, each approach has advantages and disadvantages. However, failing to do either of these simple tasks, or others that would assess the effect of these factors simultaneously, biases the analysis toward a "no effects" conclusion. The bias arises because when the analyst investigates the effect of emergency permits without considering the impact of waivers, the analyst essentially is asserting that waivers are irrelevant. Such an analysis ends up treating two schools with the same percentage of emergency teachers, but one with a low percentage of waivers and one with a high percentage of waivers, as if they have equal school resources. If one were to plot those schools' mean test scores against their proportion of teachers with emergency permits, one school might have high test scores, while the other has low test scores. But, because both schools have the same percentage of teachers with emergency permits, the plot will place them at the same location on the x-axis. This might lead an analyst to conclude that putting emergency permit personnel into classrooms does not matter, because the plotted data will show that schools with a certain percentage of emergency permit teachers vary greatly in school mean achievement. This plot will be summarized by a slope for the regression line that will tend to be flat. Consequently, by analyzing each of these very similar bases for placement in teaching positions separately, a researcher will bias their analysis toward a "no effects" conclusion.

As we consider the problem of how to treat the key resource variables, we are also led to consider one of the control variables Rossell uses. School size is included in each of the models. It makes sense to control for school size so that the model results for other variables reflect comparisons between schools of equal size. Unfortunately, the size variable is included in an uncommon and difficult-to-defend form--it is included as if each additional student adds or subtracts a constant increment to the school's mean test score.

This specification assumes, therefore, that effects of scale do not decline--it assumes that adding one student to a school with 150 students already there has the same effect as adding one student to a school that already has 1500 students. This is a very unlikely occurrence. To reflect how adding persons to units such as schools, businesses, factories, hospitals, and other organizations really affect outcomes, analysts in economics, sociology, business administration, education, and other fields usually use some function of organization size, such as the natural log of size or the square root of size, as an independent variable when they want to account for the size of the institution.

Table 7 presents the results of estimating Rossell's models, using mean imputation with control, using both indicators of the presence of non-teachers, and controlling for the natural log of school size. With these corrections we now find statistically significant effects of non-teachers in three central subjects of the curriculum--reading, mathematics, and language--as reflected in the coefficients for percent of teachers with Emergency Permits and percent of teachers with Waivers.

Table 7 -- Non-teacher effects on School Mean Achievement, Mean Imputation w/ Control, and Defensible Specification of Functional Form for School Size

Variable	Reading	Math	Language	Science	Soc Stud
%Eng Learner	-0.005	0.007	0.009	-0.016	-0.006
	0.005	0.005	0.005	0.012	0.012
%Biling00-01	-0.022	-0.012	-0.023	-0.028	0.001
	0.004	0.004	0.004	0.021	0.022
%free lunch	-0.032	-0.034	-0.034	-0.017	-0.021
	0.003	0.004	0.004	0.007	0.007
%Min 00-01	-0.042	-0.045	-0.039	-0.044	-0.072
	0.004	0.004	0.004	0.009	0.009
%ELL Spanish	-0.021	-0.027	-0.022	-0.015	-0.027
	0.002	0.003	0.003	0.006	0.007
lnsize	1.048	1.531	1.440	0.970	1.051
	0.080	0.090	0.088	0.145	0.150
Elementary	3.882	4.612	3.272	-6.770	-0.516
	0.161	0.172	0.164	3.611	3.747
Read Ach 00	0.854				
	0.006				
Math Ach 00		0.827			
		0.006			
Lang Ach 00			0.852		
			0.006		
Scie Ach 00				0.838	
				0.016	
Soci Ach 00					0.807
					0.016
% Emergency	0.007	-0.015	0.011	-0.011	0.018
	0.006	0.007	0.006	0.012	0.013
% Waivers	-0.071	-0.052	-0.061	-0.067	-0.052
	0.023	0.025	0.024	0.043	0.044
Constant	4.427	4.930	3.077	4.965	8.472
	0.635	0.679	0.661	1.186	1.215
N	8079.00	8077.00	8068.00	1669.00	1672.00
r2	0.93	0.91	0.91	0.85	0.84

legend: b/se

Note: Models contain controls for missing; see Appendix 3 for the Full models

Thus, making three very slight changes--making it possible to retain all possible cases in the analysis, using a defensible specification for school size, and analyzing indicators of teacher preparation simultaneously, overturns Rossell's finding in her analyses of all students. Instead of Rossell's finding of eight examples of non-significant effects, and no effect of waivers on school mean achievement, we find important effects of school resources in arguably the 3 central domains of the 5 areas studied. One might wonder how we could so dramatically change the finding using Rossell's own data. It is important to note that none of the changes we have made are controversial; they reflect, instead, the state's own understanding of the emergency and waiver programs, widely-accepted cross-disciplinary perspectives on how to analyze the effects of size, and a simple minimal-assumption solution to the well-known problem that simply deleting cases that can be salvaged is wrong. In short, we submit that standard analytic techniques and understandings, based on decades of social science research, and drawn from a variety of disciplines, turns out to produce results diametrically opposed to those Rossell has presented.

Concluding Remarks Concerning the Defendants' Experts' View of the Resources-Outcome Relation

Above, we have re-analyzed the data assuming that the questions Hoxby, Rossell, and Hanushek consider are relevant for the case. We have shown that by correcting methodological weaknesses and errors in their research the results of the analysis change. Hanushek's claim that there is little evidence that school resources matter is shown to be incorrect. Hoxby's assertions as to the school management effect are shown to be without foundation. Rossell's findings as to the effect of school resources in California schools is shown to be false. Hence, the claims of the defendants' experts have been undermined.

Do School Resources Matter in a Non-Linear Way?

Despite the changed nature of the findings given an appropriate analytic strategy, we submit that many of the analyses defendants' experts present do not really speak to the claims plaintiffs have raised. Plaintiffs have argued that the state need assure that schools meet some minimum standard of operation. If *minimum* resources are a main issue in litigation, one would want to estimate statistical models that reflect that theoretical concern. The defendants' experts have presented a series of models that ask, essentially, what is the *linear* relationship between resources and outcomes. Although a corrected analysis of their own data reveals statistically significant linear relationships between school resources and achievement, the plaintiffs are focusing on a different question; plaintiffs are concerned that the state should be responsible for assuring that no school fall below a minimum level of resources. The implication of the plaintiffs' claim is that below that minimum students fare very poorly. In other words, plaintiffs claims are consistent with both linear and *nonlinear* effects of school resources. Nonlinear effects of resources are not allowed by the approaches Hoxby, Rossell, and Hanushek follow. And, by testing for only linear effects, they reduce the chance that threshold effects will be found; threshold effects are a type of effect of great interest to this case. To more fully investigate the issues plaintiffs raise, therefore, one would also want to estimate models to test this claim--do students fare poorly below a certain threshold?

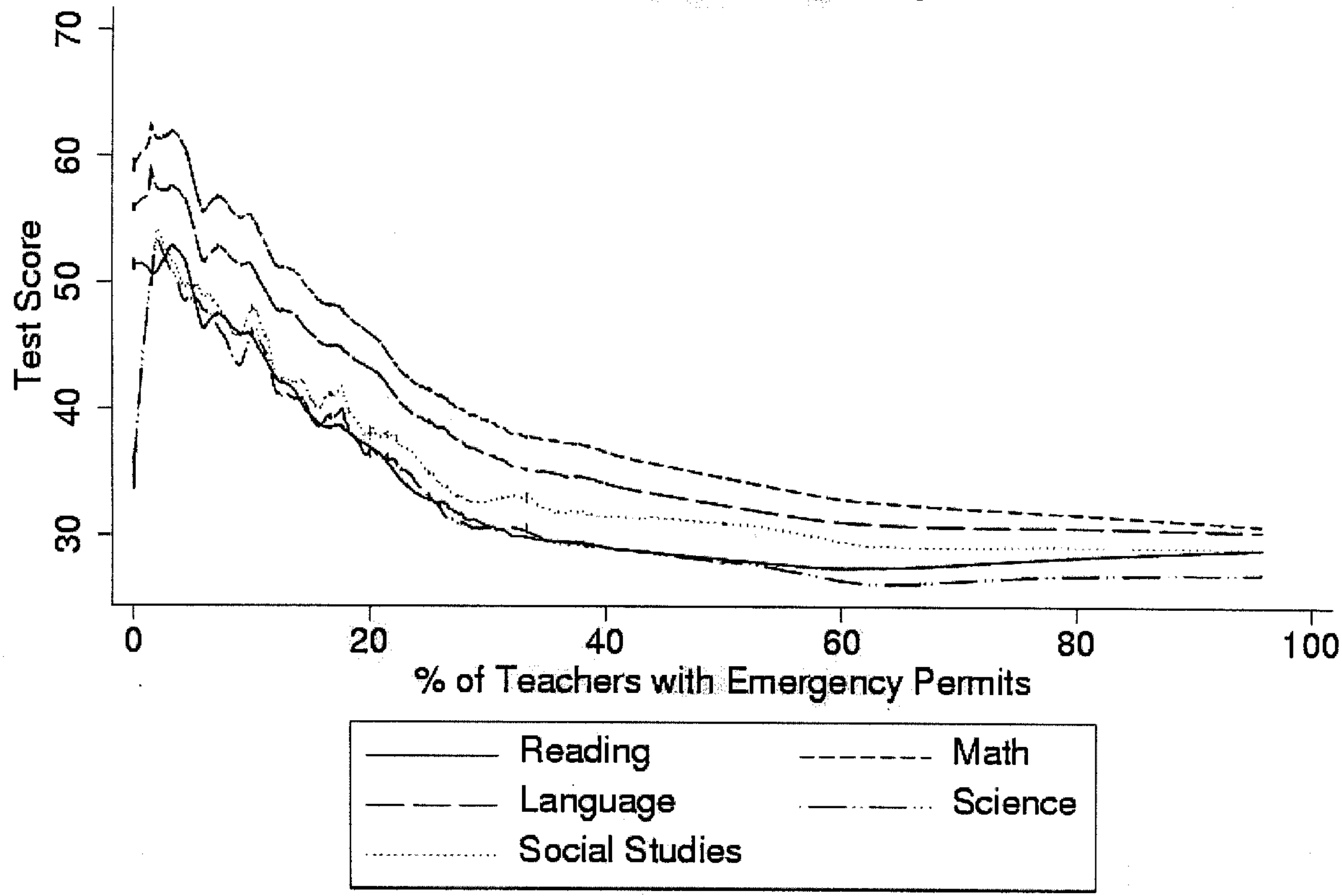
One may easily estimate models to explore these issues. Indeed, much of the science for doing so is not new. We proceed first by using lowess smoothing (Cleveland 1979) to plot the relationship between the dependent variable (mean SAT9 scores for a subject for the school) and the independent variable of interest (different types of school resources). Afterwards, we use the inflection points in the lowess smoothing--the places in the graphs where we discern a change in the slope--to construct variables that reflect the turning points in the resource-outcome relation, and use those variables in regression models. This allows an investigation of the direct relationship between resources and outcomes.

Lowess regression estimates a regression model for each case using cases that fall within a window, a window that defines the term "nearby". It weights the closest cases more than the more distant cases within the window. Thus, it runs through the entire list of cases, from one end of the independent variable to the other, and as it moves through the full list of cases, it moves the window. This allows the plotted line to reflect local structure, and therein lies the name--LOcally WEighted Scatterplot Smoothing. The size of the window is a key parameter for the estimation. Analysts are encouraged to investigate different window sizes. Appendix 2 contains plots of the investigation of two different window sizes; as results did not differ appreciably, as the sample sizes tend to be large, and as a key question concerns the extremes, the aim was to allow local flex in the plot if the pattern seemed robust against the window size.

We begin with the plot provided in Figure 1, which graphs the simple lowess regressions reflecting the relationship between achievement and percentage of teachers with emergency permits. We used Rossell's school-level data from California (described in Appendices 2, 3 and 4 of Rossell's report), focusing on the primary analyses, to investigate the prospect of nonlinear effects. At the outset, we should note that lowess methods do not force effects to be nonlinear; instead, they allow a nonlinear effect to emerge, an effect we then test in multiple regression models.

Figure 1 reveals that the relationship between emergency permit incidence and school mean test scores is non-linear, regardless of the subject under consideration. The inflection point is between 25 and 35. Below that point, the relationship between incidence of emergency teachers and reading test score appears strongly negative; above that point the relationship appears small to non-existent. This implies that a school with 25 to 35 percent of its teachers serving as emergency personnel is little different from a school with 70 percent of its teachers serving as emergency personnel. This suggests that, conditional on the size of the teaching staff, each increment in emergency personnel added makes a negative difference, up to a point, afterwhich no further damage seems likely. Of course, the relationship reflected in the simple regression, revealed in the plot, only indicates that the relationship is likely not linear, and suggests a reasonable place to hypothesize for the break

Figure 1—Test Score by Emergency Permit Incidence



in the piecewise linear model. The multiple regression models (in Table 8) show the net relationship between the piecewise indicators of emergency permits and school mean achievement.

The results in Table 8 suggest that for reading, mathematics, and language, after controlling for several additional factors, the effect of emergency credentials is not zero but, instead, *negative,* once the percentage of teachers in the school with emergency credentials exceeds 30 percent. Note that in Rossell's data, 450 schools--4.75 percent of the total--exceeded this threshold.

Table 8 -- Piecewise Linear Regression of Achievement on Emergency Permits

Variable	Reading	Math	Language	Science	Soc Stud
% Eng Learner	-0.006	0.007	0.009	-0.014	-0.003
	0.004	0.005	0.005	0.011	0.012
%Biling 00-01	-0.023	-0.012	-0.024	-0.029	-0.000
	0.004	0.004	0.004	0.021	0.022
%free lunch	-0.032	-0.034	-0.034	-0.019	-0.022
	0.003	0.004	0.004	0.007	0.007
%Min 00-01	-0.044	-0.046	-0.040	-0.045	-0.073
	0.004	0.004	0.004	0.009	0.009
%ELL Spanish	-0.022	-0.027	-0.023	-0.015	-0.027
	0.002	0.003	0.003	0.007	0.007
lnsize	0.982	1.497	1.381	0.927	1.009
	0.082	0.091	0.089	0.150	0.155
Elementary	3.988	4.660	3.358	-6.591	-0.370
	0.160	0.172	0.163	3.621	3.759
Read Ach 00	0.855				
	0.006				
Math Ach 00		0.827			
		0.006			
Lang Ach 00			0.852		
			0.006		
Scie Ach 00				0.837	
				0.016	
Soci Ach 00					0.807
					0.016
%Emerg 0-30	0.034	-0.005	0.033	-0.006	0.023
	0.008	0.009	0.009	0.020	0.020
%Emerg 30-100	-0.072	-0.048	-0.056	-0.028	0.002
	0.016	0.017	0.017	0.031	0.032
Constant	4.691	5.068	3.349	5.208	8.720
	0.637	0.682	0.664	1.195	1.224
N	8079.00	8077.00	8068.00	1669.00	1672.00
r2	0.93	0.91	0.91	0.85	0.84

legend: b/se

Note: Models contain controls for missing; see Appendix 3 for the Full models

With respect to analytic strategy, this result suggests that testing for linear effects alone is to likely mis-specify the relationship. The relationship appears to be consistent with plaintiffs' concern, that sharply negative effects kick-in as the percentage of emergency teachers increases beyond a certain point. This is the kind of possibility that a linear analysis assumes away.

Figure 2 provides the lowess plot of the relationship between waivers and school mean test scores. Again, we find evidence of non-linear effects. For waivers we find an inflection point around 35 percent, above which it appears that a sharply negative relationship pertains. For some subjects the relationship below that inflection point appears flat or curvilinear, for others it appears negative but less so. The multiple regression results, presented in Table 9, confirm the simple regression results.

Figure 2--Test Score by Waiver Incidence

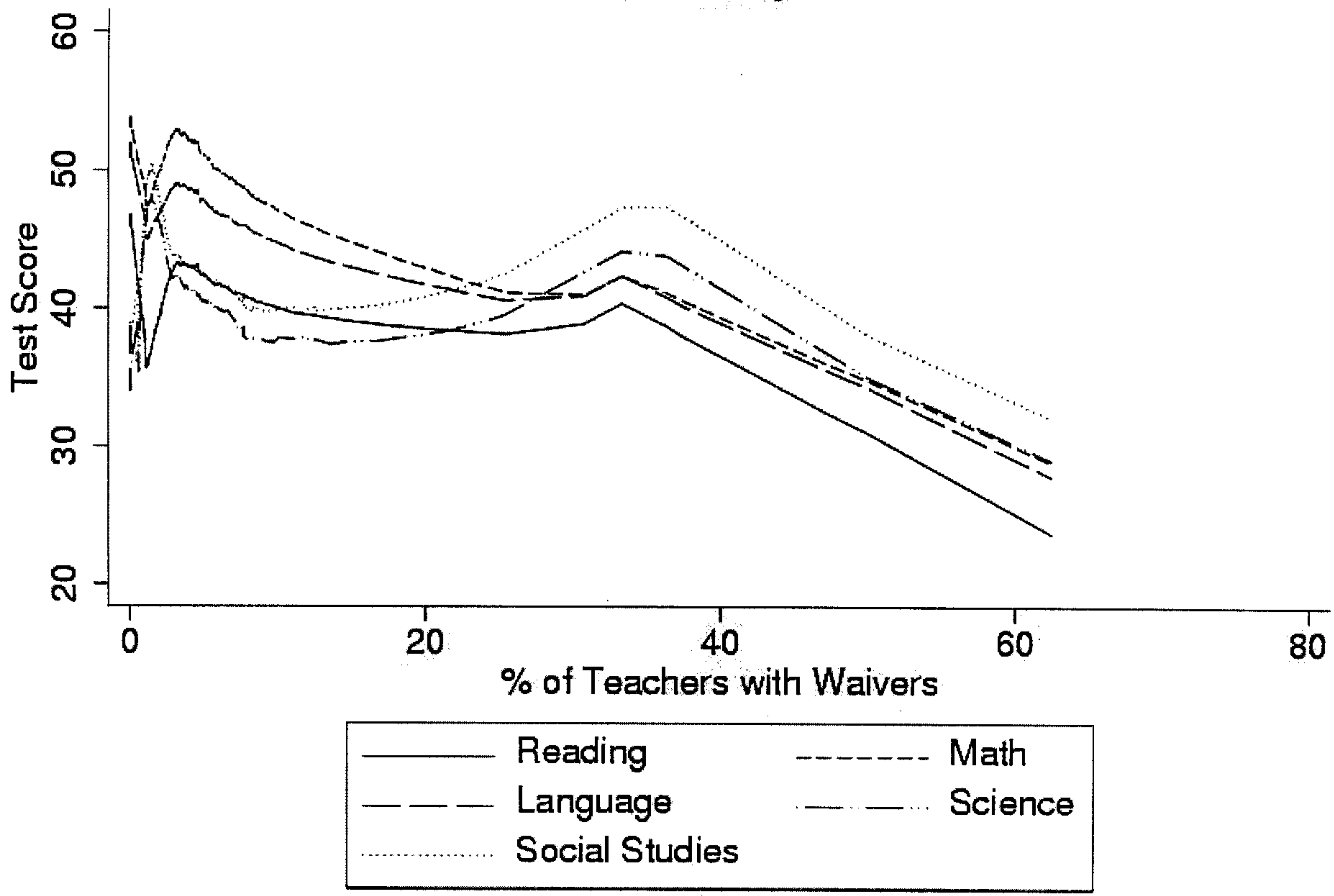


Table 9 shows no effect of waivers below 35 percent, and sharply negative effects above 35 percent. Although many fewer schools exceeded this threshold in Rossell's dataset (11 schools), 6 of those 11 also exceeded the threshold for the emergency permit variable as well.

Table 9 -- Piecewise Linear Regression of Achievement on Waivers

Variable	Reading	Math	Language	Science	Soc Stud
%Eng Learner	-0.006	0.006	0.009	-0.015	-0.006
	0.004	0.005	0.005	0.011	0.012
%Biling00-01	-0.022	-0.012	-0.024	-0.028	0.001
	0.004	0.004	0.004	0.021	0.022
%free lunch	-0.031	-0.033	-0.034	-0.017	-0.021
	0.003	0.004	0.004	0.007	0.007
%Min 00-01	-0.041	-0.047	-0.037	-0.046	-0.069
	0.004	0.004	0.004	0.008	0.008
%ELL Spanish	-0.021	-0.028	-0.022	-0.015	-0.027
	0.002	0.003	0.003	0.006	0.007
lnsize	1.077	1.518	1.457	0.941	1.059
	0.080	0.089	0.087	0.143	0.148
Elementary	3.933	4.695	3.284	-6.894	-0.380
	0.159	0.171	0.162	3.609	3.746
Read Ach 00	0.854				
	0.006				
Math Ach 00		0.828			
		0.006			
Lang Ach 00			0.852		
			0.006		
Scie Ach 00				0.839	
				0.016	
Soci Ach 00					0.806
					0.016
%Waiver 0-30	-0.000	-0.005	-0.002	-0.060	-0.030
	0.025	0.027	0.027	0.050	0.052
%Waive 30-100	-0.955	-0.765	-0.766	-0.169	-0.165
	0.151	0.165	0.160	0.214	0.222
Constant	4.168	4.876	2.911	5.060	8.474
	0.632	0.676	0.657	1.185	1.214
N	8079.00	8077.00	8068.00	1669.00	1672.00
r2	0.93	0.91	0.91	0.85	0.84

legend: b/se

Note: Models contain controls for missing; see Appendix 3 for the Full models

Table 10 contains an analysis that uses both variables. The results are unchanged from the results presented in Tables 8 and 9. We find statistically significant, negative, non-linear effects of emergency permits and waivers in reading, mathematics, and language. The pattern of effects remains consistent with the concern plaintiffs have raised.

Table 10 -- Piecewise Linear Models of Achievement and Incidence of Emergency Permits and Waivers

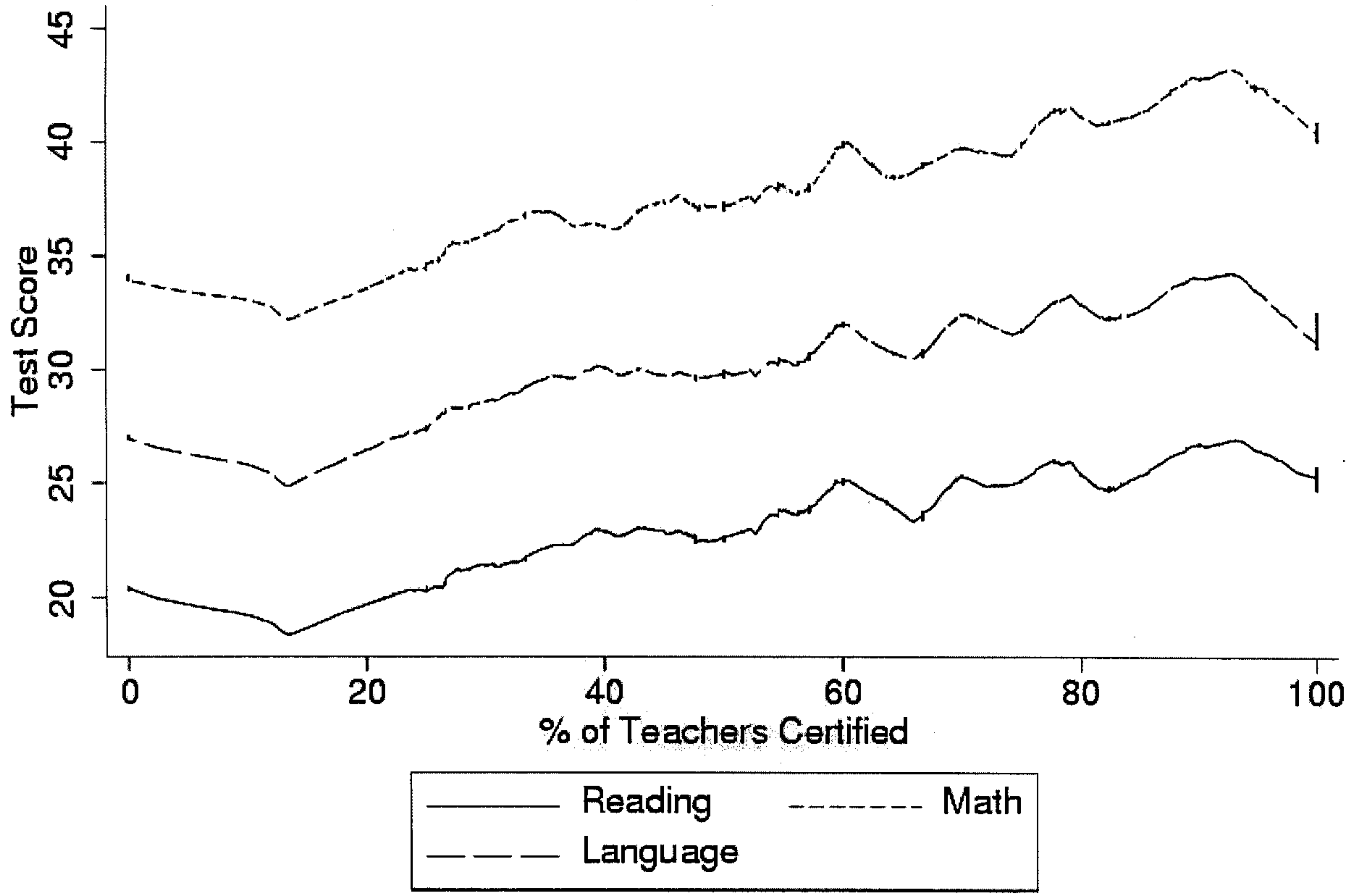
Variable	Reading	Math	Language	Science	Soc Stud
%Eng Learner	-0.005	0.007	0.009	-0.016	-0.006
	0.004	0.005	0.005	0.012	0.012
%Biling00-01	-0.023	-0.012	-0.024	-0.027	0.002
	0.004	0.004	0.004	0.021	0.022
%free lunch	-0.032	-0.034	-0.034	-0.017	-0.021
	0.003	0.004	0.004	0.007	0.007
%Min 00-01	-0.043	-0.045	-0.040	-0.045	-0.072
	0.004	0.004	0.004	0.009	0.009
%ELL Spanish	-0.022	-0.027	-0.023	-0.015	-0.028
	0.002	0.003	0.003	0.007	0.007
lnsize	0.964	1.491	1.371	0.949	1.031
	0.081	0.091	0.089	0.150	0.155
Elementary	3.956	4.651	3.339	-6.687	-0.436
	0.160	0.172	0.164	3.616	3.753
Read Ach 00	0.856				
	0.006				
Math Ach 00		0.828			
		0.006			
Lang Ach 00			0.853		
			0.006		
Scie Ach 00				0.838	
				0.016	
Soci Ach 00					0.807
					0.016
%Emerg 0-30	0.034	-0.005	0.033	-0.004	0.025
	0.008	0.009	0.009	0.020	0.020
%Emerg 30-100	-0.065	-0.042	-0.050	-0.025	0.005
	0.016	0.017	0.017	0.031	0.032
%Waiver 0-30	-0.010	-0.001	-0.012	-0.057	-0.039
	0.025	0.028	0.027	0.050	0.052
%Waive 30-100	-0.916	-0.744	-0.732	-0.153	-0.164
	0.151	0.166	0.160	0.216	0.224
Constant	4.725	5.042	3.348	5.056	8.565
	0.636	0.681	0.663	1.196	1.226
N	8079.00	8077.00	8068.00	1669.00	1672.00
r2	0.93	0.91	0.91	0.85	0.84

legend: b/se

Note: Models contain controls for missing; see Appendix 3 for the Full models

The above has analyzed the role of waivers and emergency permits. Figure 3 contains the lowess plot of the impact of certification for teachers of English Language Learners.

Figure 3--Test Score by Incidence of Certified Teachers



The plot in Figure 3 suggests that the effect of Certified English Language or Bi-Lingual Teacher incidence is relatively linear, as no clear breakpoint is visible. Hence, for this domain, it seems clear from this assessment that more certified language teachers are associated with higher achievement for English Language Learners, but we have not discerned a clear threshold effect in the simple regression model.

The final analytic change we introduce is to take seriously the joint determination of school achievement. Rossell analyzed each outcome separately. Yet, schools simultaneously seek high achievement in each domain. Hence, there is good reason to model the determination of achievement in a system of regression equations, estimated jointly. One may use the technique of Seemingly Unrelated Regressions (SUR) to estimate a system of regression equations.

The advantage of this approach is that standard errors should be smaller in the joint model than in the separately estimated models. This occurs because the set of independent variables differs across the models. Rossell's use of separate models biased results toward a no effects conclusion. We re-estimate our piecewise linear regressions using SUR models.

The bottom of Table 11 contains a test of whether the regressions are unrelated. The unrelatedness of the regressions is rejected. The bottom also contains a table showing a strong correlation between the residuals in the three different equations; this means that many of the omitted factors that seem to matter for one outcome also matter for the other outcomes. This should not surprise; it should be expected that many of the unmeasured factors that matter for reading achievement in a school would probably also matter for language and mathematics achievement.

Table 11 -- Seemingly Unrelated Regression Model of Reading, Math, and Language Achievement and the Effect of Non-Teachers

	Coef.	Std. Err.	z	P>|z|	[95% Conf.	Interval]
Reading 2001						
%Eng Learner	-.0133789	.0045048	-2.97	0.003	-.0222081	-.0045496
%Biling00-01	-.024363	.0038159	-6.38	0.000	-.0318421	-.0168839
%free lunch	-.0495346	.0032711	-15.14	0.000	-.0559459	-.0431233
%Min 00-01	-.0620135	.0035803	-17.32	0.000	-.0690307	-.0549963
%ELL Spanish	-.0308496	.0023893	-12.91	0.000	-.0355325	-.0261666
lnsize	1.086037	.0812604	13.36	0.000	.9267698	1.245305
Elementary	5.024264	.1532769	32.78	0.000	4.723847	5.324681
Read Ach 00	.7716286	.0045509	169.55	0.000	.7627089	.7805482
%Emerg 0-30	.0308288	.0083168	3.71	0.000	.0145283	.0471293
%Emerg 30-100	-.0438404	.0160544	-2.73	0.006	-.0753064	-.0123744
%Waiver 0-30	-.0041005	.0251841	-0.16	0.871	-.0534605	.0452594
%Waive 30-100	-.8041901	.1508356	-5.33	0.000	-1.099822	-.5085578
Constant	9.951938	.6133128	16.23	0.000	8.749867	11.15401
Math 2001						
%Eng Learner	.0030697	.0049391	0.62	0.534	-.0066107	.0127502
%Biling00-01	-.0127631	.0042041	-3.04	0.002	-.0210029	-.0045233
%free lunch	-.049756	.0035894	-13.86	0.000	-.0567912	-.0427209
%Min 00-01	-.0581874	.0038812	-14.99	0.000	-.0657944	-.0505805
%ELL Spanish	-.0361699	.0026451	-13.67	0.000	-.0413542	-.0309855
lnsize	1.680221	.0908882	18.49	0.000	1.502084	1.858359
Elementary	5.545551	.1663925	33.33	0.000	5.219427	5.871674
Math Ach 00	.7547447	.0047316	159.51	0.000	.745471	.7640184
%Emerg 0-30	-.0097493	.009171	-1.06	0.288	-.0277242	.0082257
%Emerg 30-100	-.0251083	.0176844	-1.42	0.156	-.0597691	.0095524
%Waiver 0-30	-.0018773	.0277451	-0.07	0.946	-.0562567	.0525021
%Waive 30-100	-.6337387	.1662431	-3.81	0.000	-.9595692	-.3079083
Constant	9.465524	.6644533	14.25	0.000	8.16322	10.76783
Language 2001						
%Eng Learner	.0020511	.0047979	0.43	0.669	-.0073526	.0114549
%Biling00-01	-.0271195	.004077	-6.65	0.000	-.0351103	-.0191286
%free lunch	-.0554477	.0035021	-15.83	0.000	-.0623118	-.0485837
%Min 00-01	-.0549106	.0037627	-14.59	0.000	-.0622854	-.0475358
%ELL Spanish	-.0331924	.0025566	-12.98	0.000	-.0382032	-.0281817
lnsize	1.611791	.0880972	18.30	0.000	1.439123	1.784458
Elementary	4.284545	.1589804	26.95	0.000	3.972949	4.596141
Lang Ach 00	.7636748	.0046935	162.71	0.000	.7544757	.7728738
%Emerg 0-30	.0299637	.0088797	3.37	0.001	.0125599	.0473675
%Emerg 30-100	-.0457681	.0171409	-2.67	0.008	-.0793636	-.0121726
%Waiver 0-30	-.0036354	.0268883	-0.14	0.892	-.0563356	.0490648
%Waive 30-100	-.5985008	.1610334	-3.72	0.000	-.9141205	-.2828811
Constant	8.558699	.6450286	13.27	0.000	7.294466	9.822932

Note: Model contains controls for missing; see Appendix 3 for the Full model

Correlation matrix of residuals:

	Reading	Math	Language
Reading	1.0000		
Math	0.7744	1.0000	
Language	0.8396	0.7820	1.0000

Breusch-Pagan test of independence: chi2(3) = 15426.155, Pr = 0.0000

It should also not surprise that when this specification is used, we find that the higher the incidence of non-teachers--emergency permit personnel and waiver personnel--the lower the school mean achievement. The effects are non-linear, and consistent with precipitously declining prospects for students in schools who have a critical mass of non-teaching personnel.

School and Society: Beyond The Analysis of Achievement

Schools provide resources for the economy, the polity, and the society. The multiple spheres for which schools matter mean that schools have multiple goals. The existence of multiple goals suggests analysts need consider a broad set of outcomes and relations when they seek to evaluate the role of school resources.

One clear aim of schools is to facilitate cognitive achievement among children. For some, another aim of schools is to increase future earnings among adults. Professors Hoxby and Rossell and Dr. Hanushek attempted to address the role of resources in the accomplishment of these goals.

But schools have additional aims as well. Schools also socialize students in particular ways. Schools may instill meritocratic and democratic values; schools may teach tolerance for racial and cultural difference; schools may nurture self-efficacy. Although each of these goals (and more) can in principle be turned into a variable, such that one may study the determinants of "commitment to democracy" or "tolerance," the defendants' experts have offered no evidence bearing on these other roles of schools and potential roles of school resources. To reject the importance of school resources one would need to investigate these other outcomes as well.

The complexities of the question of whether resources matter mean analysts who seek to address the issue must go beyond the analysis of test scores, educational attainment, and earnings. Although these are important outcomes of schools, they are by no means the only important outcomes. Any analysis that stops at these three outcomes remains woefully incomplete in its assessment of the role and impact of the distribution of school resources on the outcomes of schools. Hence, relying on such analyses alone will not aid the development of policy.

Concluding Remarks: Statistical Analysis Findings and Public Policy

The fore-going has responded to many of the technical limitations of the work submitted by Professors Caroline M. Hoxby, Christine Rossell, and Dr. Eric Hanushek. Once errors in their work are corrected, the evidence suggests that school resources matter.

The meta-analysis Hanushek presented, as well as the ad hoc approach to time series analysis he used, have been discredited by researchers in the past. We simply related those critiques and the state of knowledge in the relevant areas.

It proved impossible to replicate Hoxby's analyses, because she did not provide data sufficient to do so. Yet, we were able to evaluate other aspects of the work Hoxby submitted. Hoxby used non-representative datasets, made assertions concerning variables she did not have, and conveyed the amount of variation explained in a mis-leading manner--without providing sufficient information for anyone to see just how much variation was actually explained in the models. Taken together, this indicates that much of the work can be ignored without trying to replicate it--there is little value to replicating, correcting, and extending analyses on non-representative samples or concerning variables whose character is merely asserted rather than demonstrated. Further, findings from our replication of Rossell's analyses, that reveal a nonlinear relationship between measured inputs and outcomes, raised questions about Hoxby's analysis of California data--another analysis we could not replicate owing to the impossibility of merging the datafiles Hoxby supplied.

Rossell's analysis ignored missing data, used an implausible form for the school size variable, and analyzed each measured input separately. Yet the State of California Department of Education uses both measured inputs simultaneously in its assessment of teacher shortages; we regarded this as additional warrant for treating both factors together in the models. Correcting these weaknesses was sufficient to reverse Rossell's finding.

We went further, to explore nonlinear effects, a pattern of effects more consistent with plaintiffs' concerns. When we investigated nonlinear effects, we found important effects of the incidence of emergency permits and waivers. For each variable, above a certain threshold the effects of resources were sharply negative.

Throughout, as evidenced in our treatment of missing data and in our exploration of nonlinear effects, we took pains to make minimal assumptions, and to control for any effects of those assumptions as well.

Finally, we estimated a joint model of reading, mathematics, and language achievement, using Seemingly Unrelated Regressions. This model takes account of the simultaneous nature of school achievement and resources. The findings echoed those of our earlier analyses--school resources matter non-linearly, with evidence of sharply lower outcomes in schools with large percentages of non-teachers.

The sum total of the corrected and extended analyses of defendants' experts' own data is that school resources matter. Informative as it may be to see evidence suggesting that public policy may alter schools and thus make a difference, at least one final point need be raised.

As we have demonstrated, the social science research indicates that school resources matter for achievement. Yet, even were social science research to find no effect of school resources on achievement, educational attainment, and earnings, such research would not come close to suggesting there is no value to establishing and maintaining a baseline of resources below which no child should fall. The multiplicity of goals of schools renders any analysis of the effect of school resources on these three outcomes alone an incomplete investigation of the role of school resources.

Professors Hoxby and Rossell, and Dr. Hanushek, have presented a set of analyses that purport to address whether school resources matter. Yet, methodological mis-steps made their analyses miss the mark. Further, the questions they addressed were not consistent with plaintiffs' claims. Moreover, defendants' experts addressed only a subset of the complex questions one would need to answer to address whether resources matter.

Correcting their methodological mis-steps reversed their findings. Using defendants' expert's own data and appropriate analytic methods, we find powerful evidence that school inputs matter. Once we conduct an analysis more consistent with plaintiffs' claims, we find important nonlinear effects of school resources--consistent with plaintiffs' proposition that below some level school resource effects are heightened. Finally, we noted the incomplete nature of the defendants' experts' analysis of a complex issue; in such a complex issue, analyzing three school outcomes is woefully inadequate. Taken together, these corrections and elaborations culminate in a clear conclusion: school resources matter, they matter non-linearly, and thus inequality in those resources is consequential for school outcomes.

References

Campbell, Donald T., and Julian C. Stanley. 1963. *Experimental and Quasi-Experimental Designs for Research.* Chicago: Rand McNally Publishing Company.

Cleveland, William S. 1979. "Robust Locally Weight Regression and Smoothing Scatterplots." *Journal of the American Statistical Association* 74: 829-836.

Hanushek, Eric . 2003. "The Structure of Analysis and Argument in Plaintiff Expert Reports for *Williams v. California*." Report submitted as Defendants' Expert in *Williams v. California*.

Hoxby, Caroline M. 2001. "If Families Matter Most, Where Do Schools Come In?," pp. 89-125 in *A Primer on America's Schools,* edited by Terry M. Moe. Palo Alto, CA: Hoover Institution Press.

Hoxby, Caroline M. 2003. "Achievement, Efficiency, and Centralization in California Public Schools." Report submitted as Defendants' Expert in *Williams v. California*.

Ingels, Steven J., Leslie A. Scott, John R. Taylor, Jeffrey Owings, and Peggy Quinn. 1998. "National Education Longitudinal Study of 1988 (NELS:88) Base Year Through Second Follow-up: Final Methodology Report." United States Department of Education Working Paper 98-06.

Kalton, Graham. 1983. *Introduction to Survey Sampling.* Beverly Hills: Sage Publications.

Krueger, Alan B., and Diane M. Whitmore. 2002. "Would Smaller Classes Help Close the Black-White Achievement Gap?" pp. 11-46 in *Bridging the Achievement Gap,* edited by John E. Chubb and Tom Loveless. Washington, DC: Brookings.

Little, Roderick J. A. 1983. "Models for Nonresponse in Sample Surveys." *Journal of the American Statistical Association* 77: 237-250.

Little, Roderick J. A., and Donald B. Rubin. 1990. "The Analysis of Social Science Data with Missing Values." *Sociological Methods and Research* 18: 292-326.

Rossell, Christine H. 2003. "Equity and Efficiency in California Schools." Report submitted as Defendants' Expert in *Williams v. California.*

Snyder, Thomas D., Charlene M. Hoffman, Claire M. Geddes. 1999. *Digest of*

Education Statistics 1998. Washington, DC: U.S. Department of Education, Office of Educational Research and Improvement.

StataCorp. 2003. *Stata Statistical Software: Release 8.0.* College Station, TX: Stata Corporation.

Appendix 1 -- downloaded from the State of California Department of Education web-site on September 3, 2003
http://www.cde.ca.gov/fiscal/categorical/title1/tchrshortage01.htm

DELAINE EASTIN

CALIFORNIA DEPARTMENT OF EDUCATION

September 6, 2001

Secretary of Education
U.S. Department of Education
Washington, D.C. 20202

Dear Sir:

SUBJECT: Designated Teacher Shortage Areas for California

The designated teacher shortage areas for California in 2001-2002 are as follows:

Special Education
Physical and Life Sciences
Foreign Language
Mathematics/Computer Science

These areas were identified using written objective standards developed for the California Assumption Program of Loans of Education (APLE). The shortage definition is the sum of the FTE teachers on emergency permits or waivers and the estimated FTE new hires by subject area divided by teacher FTE by subject area. The attached table lists the subject areas in descending order of their shortage percentages based on total teacher FTEs in California.

The information is submitted in accordance with federal regulation 34 CFR 682.21(q)(6)(iv). Data for academic year 1999-2000 were used, as more currently data was not yet available. This listing of designated teacher shortage areas will be provided to principals of all California schools.

If you have questions regarding this information, please contact:

Jeanne Ludwig, Educational Consultant
Intersegmental Relations Unit
Professional Development & Curriculum Support Division
California Department of Education
830 S Street
Sacramento, CA 95814

Her phone number is 916-323-5190 and her fax number is 916-323-2817; e-mail address is jludwig@cde.ca.gov.

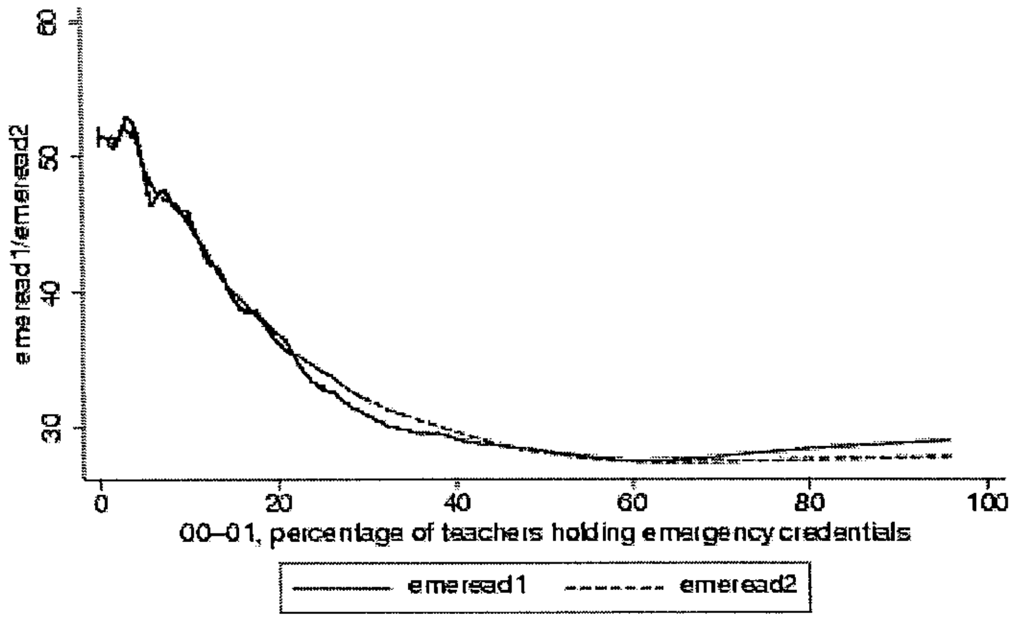
Sincerely,

DELAINE EASTIN
California Superintendent of Public Instruction

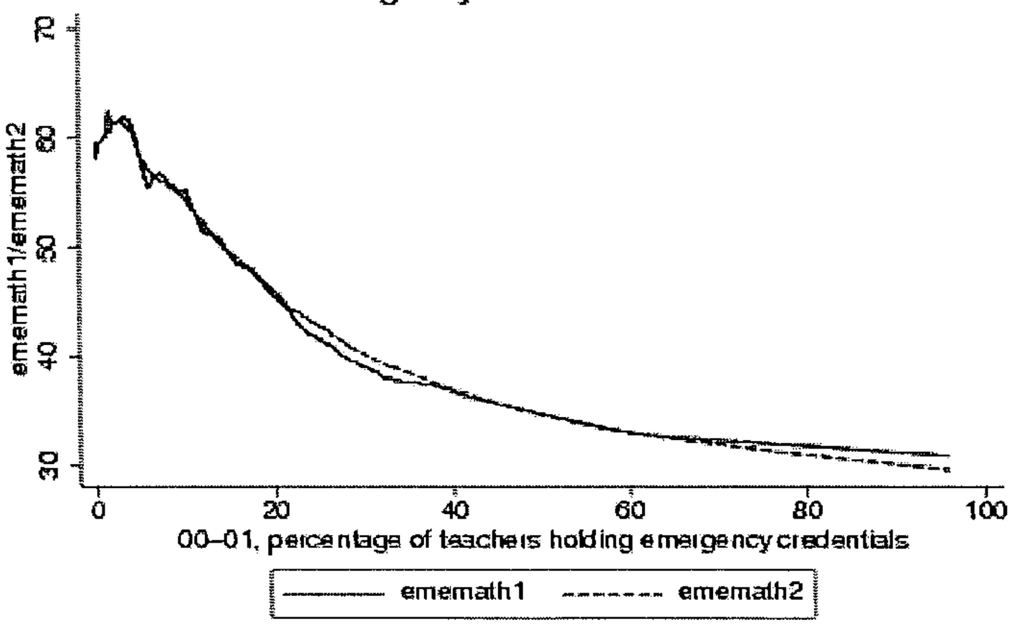
CDE VALID

Appendix 2 – Lowess Plots to Investigate Sensitivity to Bandwidth

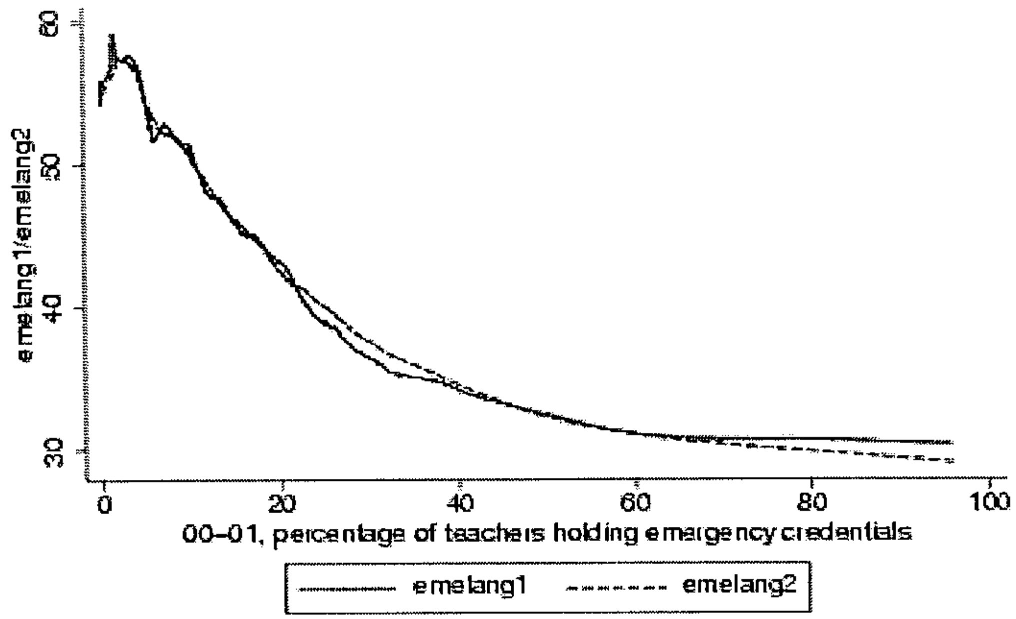
Appendix 2 Figure 1—Bandwidth Evaluation, Emergency Permits and Reading



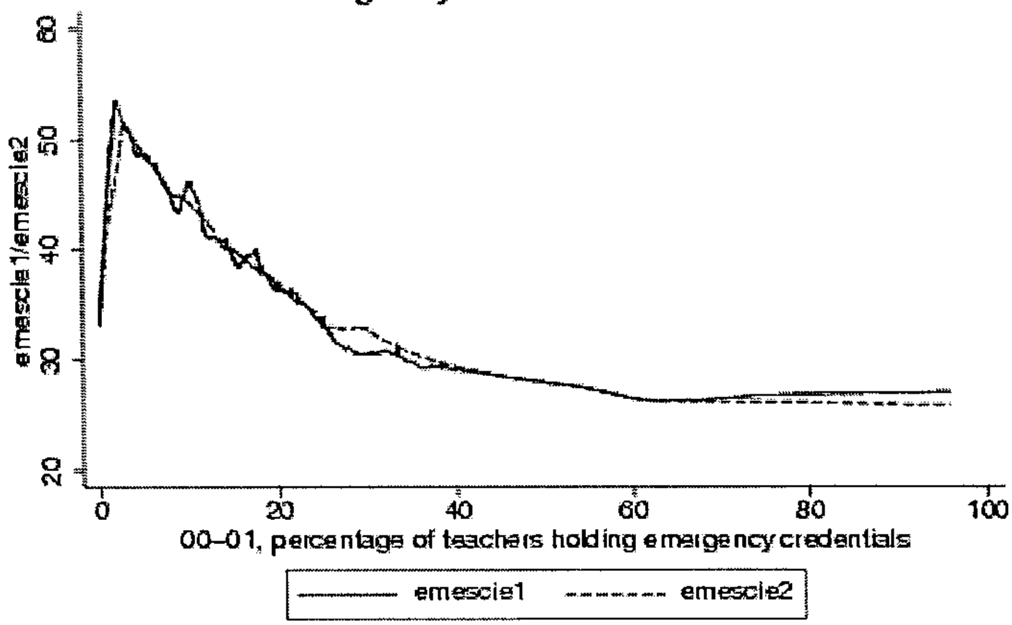
Appendix 2 Figure 2—Bandwidth Evaluation, Emergency Permits and Math



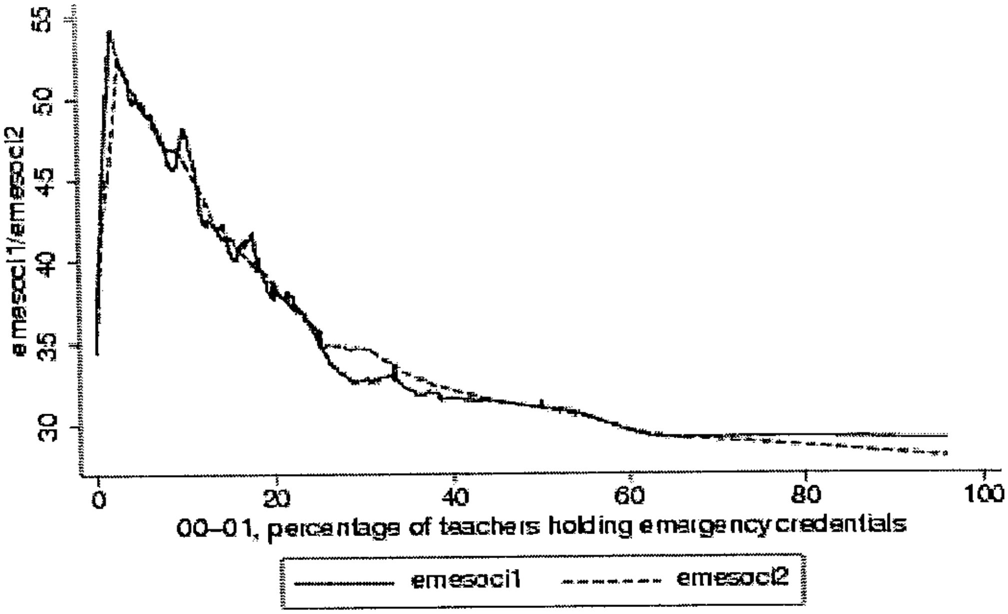
Appendix 2 Figure 3—Bandwidth Evaluation, Emergency Permits and Language



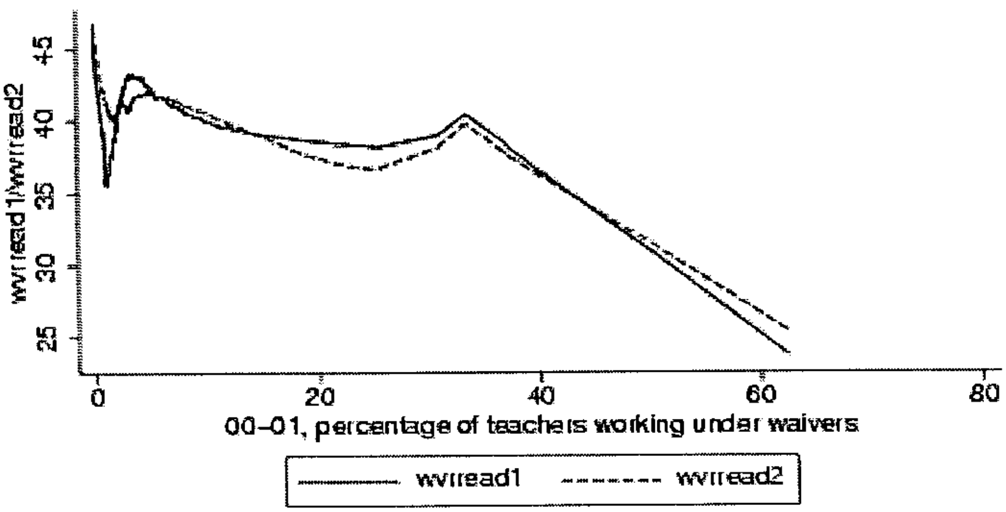
Appendix 2 Figure 4—Bandwidth Evaluation, Emergency Permits and Science

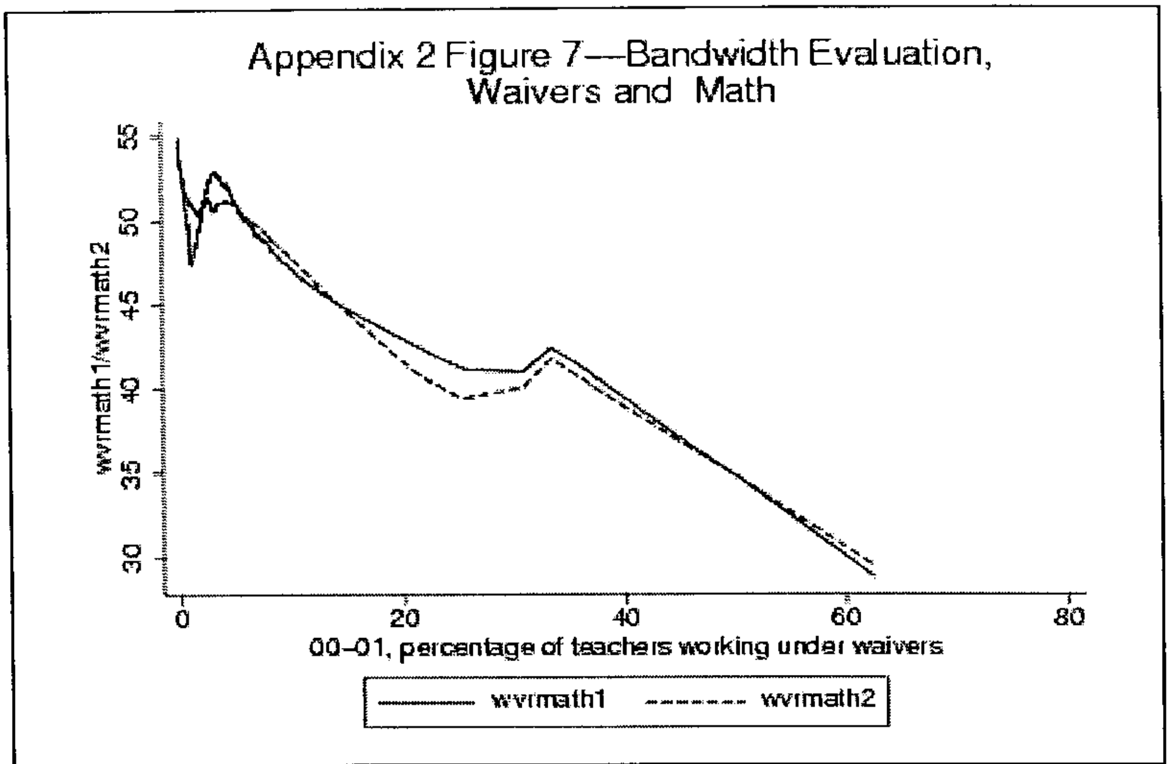


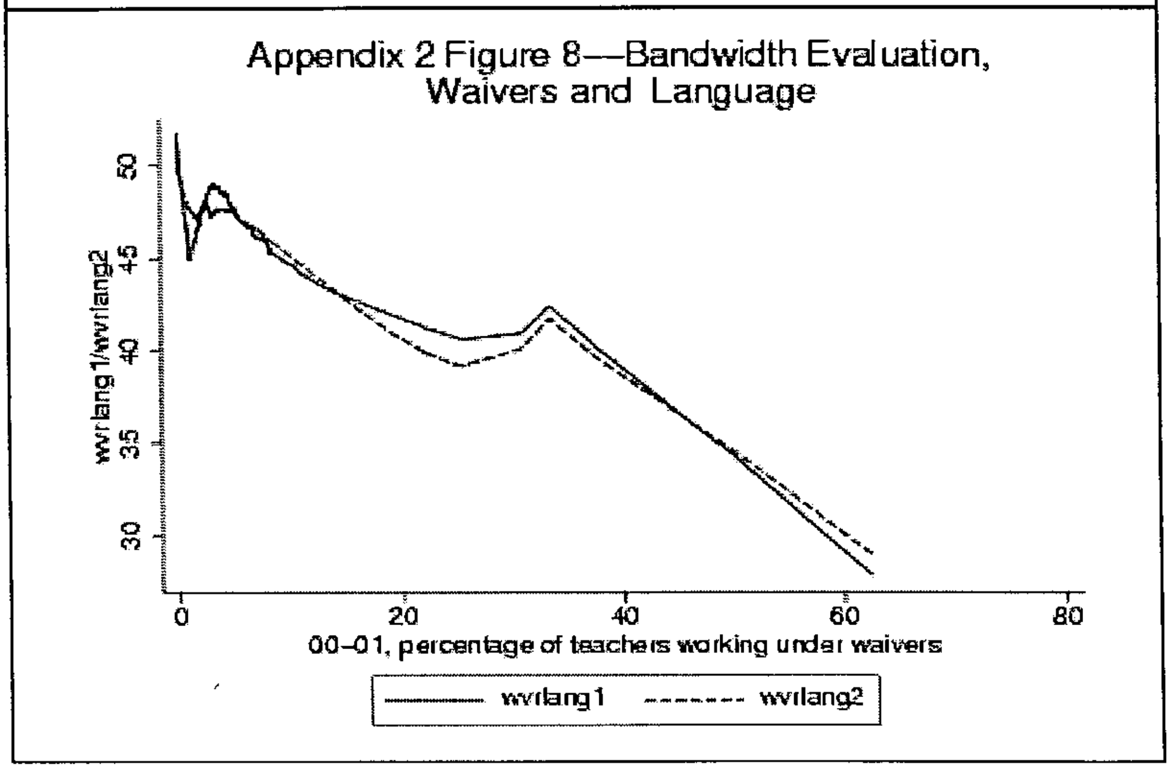
Appendix 2 Figure 5—Bandwidth Evaluation, Emergency Permits and Social Studies



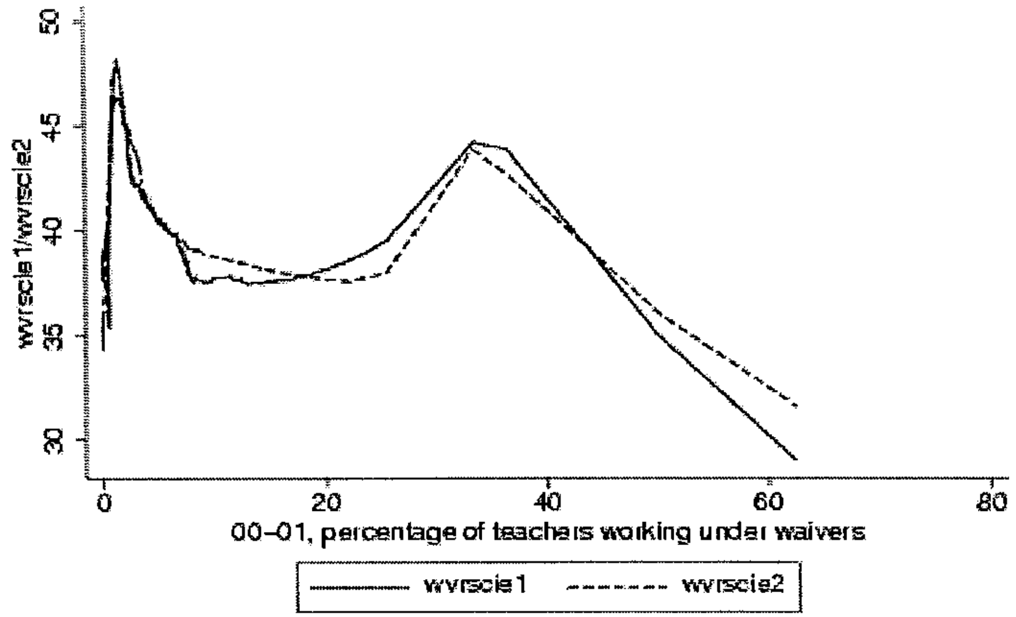
Appendix 2 Figure 6—"Bandwidth Evaluation," Waivers and Reading



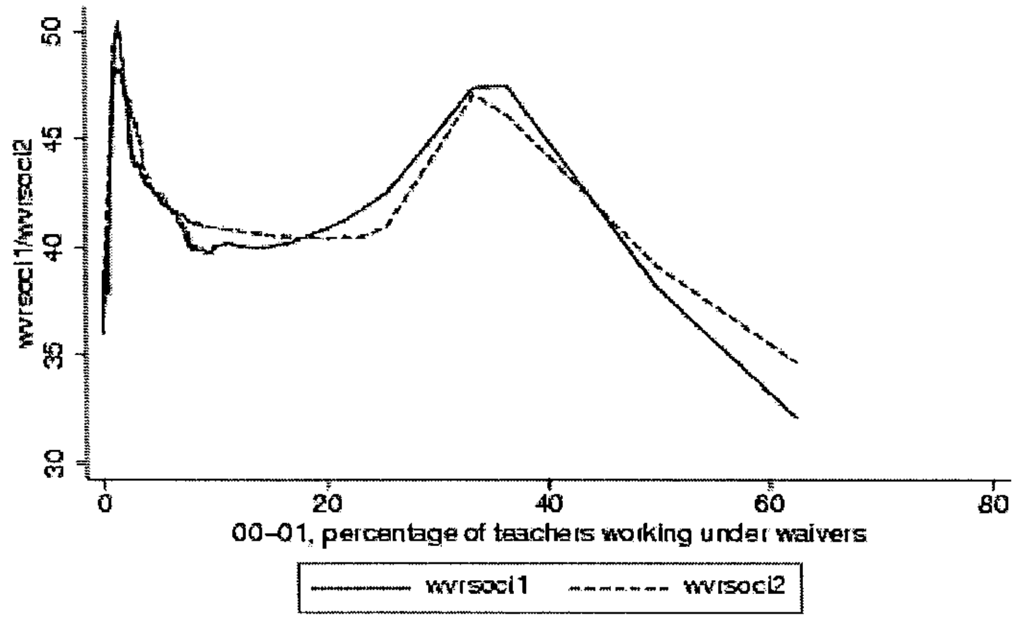

Appendix 2 Figure 7—Bandwidth Evaluation, Waivers and Math
wvrmath1/wvrmath2
30
35
40
45
50
55
0
20
40
60
80
00–01, percentage of teachers working under waivers
wvrmath1
wvrmath2


Appendix 2 Figure 8—Bandwidth Evaluation, Waivers and Language
wvrlang1/wvrlang2
30
35
40
45
50
0
20
40
60
80
00–01, percentage of teachers working under waivers
wvrlang1
wvrlang2

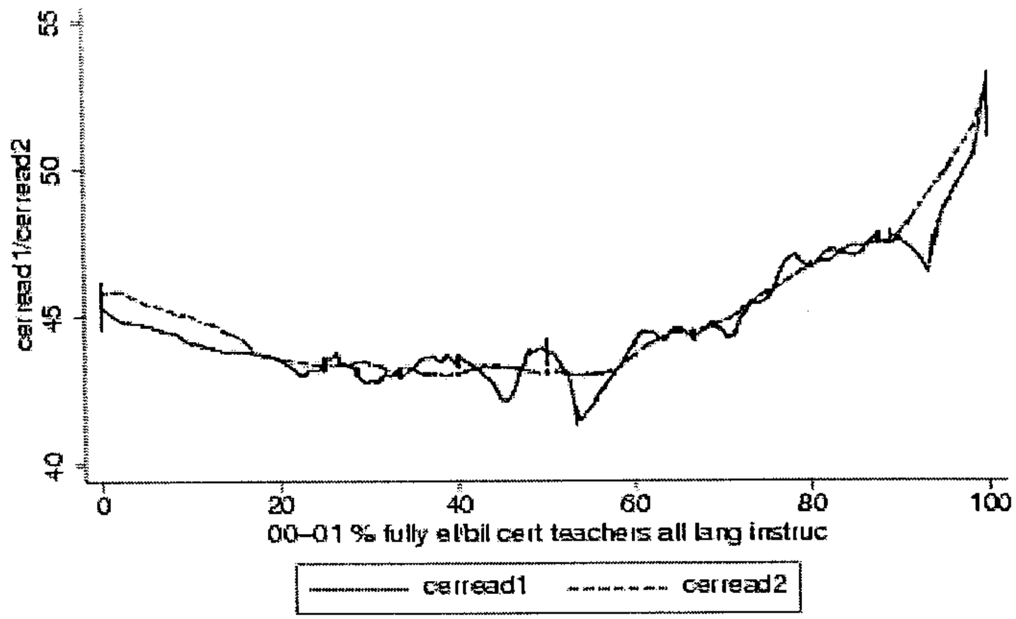
Appendix 2 Figure 9—Bandwidth Evaluation, Waivers and Science



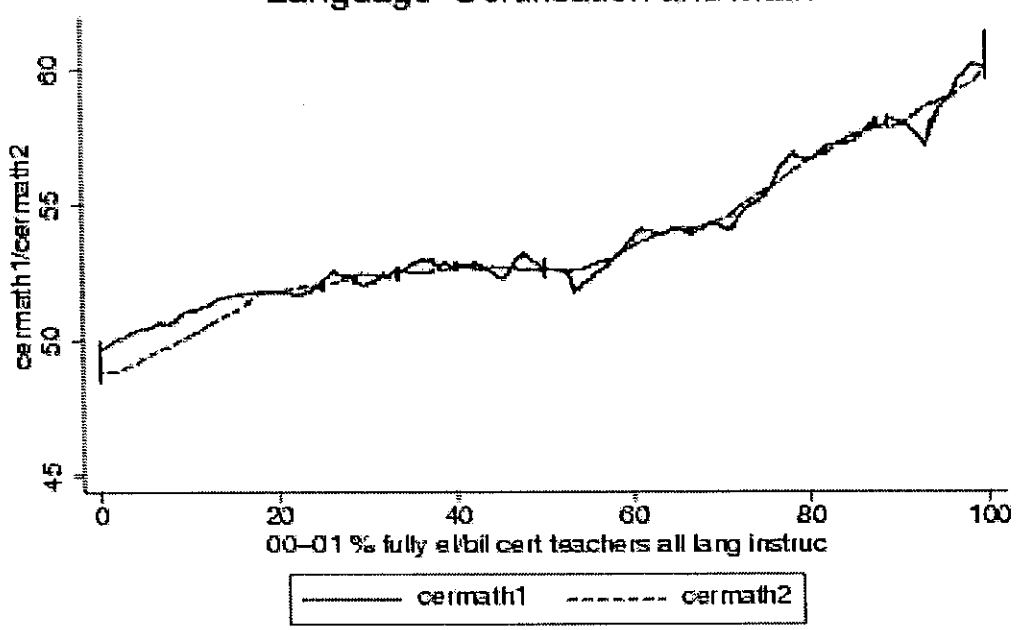
Appendix 2 Figure 10—Bandwidth Evaluation, Waivers and Social Studies



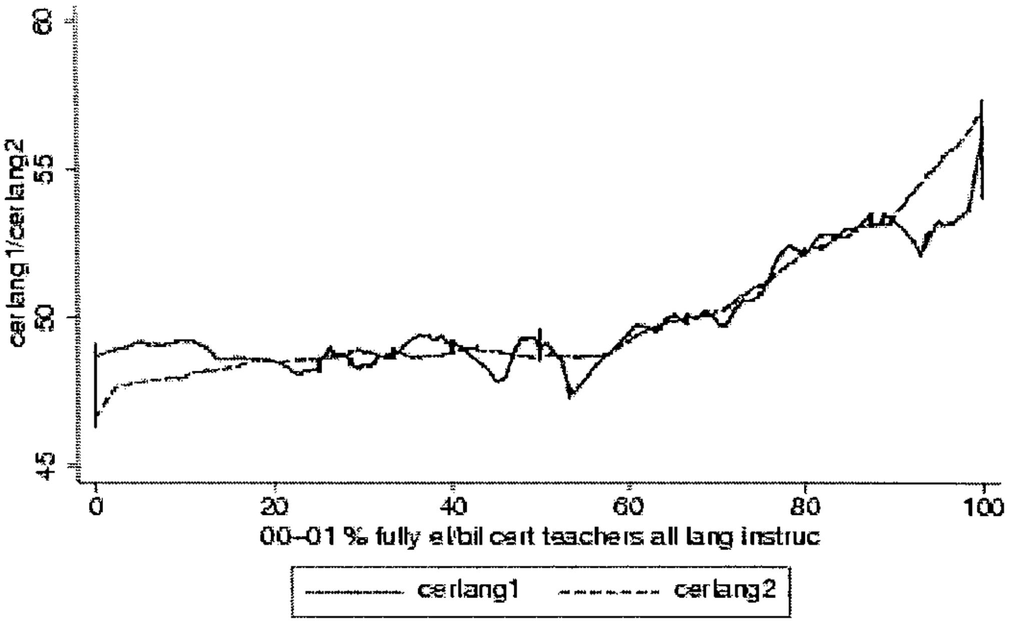
Appendix 2 Figure 11—Bandwidth Evaluation, Language Certification and Reading



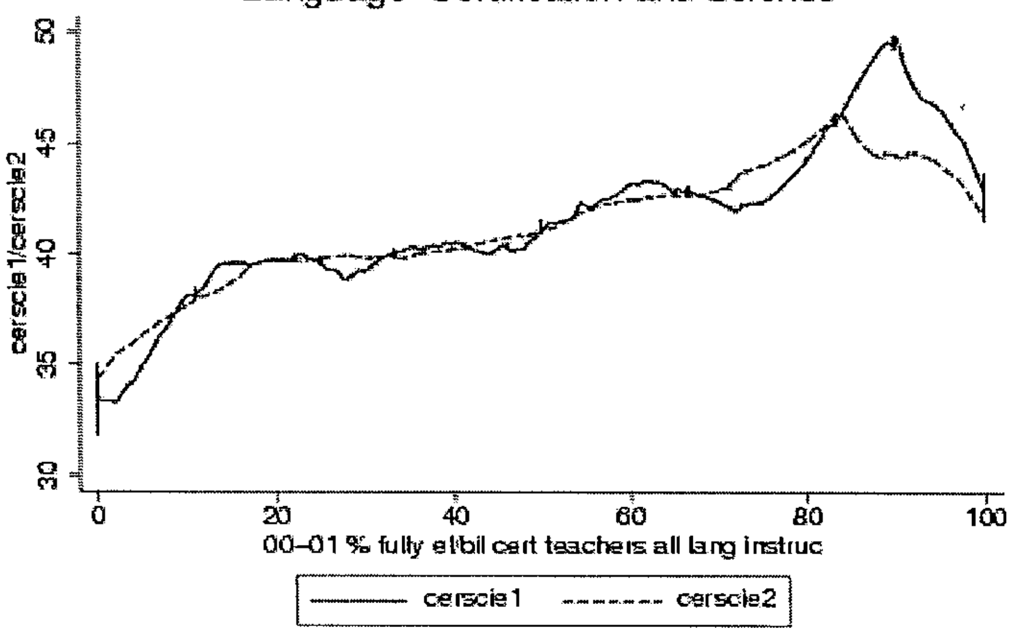
Appendix 2 Figure 12—Bandwidth Evaluation, Language Certification and Math

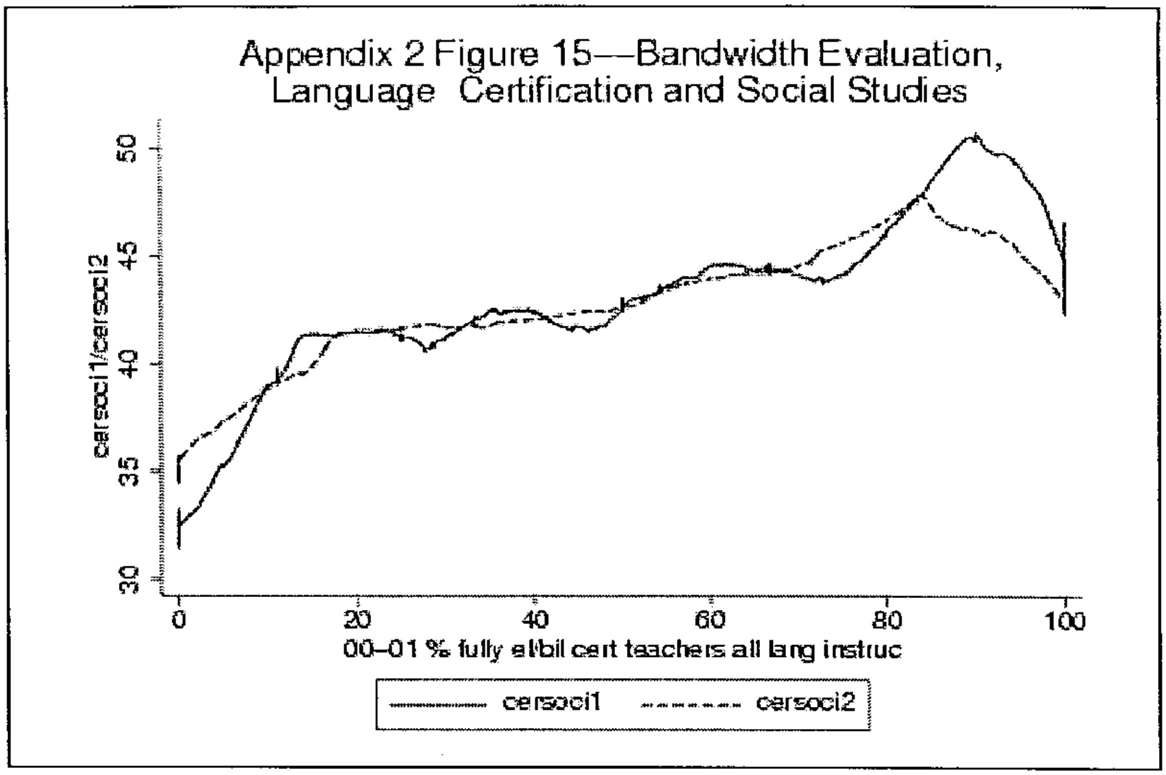


Appendix 2 Figure 13—Bandwidth Evaluation, Language Certification and Language



Appendix 2 Figure 14—Bandwidth Evaluation, Language Certification and Science




Appendix 2 Figure 15—Bandwidth Evaluation,
Language Certification and Social Studies
cersoci1/cersoci2
30
35
40
45
50
0
20
40
60
80
100
00–01 % fully el/bil cert teachers all lang instruc
cersoci1
cersoci2

Appendix 3 -- Full Models

Appendix 3, Table 1 -- Full Models for Table 3 of the Report

Variable	Reading		Mathematics		Language		Science		Social Studies	
	Rossell	Corrected	Rossell	Corrected	Rossell	Corrected	Rossell	Corrected	Rossell	Corrected
pcel01xxx	0.007	-0.001	0.014	0.013	0.017	0.014	-0.006	-0.014	-0.000	-0.004
	0.003	0.005	0.004	0.005	0.004	0.005	0.010	0.012	0.009	0.012
pcbil01xxx	-0.022	-0.020	-0.012	-0.009	-0.022	-0.021	-0.002	-0.026	0.014	0.004
	0.003	0.004	0.003	0.004	0.003	0.004	0.017	0.021	0.016	0.022
pcfree01xxx	-0.010	-0.030	-0.017	-0.031	-0.013	-0.031	-0.015	-0.019	-0.009	-0.022
	0.003	0.003	0.003	0.004	0.003	0.004	0.007	0.007	0.006	0.007
pcmin01xxx	-0.010	-0.038	-0.019	-0.041	-0.010	-0.035	-0.020	-0.042	-0.048	-0.071
	0.003	0.004	0.003	0.004	0.003	0.004	0.008	0.009	0.008	0.009
pcspan01xxx	-0.008	-0.022	-0.014	-0.027	-0.009	-0.022	-0.009	-0.015	-0.019	-0.027
	0.002	0.002	0.002	0.003	0.002	0.003	0.005	0.007	0.005	0.007
tot_en01xxx	-0.000	0.000	0.000	0.001	0.000	0.001	0.001	0.001	0.001	0.001
	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
elem01xxx	1.898	3.706	2.950	4.435	1.697	3.170	0.423	-6.621	-0.691	-0.251
	0.120	0.167	0.151	0.183	0.134	0.173	4.682	3.635	4.571	3.770
rd00np_axxx	0.946	0.868								
	0.004	0.006								
emerpc01xxx	0.011	0.009	-0.008	-0.011	0.012	0.013	-0.003	-0.007	0.009	0.023
	0.005	0.006	0.006	0.007	0.005	0.006	0.013	0.012	0.012	0.013
pcel01mi		-2.168		-3.005		-3.641		-0.684		-0.061
		1.347		1.484		1.401		1.898		1.969
pcbil01mi		-0.647		-1.471		-0.395		-0.466		-0.819
		0.270		0.298		0.290		0.495		0.514
pcfree01mi		-1.601		-2.926		-2.721		-1.435		-1.349
		0.499		0.549		0.533		0.687		0.710
pcmin01mi		-14.192		-6.718		-6.169		13.181		5.558
		5.481		6.042		5.829		6.316		6.548
pcspan01mi		0.000		0.000		0.000		0.000		0.000
		0.000		0.000		0.000		0.000		0.000
tot_en01mi		36.920		23.366		23.188		0.000		0.000
		6.079		6.701		6.470		0.000		0.000
elem01mi		-15.368		-5.660		-10.574		-9.741		-1.897
		3.235		3.568		3.436		6.936		7.183
rd00np_ami		-4.027								
		0.366								
emerpc01mi		-7.156		-7.750		-5.533		-4.077		-4.599
		0.794		0.858		0.837		1.267		1.288
ma00np_axxx			0.905	0.847						
			0.005	0.006						
ma00np_ami				-7.740						
				0.408						
la00np_axxx					0.938	0.873				
					0.005	0.006				
la00np_ami						-6.011				
						0.394				
sc00np_axxx							0.918	0.857		
							0.014	0.016		
sc00np_ami								-6.360		

								0.672		
ss00np_axxx									0.890	0.825
									0.014	0.016
ss00np_ami										-5.824
										0.672
_cons	4.035	9.814	7.875	12.456	4.815	9.982	4.998	9.069	8.582	13.086
	0.352	0.468	0.430	0.507	0.399	0.497	1.020	1.126	0.970	1.131
N	7275.00	8079.00	7275.00	8077.00	7271.00	8068.00	1240.00	1669.00	1239.00	1672.00
r2	0.96	0.92	0.94	0.90	0.95	0.91	0.91	0.85	0.91	0.84

Appendix 3, Table 2 -- Full Models for Table 4 of the Report

	Reading		Mathematics		Language		Science		Social Studies	
Variable	Rossell	Corrected	Rossell	Corrected	Rossell	Corrected	Rossell	Corrected	Rossell	Corrected
pcel01xxx	0.007	-0.001	0.014	0.012	0.017	0.014	-0.004	-0.016	0.001	-0.007
	0.003	0.005	0.004	0.005	0.004	0.005	0.009	0.012	0.009	0.012
pcbil01xxx	-0.022	-0.020	-0.012	-0.009	-0.022	-0.021	-0.003	-0.024	0.013	0.006
	0.003	0.004	0.003	0.004	0.003	0.004	0.017	0.021	0.016	0.022
pcfree01xxx	-0.010	-0.030	-0.017	-0.031	-0.013	-0.031	-0.014	-0.018	-0.010	-0.021
	0.003	0.003	0.003	0.004	0.003	0.004	0.007	0.007	0.006	0.007
pcmin01xxx	-0.008	-0.036	-0.020	-0.041	-0.008	-0.032	-0.020	-0.042	-0.048	-0.067
	0.003	0.004	0.003	0.004	0.003	0.004	0.008	0.008	0.007	0.009
pcspan01xxx	-0.008	-0.021	-0.015	-0.028	-0.009	-0.022	-0.009	-0.015	-0.019	-0.027
	0.002	0.002	0.002	0.003	0.002	0.003	0.005	0.007	0.005	0.007
tot_en01xxx	-0.000	0.001	0.000	0.001	0.000	0.001	0.001	0.001	0.001	0.001
	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
elem01xxx	1.855	3.658	2.962	4.452	1.637	3.108	0.446	-6.768	-0.795	-0.099
	0.121	0.167	0.152	0.183	0.134	0.173	4.700	3.628	4.582	3.766
rd00np_axxx	0.946	0.869								
	0.004	0.006								
wvrpc01xxx	-0.007	-0.064	-0.008	-0.053	-0.015	-0.050	-0.043	-0.060	0.019	-0.032
	0.020	0.023	0.025	0.025	0.023	0.024	0.055	0.042	0.053	0.044
pcel01mi		-2.438		-3.050		-3.932		-0.604		-0.426
		1.347		1.483		1.400		1.886		1.957
pcbil01mi		-0.591		-1.472		-0.332		-0.460		-0.754
		0.269		0.297		0.290		0.494		0.513
pcfree01mi		-1.546		-2.987		-2.606		-1.471		-1.173
		0.498		0.547		0.532		0.679		0.703
pcmin01mi		-13.993		-6.652		-5.964		13.065		5.035
		5.490		6.045		5.833		6.304		6.541
pcspan01mi		0.000		0.000		0.000		0.000		0.000
		0.000		0.000		0.000		0.000		0.000
tot_en01mi		34.739		21.351		21.608		0.000		0.000
		6.080		6.695		6.465		0.000		0.000
elem01mi		-14.070		-3.314		-9.170		-8.465		0.293
		3.287		3.620		3.489		6.951		7.201
rd00np_ami		-4.016								
		0.366								
wvrpc01mi		-6.307		-7.999		-5.458		-5.257		-6.160
		0.887		0.953		0.943		1.405		1.422
ma00np_axxx			0.904	0.848						
			0.005	0.006						
ma00np_ami				-7.776						
				0.408						
la00np_axxx					0.938	0.873				
					0.005	0.006				
la00np_ami						-5.995				
						0.394				
sc00np_axxx							0.920	0.859		
							0.014	0.016		
sc00np_ami								-6.264		

								0.672		
ss00np_axxx									0.888	0.826
									0.014	0.016
ss00np_ami										-5.691
										0.672
_cons	4.013	9.776	7.926	12.421	4.867	9.957	4.881	8.981	8.696	13.041
	0.352	0.469	0.430	0.508	0.399	0.498	1.025	1.127	0.972	1.134
N	7280.00	8079.00	7280.00	8077.00	7276.00	8068.00	1243.00	1669.00	1242.00	1672.00
r2	0.96	0.92	0.94	0.90	0.95	0.91	0.91	0.85	0.91	0.84

Appendix 3, Table 3 -- Full Models for Table 5 of the Report

	Reading		Mathematics		Language	
Variable	Rossell	Corrected	Rossell	Corrected	Rossell	Corrected
pcel01xxx	0.025	0.017	0.007	0.029	0.036	0.033
	0.005	0.007	0.007	0.008	0.006	0.007
pcbil01xxx	-0.041	-0.048	-0.071	-0.021	-0.044	-0.049
	0.004	0.005	0.005	0.006	0.005	0.006
pcfree01xxx	-0.047	-0.068	-0.066	-0.088	-0.060	-0.082
	0.005	0.006	0.006	0.007	0.006	0.006
pcmin01xxx	0.004	0.001	0.004	-0.000	0.009	0.005
	0.005	0.006	0.007	0.008	0.006	0.007
pcspan01xxx	-0.035	-0.066	-0.032	-0.126	-0.053	-0.087
	0.003	0.004	0.005	0.005	0.004	0.005
tot_en01xxx	-0.001	-0.001	-0.003	-0.000	-0.001	-0.001
	0.000	0.000	0.000	0.000	0.000	0.000
elem01xxx	3.698	6.474	7.369	7.294	3.939	6.164
	0.233	0.271	0.283	0.317	0.268	0.293
rd00np_axxx	0.756	0.651				
	0.010	0.013				
pccertf1xxx	-0.002	-0.001	-0.000	0.002	0.002	0.001
	0.003	0.004	0.004	0.005	0.004	0.004
pcel01mi		8.977		8.367		8.620
		5.785		7.041		6.420
pcbil01mi		-2.337		-6.650		-3.603
		3.408		4.148		3.782
pcfree01mi		-6.346		-6.868		-6.948
		1.769		2.153		1.963
pcmin01mi		-11.165		-7.564		-6.258
		8.756		10.659		9.719
pcspan01mi		0.000		0.000		0.000
		0.000		0.000		0.000
tot_en01mi		0.000		0.000		0.000
		0.000		0.000		0.000
elem01mi		3.858		1.970		-2.675
		7.805		9.500		8.662
read00npmi		5.599				
		0.375				
pccertf1mi		0.000		0.000		0.000
		0.000		0.000		0.000
ma00np_axxx			0.389	0.619		
			0.010	0.011		
math00npmi				2.713		
				0.462		
la00np_axxx					0.712	0.624
					0.011	0.012
lang00npmi						3.521
						0.418
_cons	10.393	14.844	14.288	25.630	14.594	19.808
	0.576	0.686	0.797	0.904	0.728	0.810

N	4769.00	5157.00	4798.00	5188.00	4788.00	5170.00
r2	0.78	0.70	0.65	0.72	0.74	0.68

Appendix 3, Table 4 -- Full Models for Table 7 of the Report

Variable	Reading	Math	Language	Science	Soc Stud
pcel01xxx	-0.005	0.007	0.009	-0.016	-0.006
	0.005	0.005	0.005	0.012	0.012
pcbil01xxx	-0.022	-0.012	-0.023	-0.028	0.001
	0.004	0.004	0.004	0.021	0.022
pcfree01xxx	-0.032	-0.034	-0.034	-0.017	-0.021
	0.003	0.004	0.004	0.007	0.007
pcmin01xxx	-0.042	-0.045	-0.039	-0.044	-0.072
	0.004	0.004	0.004	0.009	0.009
pcspan01xxx	-0.021	-0.027	-0.022	-0.015	-0.027
	0.002	0.003	0.003	0.006	0.007
lnsize	1.048	1.531	1.440	0.970	1.051
	0.080	0.090	0.088	0.145	0.150
elem01xxx	3.882	4.612	3.272	-6.770	-0.516
	0.161	0.172	0.164	3.611	3.747
rd00np_axxx	0.854				
	0.006				
emerpc01xxx	0.007	-0.015	0.011	-0.011	0.018
	0.006	0.007	0.006	0.012	0.013
wvrpc01xxx	-0.071	-0.052	-0.061	-0.067	-0.052
	0.023	0.025	0.024	0.043	0.044
pcel01mi	-2.182	-3.252	-3.752	-0.952	-0.281
	1.335	1.465	1.385	1.886	1.957
pcbil01mi	0.265	-0.302	0.636	0.075	-0.284
	0.276	0.303	0.295	0.506	0.526
pcfree01mi	-1.393	-2.745	-2.624	-1.675	-1.599
	0.494	0.541	0.526	0.683	0.706
pcmin01mi	20.681	13.030	13.909	11.038	3.307
	2.720	2.984	2.884	6.287	6.520
pcspan01mi	0.000	0.000	0.000	0.000	0.000
	0.000	0.000	0.000	0.000	0.000
tot_en01mi	0.000	0.000	0.000	0.000	0.000
	0.000	0.000	0.000	0.000	0.000
totzero	-36.624	-22.474	-22.890	0.000	0.000
	6.026	6.616	6.395	0.000	0.000
elem01mi	-16.449	-4.703	-10.403	-7.611	0.516
	3.280	3.601	3.471	6.925	7.171
rd00np_ami	-3.005				
	0.371				
emerpc01mi	-8.659	-4.290	-3.787	1.892	2.704
	1.699	1.864	1.719	2.823	2.927
wvrpc01mi	3.044	-2.785	-0.739	-6.971	-8.512
	1.891	2.064	1.932	3.132	3.231
ma00np_axxx		0.827			
		0.006			
ma00np_ami		-6.389			
		0.411			
la00np_axxx			0.852		
			0.006		

la00np_ami			-4.750		
			0.398		
sc00np_axxx				0.838	
				0.016	
sc00np_ami				-5.461	
				0.693	
ss00np_axxx					0.807
					0.016
ss00np_ami					-4.892
					0.696
_cons	4.427	4.930	3.077	4.965	8.472
	0.635	0.679	0.661	1.186	1.215
N	8079.00	8077.00	8068.00	1669.00	1672.00
r2	0.93	0.91	0.91	0.85	0.84

Appendix 3, Table 5 -- Full Models for Table 8 of the Report

Variable	Reading	Math	Language	Science	Soc Stud
pcel01x	-0.006	0.007	0.009	-0.014	-0.003
	0.004	0.005	0.005	0.011	0.012
pcbil01x	-0.023	-0.012	-0.024	-0.029	-0.000
	0.004	0.004	0.004	0.021	0.022
pcfree01x	-0.032	-0.034	-0.034	-0.019	-0.022
	0.003	0.004	0.004	0.007	0.007
pcmin01x	-0.044	-0.046	-0.040	-0.045	-0.073
	0.004	0.004	0.004	0.009	0.009
pcspan01x	-0.022	-0.027	-0.023	-0.015	-0.027
	0.002	0.003	0.003	0.007	0.007
lnsize	0.982	1.497	1.381	0.927	1.009
	0.082	0.091	0.089	0.150	0.155
elem01x	3.988	4.660	3.358	-6.591	-0.370
	0.160	0.172	0.163	3.621	3.759
rd00np_ax	0.855				
	0.006				
xeme0	0.034	-0.005	0.033	-0.006	0.023
	0.008	0.009	0.009	0.020	0.020
xeme1	-0.072	-0.048	-0.056	-0.028	0.002
	0.016	0.017	0.017	0.031	0.032
pcel01mi	-2.612	-3.352	-4.004	-0.954	-0.293
	1.332	1.462	1.382	1.890	1.962
pcbil01mi	0.293	-0.316	0.638	0.030	-0.326
	0.276	0.303	0.295	0.507	0.526
pcfree01mi	-1.220	-2.695	-2.487	-1.642	-1.561
	0.495	0.542	0.527	0.686	0.709
pcmin01mi	20.925	-9.275	14.612	11.380	3.623
	2.705	5.957	2.871	6.298	6.535
pcspan01mi	0.000	0.000	0.000	0.000	0.000
	0.000	0.000	0.000	0.000	0.000
totzero	-36.377	0.000	-23.286	0.000	0.000
	6.011	0.000	6.383	0.000	0.000
elem01mi	-15.646	-5.884	-11.003	-9.286	-1.357
	3.200	3.518	3.392	6.904	7.156
rd00np_ami	-2.956				
	0.370				
emerpc01mi	-6.409	-6.585	-4.506	-3.727	-4.240
	0.790	0.852	0.831	1.268	1.289
tot_en01mi	0.000	23.075	0.000	0.000	0.000
	0.000	6.606	0.000	0.000	0.000
ma00np_ax		0.827			
		0.006			
ma00np_ami		-6.392			
		0.411			
la00np_ax			0.852		
			0.006		
la00np_ami			-4.722		
			0.398		

sc00np_ax				0.837	
				0.016	
sc00np_ami				-5.607	
				0.692	
ss00np_ax					0.807
					0.016
ss00np_ami					-5.020
					0.696
_cons	4.691	5.068	3.349	5.208	8.720
	0.637	0.682	0.664	1.195	1.224
N	8079.00	8077.00	8068.00	1669.00	1672.00
r2	0.93	0.91	0.91	0.85	0.84

Appendix 3, Table 6 -- Full Models for Table 9 of the Report

Variable	Reading	Math	Language	Science	Soc Stud
pcel01x	-0.006	0.006	0.009	-0.015	-0.006
	0.004	0.005	0.005	0.011	0.012
pcbil01x	-0.022	-0.012	-0.024	-0.028	0.001
	0.004	0.004	0.004	0.021	0.022
pcfree01x	-0.031	-0.033	-0.034	-0.017	-0.021
	0.003	0.004	0.004	0.007	0.007
pcmin01x	-0.041	-0.047	-0.037	-0.046	-0.069
	0.004	0.004	0.004	0.008	0.008
pcspan01x	-0.021	-0.028	-0.022	-0.015	-0.027
	0.002	0.003	0.003	0.006	0.007
lnsize	1.077	1.518	1.457	0.941	1.059
	0.080	0.089	0.087	0.143	0.148
elem01x	3.933	4.695	3.284	-6.894	-0.380
	0.159	0.171	0.162	3.609	3.746
rd00np_ax	0.854				
	0.006				
xwvr0	-0.000	-0.005	-0.002	-0.060	-0.030
	0.025	0.027	0.027	0.050	0.052
xwvr1	-0.955	-0.765	-0.766	-0.169	-0.165
	0.151	0.165	0.160	0.214	0.222
pcel01mi	-1.948	-2.672	-3.522	-0.779	-0.575
	1.335	1.464	1.384	1.876	1.948
pcbil01mi	0.276	-0.379	0.645	0.026	-0.247
	0.275	0.302	0.294	0.506	0.525
pcfree01mi	-1.400	-2.887	-2.584	-1.752	-1.470
	0.492	0.539	0.524	0.678	0.701
pcmin01mi	18.406	-9.779	12.891	11.245	3.002
	2.688	5.954	2.852	6.283	6.519
pcspan01mi	0.000	0.000	0.000	0.000	0.000
	0.000	0.000	0.000	0.000	0.000
totzero	-34.470	0.000	-22.034	0.000	0.000
	6.004	0.000	6.372	0.000	0.000
elem01mi	-14.452	-3.782	-9.665	-7.856	1.007
	3.248	3.565	3.440	6.918	7.167
rd00np_ami	-2.827				
	0.371				
wvrpc01mi	-5.549	-7.008	-4.521	-5.049	-5.919
	0.878	0.941	0.932	1.399	1.416
tot_en01mi	0.000	21.549	0.000	0.000	0.000
	0.000	6.591	0.000	0.000	0.000
ma00np_ax		0.828			
		0.006			
ma00np_ami		-6.309			
		0.412			
la00np_ax			0.852		
			0.006		
la00np_ami			-4.605		
			0.399		

sc00np_ax				0.839	
				0.016	
sc00np_ami				-5.470	
				0.695	
ss00np_ax					0.806
					0.016
ss00np_ami					-4.801
					0.698
_cons	4.168	4.876	2.911	5.060	8.474
	0.632	0.676	0.657	1.185	1.214
N	8079.00	8077.00	8068.00	1669.00	1672.00
r2	0.93	0.91	0.91	0.85	0.84

Appendix 3, Table 7 -- Full Models for Table 10 of the Report

Variable	Reading	Math	Language	Science	Soc Stud
pcel01x	-0.005	0.007	0.009	-0.016	-0.006
	0.004	0.005	0.005	0.012	0.012
pcbil01x	-0.023	-0.012	-0.024	-0.027	0.002
	0.004	0.004	0.004	0.021	0.022
pcfree01x	-0.032	-0.034	-0.034	-0.017	-0.021
	0.003	0.004	0.004	0.007	0.007
pcmin01x	-0.043	-0.045	-0.040	-0.045	-0.072
	0.004	0.004	0.004	0.009	0.009
pcspan01x	-0.022	-0.027	-0.023	-0.015	-0.028
	0.002	0.003	0.003	0.007	0.007
lnsize	0.964	1.491	1.371	0.949	1.031
	0.081	0.091	0.089	0.150	0.155
elem01x	3.956	4.651	3.339	-6.687	-0.436
	0.160	0.172	0.164	3.616	3.753
rd00np_ax	0.856				
	0.006				
xeme0	0.034	-0.005	0.033	-0.004	0.025
	0.008	0.009	0.009	0.020	0.020
xeme1	-0.065	-0.042	-0.050	-0.025	0.005
	0.016	0.017	0.017	0.031	0.032
xwvr0	-0.010	-0.001	-0.012	-0.057	-0.039
	0.025	0.028	0.027	0.050	0.052
xwvr1	-0.916	-0.744	-0.732	-0.153	-0.164
	0.151	0.166	0.160	0.216	0.224
pcel01mi	-1.685	-2.787	-3.386	-0.952	-0.275
	1.336	1.469	1.387	1.888	1.959
pcbil01mi	0.245	-0.331	0.617	0.059	-0.304
	0.275	0.303	0.295	0.507	0.527
pcfree01mi	-1.277	-2.721	-2.516	-1.664	-1.593
	0.494	0.541	0.526	0.686	0.710
pcmin01mi	20.985	-9.773	14.191	11.088	3.355
	2.713	5.951	2.881	6.291	6.525
pcspan01mi	0.000	0.000	0.000	0.000	0.000
	0.000	0.000	0.000	0.000	0.000
totzero	-37.292	0.000	-23.423	0.000	0.000
	6.005	0.000	6.382	0.000	0.000
elem01mi	-17.070	-5.048	-10.918	-7.603	0.525
	3.270	3.598	3.465	6.928	7.175
rd00np_ami	-2.760				
	0.371				
emerpc01mi	-8.935	-4.410	-4.032	1.847	2.665
	1.693	1.862	1.717	2.826	2.930
wvrpc01mi	3.090	-2.780	-0.683	-6.976	-8.521
	1.884	2.062	1.928	3.133	3.232
tot_en01mi	0.000	22.841	0.000	0.000	0.000
	0.000	6.608	0.000	0.000	0.000
ma00np_ax		0.828			
		0.006			

ma00np_ami		-6.225			
		0.412			
la00np_ax			0.853		
			0.006		
la00np_ami			-4.558		
			0.399		
sc00np_ax				0.838	
				0.016	
sc00np_ami				-5.422	
				0.696	
ss00np_ax					0.807
					0.016
ss00np_ami					-4.849
					0.700
_cons	4.725	5.042	3.348	5.056	8.565
	0.636	0.681	0.663	1.196	1.226
N	8079.00	8077.00	8068.00	1669.00	1672.00
r2	0.93	0.91	0.91	0.85	0.84

Appendix 3, Table 8 -- Full Models for Table 11 of the Report

	Coef.	Std. Err.	z	P>|z|	[95% Conf.	Interval]
rd01np_a						
pcel01xxx	-.0133789	.0045048	-2.97	0.003	-.0222081	-.0045496
pcbil01xxx	-.024363	.0038159	-6.38	0.000	-.0318421	-.0168839
pcfree01xxx	-.0495346	.0032711	-15.14	0.000	-.0559459	-.0431233
pcmin01xxx	-.0620135	.0035803	-17.32	0.000	-.0690307	-.0549963
pcspan01xxx	-.0308496	.0023893	-12.91	0.000	-.0355325	-.0261666
tot_en01xxx	1.086037	.0812604	13.36	0.000	.9267698	1.245305
elem01xxx	5.024264	.1532769	32.78	0.000	4.723847	5.324681
rd00np_axxx	.7716286	.0045509	169.55	0.000	.7627089	.7805482
xeme0	.0308288	.0083168	3.71	0.000	.0145283	.0471293
xeme1	-.0438404	.0160544	-2.73	0.006	-.0753064	-.0123744
xwvr0	-.0041005	.0251841	-0.16	0.871	-.0534605	.0452594
xwvr1	-.8041901	.1508356	-5.33	0.000	-1.099822	-.5085578
pcel01mi	-1.759063	1.339383	-1.31	0.189	-4.384206	.8660802
pcbil01mi	-.0254734	.2779736	-0.09	0.927	-.5702917	.519345
pcfree01mi	-1.936656	.4966935	-3.90	0.000	-2.910158	-.9631547
pcmin01mi	23.21167	2.716291	8.55	0.000	17.88784	28.5355
pcspan01mi	(dropped)					
tot_en01mi	(dropped)					
elem01mi	-21.31184	3.27398	-6.51	0.000	-27.72872	-14.89496
rd00np_ami	-4.522014	.3388495	-13.35	0.000	-5.186146	-3.857881
emerpc01mi	-8.327489	1.697334	-4.91	0.000	-11.6542	-5.000775
wvrpc01mi	3.485439	1.893774	1.84	0.066	-.2262894	7.197168
totzero	-41.35163	6.01718	-6.87	0.000	-53.14509	-29.55818
_cons	9.951938	.6133128	16.23	0.000	8.749867	11.15401
ma01np_a						
pcel01xxx	.0030697	.0049391	0.62	0.534	-.0066107	.0127502
pcbil01xxx	-.0127631	.0042041	-3.04	0.002	-.0210029	-.0045233
pcfree01xxx	-.049756	.0035894	-13.86	0.000	-.0567912	-.0427209
pcmin01xxx	-.0581874	.0038812	-14.99	0.000	-.0657944	-.0505805
pcspan01xxx	-.0361699	.0026451	-13.67	0.000	-.0413542	-.0309855
tot_en01xxx	1.680221	.0908882	18.49	0.000	1.502084	1.858359
elem01xxx	5.545551	.1663925	33.33	0.000	5.219427	5.871674
ma00np_axxx	.7547447	.0047316	159.51	0.000	.745471	.7640184
xeme0	-.0097493	.009171	-1.06	0.288	-.0277242	.0082257
xeme1	-.0251083	.0176844	-1.42	0.156	-.0597691	.0095524
xwvr0	-.0018773	.0277451	-0.07	0.946	-.0562567	.0525021
xwvr1	-.6337387	.1662431	-3.81	0.000	-.9595692	-.3079083
pcel01mi	-2.869526	1.475748	-1.94	0.052	-5.761939	.0228859
pcbil01mi	-.6980935	.3064824	-2.28	0.023	-1.298788	-.097399
pcfree01mi	-3.183606	.5477362	-5.81	0.000	-4.25715	-2.110063
pcmin01mi	-12.49133	5.973761	-2.09	0.037	-24.19969	-.782975
pcspan01mi	(dropped)					
tot_en01mi	27.23728	6.632996	4.11	0.000	14.23685	40.23772
elem01mi	-9.402805	3.610941	-2.60	0.009	-16.48012	-2.32549
ma00np_ami	-7.579148	.379969	-19.95	0.000	-8.323873	-6.834422
emerpc01mi	-4.040951	1.870153	-2.16	0.031	-7.706383	-.3755189

wvrpc01mi	-1.637221	2.086738	-0.78	0.433	-5.727153	2.452711
totzero	(dropped)					
_cons	9.465524	.6644533	14.25	0.000	8.16322	10.76783
la01np_a						
pcel01xxx	.0020511	.0047979	0.43	0.669	-.0073526	.0114549
pcbil01xxx	-.0271195	.004077	-6.65	0.000	-.0351103	-.0191286
pcfree01xxx	-.0554477	.0035021	-15.83	0.000	-.0623118	-.0485837
pcmin01xxx	-.0549106	.0037627	-14.59	0.000	-.0622854	-.0475358
pcspan01xxx	-.0331924	.0025566	-12.98	0.000	-.0382032	-.0281817
tot_en01xxx	1.611791	.0880972	18.30	0.000	1.439123	1.784458
elem01xxx	4.284545	.1589804	26.95	0.000	3.972949	4.596141
la00np_axxx	.7636748	.0046935	162.71	0.000	.7544757	.7728738
xeme0	.0299637	.0088797	3.37	0.001	.0125599	.0473675
xeme1	-.0457681	.0171409	-2.67	0.008	-.0793636	-.0121726
xwvr0	-.0036354	.0268883	-0.14	0.892	-.0563356	.0490648
xwvr1	-.5985008	.1610334	-3.72	0.000	-.9141205	-.2828811
pcel01mi	-2.605647	1.430132	-1.82	0.068	-5.408654	.197359
pcbil01mi	.1376627	.297106	0.46	0.643	-.4446545	.7199798
pcfree01mi	-3.075305	.5309773	-5.79	0.000	-4.116001	-2.034609
pcmin01mi	(dropped)					
pcspan01mi	(dropped)					
tot_en01mi	16.22947	2.900342	5.60	0.000	10.54491	21.91404
elem01mi	-15.91342	3.495471	-4.55	0.000	-22.76442	-9.062424
la00np_ami	-6.636496	.3611028	-18.38	0.000	-7.344244	-5.928747
emerpc01mi	-1.421683	1.812178	-0.78	0.433	-4.973487	2.130121
wvrpc01mi	-2.670008	2.022113	-1.32	0.187	-6.633277	1.293261
totzero	-13.48979	5.788593	-2.33	0.020	-24.83523	-2.144362
_cons	8.558699	.6450286	13.27	0.000	7.294466	9.822932

Correlation matrix of residuals:

	rd01np_a	ma01np_a	la01np_a
rd01np_a	1.0000		
ma01np_a	0.7744	1.0000	
la01np_a	0.8396	0.7820	1.0000

Breusch-Pagan test of independence: chi2(3) = 15426.155, Pr = 0.0000