The Educational Effectiveness of Bilingual Education[1]

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Bilingual education is the use of the native tongue to instruct limited English-speaking children. The authors read studies of bilingual education from the earliest period of this literature to the most recent. Of the 300 program evaluations read, only 72 (25%) were methodologically acceptable—that is, they had a treatment and control group and a statistical control for pre-treatment differences where groups were not randomly assigned. Virtually all of the studies in the United States were of elementary or junior high school students and Spanish speakers. The few studies conducted outside the United States were almost all in Canada. The research evidence indicates that, on standardized achievement tests, transitional bilingual education (TBE) is better *than regular classroom instruction in only 22% of the methodologically acceptable studies when the outcome is reading, 7% of the studies when the outcome is language, and 9% of the studies when the outcome is math. TBE is never better than structured immersion, a special program for limited English proficient children where the children are in a self-contained classroom composed solely of English learners, but the instruction is in English at a pace they can understand. Thus, the research evidence does not support transitional bilingual education as a superior form of instruction for limited English proficient children.*

Bilingual education as it is practiced in the public schools of the United States means teaching non-English-speaking students to read and write in their native tongue, teaching them content in their native tongue, and gradually transitioning them to English over a period of several years. This pedagogical approach for preparing non-English speaking students for instruction in English is widely practiced in the United States and is highly controversial. Critics of transitional bilingual education allege that children emerge from these programs not knowing English, while supporters claim that the alternative—all-English instruction in a regular classroom—stunts a Limited English Proficient (LEP) child's intellectual development and self-esteem. The purpose of this article is to summarize the quantitative evidence that bears on these assertions and to explain

the relevance of our findings for the major theories behind transitional bilingual education as it is practiced in the United States.

We write for a broad range of readers—from researchers to curriculum coordinators and classroom teachers. Our focus in this article is not on what goes on in the classroom, nor do we address the qualitative research in this field in any systematic way. Although ethnographic research in bilingual education can answer some questions of educational interest, it does not answer the questions we seek to answer. We want to know if bilingual education as a pedagogical approach to English language instruction achieves its primary goal. This avowed goal, which its supporters have not disputed, is to transition non-English-speaking students from their native tongue to English and to produce the highest possible achievement for these students both in the English class itself and in other subjects. The goal of this study is to determine whether bilingual education is the most effective instructional approach for LEP children if the goal is their highest possible achievement in the English language and in subjects tested in English.

Background for the Study

Although bilingual education is controversial, all levels of government—federal, state, and local—have for the most part accepted it as the preferred method of instruction for LEP children. Nevertheless, researchers continue to ask whether "it works." Indeed, this question was asked recently in the December 1994 Report of the Massachusetts Bilingual Education Commission, and this same Commission concluded that

> . . . we do not know, on the basis of measured outcomes, whether TBE programs in Massachusetts produce good results or poor results. There are no comprehensive data that evaluate the performance of TBE pupils compared with pupils from other groups. This specialized program which accounts for 5% of all pupils in Massachusetts public schools and 17% of all pupils in Boston public schools is not held separately accountable for its performance. (p. 41)

This problem of a lack of accountability exists nationally, as well as in Massachusetts. Even well known supporters of bilingual education for ideological or political reasons have questioned the research foundation for bilingual education. Hakuta (1986) concluded, in *Mirror of Language*:

> There is a sober truth that even the ardent advocate of bilingual education would not deny. Evaluation studies of the effectiveness of bilingual education in improving either English or math scores have not been overwhelmingly in favor of bilingual education . . . An awkward tension blankets the lack of empirical demonstration of the success of bilingual education programs. Someone promised bacon, but it's not there. (p. 219)

Carter (Carter & Chatfield, 1986), despite being an advocate of bilingual education and an expert witness for LEP Spanish-speaking plaintiffs in several bilingual education cases over the last decade, began an article with the question:

> Regardless of the many roots of the debate, one issue is unresolved. Does bilingual education work? (p. 210)

Paulston (1982), a well known linguist and advocate of bilingual education, made some telling points in her 1982 report to the National Swedish Board of Education. With regard to transitional bilingual education in the United States as the best way to learn English, she noted:

> The rationale for bilingual programs are that they are more efficient in teaching English although there [are] not much hard data to support such a view; it has however been the standard argument . . . The Canadians believe, *with justification* [emphasis added], that fluent proficiency in the target language only occurs when that language is used as a medium of instruction. (pp. 47–48)

She also approvingly cited Toukomaa (1982), another bilingual education advocate, who wrote:

> . . . we wish to dissociate ourselves from those arguments, for teaching in the mother tongue, which attempt to frighten parents into choosing mother tongue-teaching by threatening emotional and intellectual under-development in those children who do not receive mother tongue-teaching. Teaching in the mother tongue does not seem to have the magical effect on the child's development, for good or for ill, which it has sometimes been ascribed. (cited in Paulston, 1982, p. 49)

The Association for Supervision and Curriculum Development, an organization of 90,000 principals, school superintendents, teachers and other educational leaders, noted in their 1987 report on bilingual education that "It is unclear which approach is better [teaching children in English or in their native tongue]" (p. 35). This lack of certainty among those who review the research seems not to have dampened the intensity of the debate, however.

The ill-considered and thoughtless treatment of linguistic minorities during the first half of this century (e.g., mislabeling LEP children as mentally retarded and punishing LEP children who used their native tongue in school), combined with the continuing problem of a high dropout rate and low achievement, has influenced many social scientists, practitioners, civil rights attorneys, and reviewers of the research to believe that any policy which ignores the mother tongue in favor of English is racist, and any policy which maintains the mother tongue, however inadequately, is equitable. This has created an atmosphere in which it is

difficult for anyone to criticize current policy in this field, and all too easy for both supporters and critics of bilingual education to interpret flawed studies as support for, or refutation of, bilingual education.

To assess the educational effectiveness of transitional bilingual education, it must be compared to three other educational programs for LEP children. The first instructional alternative we call *submersion*, commonly known as "sink-or-swim." In this model, the LEP child is placed in a regular English classroom with English-speaking children and given no more special help than any child with educational problems.

A second alternative is *English as a Second Language* (ESL) instruction for one or two periods a day, or in some districts two or three periods a week, and participation in the regular classroom for the rest of the time. ESL is a pull-out program usually based on a special curriculum for teaching English to LEP children, but the instructors do not have to speak the child's native language.

A third alternative is *structured immersion* where instruction is in the language being learned (L2), but the teacher speaks the students' native tongue (L1). The second language used in these programs is always geared to the children's language proficiency at each stage so that it is comprehensible. The native tongue is used only in the rare instances when the student cannot complete a task without it. The student thus learns the second language and subject matter content simultaneously. Immersion programs in which the second language is not the dominant language of the country typically include at least 30–60 minutes a day of native tongue language arts beginning sometime in the early elementary years. In fact, most of the Canadian "immersion" programs become bilingual programs after the first grade and, as a result, serve as a laboratory for assessing the effect of "time-on-task" in second language learning.

In transitional bilingual education (TBE), the student is taught to read and write in the native tongue, and subject matter is also taught in the native tongue. The second language (English) is initially taught for only a small portion of the day. As the child progresses in English, the amount of instructional time in the native tongue is reduced and English increased, until the student is proficient enough in English to join the regular instructional program. The rationale underlying TBE differs depending on the age of the child. For very young children, learning to read in the native tongue first is considered a necessary condition for optimal reading ability in the second language. For all children, it is argued that learning a second language takes time and children should not lose ground in other subject matters, particularly math, during that time period.

A variation on transitional bilingual education is bilingual maintenance. These programs resemble TBE in their early years, but they differ in that the goal of a bilingual maintenance program is to produce bilingual children and thus students are not exited when they master English. Although bilingual maintenance programs enjoy a great deal of support from the intellectual community, they are not implemented widely because they do not enjoy political support from the state and federal legislatures that fund bilingual education.

The majority of elementary school programs have as their goal exiting a student after three years. But these programs also allow students to stay in the program longer than three years if they are judged to be below par in English language skills. Indeed, many children stay in a bilingual program throughout their elementary school career (see Ramirez and his associates, 1991; Rossell, 1992; Rossell & Baker, 1988). Transitional bilingual education is less common once a child reaches the grade where departmentalization occurs and different subjects are taught by different teachers. Because teachers have to be certified in both a subject matter and in a foreign language to teach in a bilingual program in junior high or high school, few school districts are able to staff bilingual programs at these grade levels. Thus the typical LEP child enters a regular English program at junior high school. It is only in large school districts with large numbers of LEP students of a single language group that native tongue instruction in a subject might occur at the secondary level.

According to Young and his colleagues (1984), at least 40% of all LEP children nationwide are in TBE programs, and only 26% are in English instruction classrooms. The other 34% are divided among bilingual maintenance, Spanish instruction, and ESL classes. However, Okada's (1983) study found *no* projects which reported *English only* as a literacy goal for LEP students. The American Legislative Exchange Council (1994) recently reported that 60% of the state and locally funded programs were labeled bilingual education in 1991–92. Thus, at least nominally, TBE appears to be the dominant special language instructional program in the United States. We use the word "nominally" in a deliberate sense, however, because it is quite clear from visiting classrooms and reading evaluation reports that virtually the only children receiving native tongue instruction in the United States according to the theory—learning to read and write in the native tongue and learning subject matter in the native tongue—are Spanish-speaking children and Creole-speaking Haitian children. Other bilingual education programs are generally closer to what we call structured immersion, even though for political, legal, or funding reasons they may describe themselves as "bilingual education."

This phenomenon, of course, only complicates the issue of evaluating and analyzing the effects of bilingual education programs.

While ethnographic studies can tell us a great deal about what goes on in a bilingual classroom, they cannot tell us whether bilingual education is more or less effective than some other instructional approach. The typical ethnographic study is limited to one classroom and cannot tell us how the pedagogical approach varies across classrooms and ethnic groups. The first author, for example, visited more than a hundred bilingual education classrooms in Massachusetts and California over the last 15 years,[2] discovering that Spanish-speaking elementary school children are the only LEP students in true bilingual education programs in those states, a situation we did not learn from the dozens of ethnographic studies of bilingual classrooms that we have read. Conclusions about the effectiveness of bilingual education as a pedagogical approach cannot be drawn from these ethnographic studies, although they often are, because there are no comparisons to similar students in a different kind of classroom and no attempt to systematically link classroom processes to outcomes.[3]

The Present Study

The purpose of this report is to update the comprehensive reviews that Baker and de Kanter (1981, 1983a, 1983b) and Rossell and Ross (1986) conducted about a decade ago. The focus of this report is on the effect of alternative programs for non-English-speaking children on school performance.[4] There have been a number of other extensive reviews of the effectiveness of TBE for LEP students (Dulay & Burt, 1978; Engle, 1975; Government Accounting Office, 1987; Holland, 1986; Peterson, 1976; Ravitch, 1983; Rotberg, 1982; Troike, 1978; Willig, 1985; Yates & Ortiz, 1983; Zappert & Cruz, 1977; Zirkel, 1972). None of these reviews, however, has provided a definitive answer to the effectiveness question.

In this report, we provide a) an explanation of our methodological approach and the reasons for accepting or rejecting a study for inclusion in our analysis; b) a summary of the findings of the methodologically acceptable studies on the effect of transitional bilingual education compared to ESL, structured immersion, doing nothing, and bilingual maintenance on reading, language, and math achievement as measured by standardized achievement tests; c) a discussion of previous reviews of quantitative studies; d) an examination of the theories on which bilingual education rests in light of our research findings; e) a discussion of the implications of the research findings; and f) our conclusions regarding the effectiveness of educational alternatives for LEP children.

Method

We began with the studies reviewed earlier by Baker and de Kanter (1981, 1983a, 1983b) and by Rossell and Ross (1986) and added to them. The total number of studies and books we have read now numbers above 500, of which 300 are program evaluations in the sense that their purpose was to evaluate the effectiveness of TBE or some other second language acquisition program. Reviewing the research was a frustrating and arduous task. This is a fugitive literature, most of it unpublished, and some of it available only by writing directly to school districts. It consists in large part of local evaluations that do not even come close to meeting methodologically sound research standards. This is true not only of the "in-house" evaluations performed by school district staff but of those conducted by outside consulting firms that are supposedly hired for their methodological expertise. Indeed, we find ourselves wondering how many millions of dollars are wasted each year on methodologically inadequate descriptive evaluations of local school district bilingual education programs. Unfortunately, the fact that an article was published in an academic journal did not guarantee it was methodologically sound. Approximately 11% of the methodologically unacceptable studies were published in academic journals.

Each of the 300 program evaluations[5] we were able to find was assessed to determine if it addressed the relevant questions with a methodologically sound research design. The time period for the studies was as far back as we could find a study on the subject and as recent as the writing of this paper in 1995. The study had to claim to be a program evaluation—that is, a study testing the effectiveness of bilingual education as an educational treatment—for it to be considered at all.

Methodologically Acceptable Studies

Acceptable studies generally had the following characteristics:

1. They were true experiments in which students were randomly assigned to treatment and control groups;
2. They had non-random assignment that either matched students in the treatment and comparison groups on factors that influence achievement or statistically controlled for them;
3. They included a comparison group of LEP students of the same ethnicity and similar language background;
4. Outcome measures were in English using normal curve equivalents (NCEs), raw scores, scale scores, or percentiles, but not grade equivalents;

5. There were no additional educational treatments, or the studies controlled for additional treatments if they existed.

Analysis of covariance was by far the most common statistical method used to control for preexisting differences in nonexperimental studies. Many statisticians have serious reservations about whether this method succeeds in properly adjusting preexisting differences. Similarly there are doubts that matching students on important characteristics that influence achievement is entirely successful. Nevertheless, as do most statisticians, we generally accepted both methods unless there were serious defects in their application.

Of course, as occurs in any analysis of this kind, one can never be certain about what the treatment actually is. The likelihood that these evaluations are of true bilingual education programs, that is, programs that use native tongue instruction according to the theory behind TBE, is enhanced by the fact that the acceptable studies in the United States are virtually all of Spanish language programs. Almost all of the other acceptable studies were conducted in Canada and are well described. There is only one acceptable study of a non-Spanish language TBE program in the United States (Yap, Enoki, & Ishitani, 1988), but no acceptable United States studies evaluating the effectiveness of TBE programs for Haitian, Portuguese, Khmer, Cape Verdean, or Vietnamese students. Altogether, there were 72 methodologically acceptable studies, all of which are listed in Appendix A.

Some methodologically sound Canadian studies are not included in this category because they compared students in their second language learning programs not to students in other types of programs but to native speakers of each language. These studies are cited in the text where relevant but are not included in the assessment of alternative second language learning programs.

Methodologically Unacceptable Studies

Unacceptable studies generally had the following characteristics:

1. The study did not compare program alternatives or assess educational outcomes.
2. The study did not use randomly assigned students and made no effort to control for possible initial differences between students in different programs.
3. The study did not apply appropriate statistical tests.
4. The study used a norm-referenced design.

5. The study examined gains over the school year without a control group.
6. The study used grade-equivalent scores.
7. The study compared test results in different languages for students in different programs.
8. The study did not control for the confounding effect of other important educational treatments that were administered to at least one of the groups, but not all of them.

We discuss each of these fatal flaws in more detail below.

Reasons for Rejecting Studies

The Study Did Not Compare Program Alternatives or Assess Educational Outcomes

If a study claimed to be assessing the effectiveness of bilingual education on school performance, but merely described what went on in the bilingual education classroom rather than comparing students in bilingual education to similar students not in bilingual education, the study was rejected as not methodologically acceptable. If the purpose of the study was not to draw conclusions about the effectiveness of bilingual education, but it contained information that we referred to, we listed the study in the references, but did not classify it as "methodologically unacceptable."

The Study Did Not Use Randomly Assigned Students and Made No Effort To Control for Between Group Differences

Studies were rejected if students were not matched on important factors affecting school performance and these factors were not controlled for statistically. Among the important factors affecting the performance of non-English-speaking children in school, especially in learning English, are the following: *age* (Asher & Garcia, 1969; Giles, 1971; Izzo, 1981; Krashen, Long, & Scarcella, 1979); *socioeconomic status* (DeAvila, 1981; Izzo, 1981; Moore & Parr, 1978; Rosenthal, Milne, Ginsberg, & Baker, 1983; Veltman, 1980); *ethnicity* (Baker & de Kanter, 1981; Balasubramonian, Seelye, & Elizondo de Weffer, 1973; Matthews, 1979; New York City Schools, 1994; Rosenthal, Milne, Ginsberg, & Baker, 1983; Veltman, 1980); *student's motivation and self-concept* (Christian, 1976; Del Buono, 1971; Izzo, 1981; Modiano, 1973; Skoczylas, 1972; van Maltitz, 1975; Zirkel, 1972); *parental support* for the educational program (Del Buono, 1971; Izzo, 1981; Lambert & Tucker, 1972); *the language and environment of the*

community (Lambert & Sidoti, 1980; Lambert & Tucker, 1972; Read, 1980; Skoczylas, 1972; Teschner, 1990); *cognitive ability* (Coronado, 1979; Fishman, 1965; Humphrey, 1977; Jensen, 1962a, 1962b; Landry, 1974; Malherbe, 1946; Peal & Lambert, 1962; Segalowitz, 1977); *place of birth*—immigrant or native-born (Anderson & Johnson, 1971; Baral, 1979; Cardenas & Cardenas, 1977; Carter, 1970; Ferris, 1979; Kimball, 1968; Troike, 1978); and degree of *home-language dominance* (Teschner, 1990).

If the students in one program are substantially different from those in another program on any of the dimensions detailed above, one cannot determine whether the outcomes are a result of the program treatment or these other factors unless one statistically controls for, or matches students on, these dimensions.

Bias can also occur when parents are permitted to volunteer their children for a bilingual program, but the control group consists of students assigned to their programs. If bias from self-selection occurs, the evaluator cannot be sure whether the outcomes observed are a function of the program or of the characteristics that caused the parents to volunteer their child for the program. Parents who volunteer their child for a special program are usually more involved in their child's schooling and more ambitious than are parents who do not volunteer their child. In the case of second language learning programs, they may also be more gifted language learners. Thus, the special program may show gains solely due to the inclusion of these "better" students even though the program might actually be no more effective than regular schooling (see Laumann, 1969).

Appropriate Statistical Tests Were Not Applied

Studies that did not apply appropriate statistical tests to demonstrate the presence of program effects were not acceptable. For a study to be acceptable, it had to use statistical tests designed to take into account the number of subjects in each group, the size of the outcome difference between the groups, and the variation in outcomes within groups to verify that the results were "statistically significant"—that is, that they could not have happened by chance.

The Study Used an English-Speaking Norm-Referenced Design

Some studies form what they believe amounts to a control group by comparing LEP achievement to a national norm based on the achievement of English-speaking students to see if students in the special program showed greater gains. The norm-referenced approach is widely used in educational evaluation where it is thought to be suitable for analyzing the progress of regular English-speaking students.

When it is applied to assessing the effectiveness of bilingual education, however, apples are being compared to oranges. Standardized achievement tests are designed for English-speaking students, and they are constructed so that students who make grade level progress will leave with the same percentile score that they entered with. This is not the case with LEP students. For an LEP child, a standardized achievement test becomes both a test of English comprehension and an achievement test. We should expect LEP children to make larger gains than an English-speaking population because the LEP children are starting from a point where their low scores reflect their lack of English skills. When they learn enough English to be able to understand the test, their scores may rise dramatically because they can now demonstrate what they actually know about the topic (see for example, Cohen; 1975; Garcia, 1978; Stern, 1975; and Young, 1980), thus violating the assumptions underlying the norm-referenced model. Therefore, studies that relied entirely on comparing the progress of bilingual students to the rate of progress conveyed by norms based on the achievement of English-speaking students were classified as "methodologically unacceptable".

The Study Examined Gains Over the School Year Without Using a Comparison Group

Most students learn something over the school year, so their scores will increase even if they are rapidly falling behind the norm or even if they might have made greater gains in another program. Indeed, an achievement gain of 20 points in a year may actually be construed as a *negative* outcome if the same student would have gained 40 points in an alternative program. Without a comparison group, the conclusion would be that the program increased achievement by 20 points. With a comparison group, however, the correct conclusion is that the program confined achievement to 20 points. Thus, a comparison or control group is essential to a valid program evaluation. Unfortunately, it is often lacking in bilingual education evaluations.

The Study Used Grade-Equivalent Scores

The use of grade equivalents has often been criticized by evaluation experts. Perez and Horst (1982), for example, emphatically state:

> Grade-equivalent scores provide an illusion of simplicity, but in fact they are almost impossible to interpret even for specialists in test construction. Grade-equivalent scores should never be used by anyone for any purpose whatsoever. (pp. 115–116)

The problem with grade equivalents is that the methods used to produce them are inaccurate, and they do not correspond to the time pattern of

learning. As a result, equal grade-equivalent gains for two students may not represent equal learning. Because of these problems, we classified as methodologically unacceptable the few studies that used grade equivalent scores to test program effectiveness.

The Study Compared Test Results in Different Languages for Students in Different Programs

Because the goal of all federal and state legislation and regulations is that the LEP child will eventually be able to demonstrate understanding in subjects such as math, geography, science, or history in competition with English-speaking children in an English-speaking society, and to speak, read, and write English, the effectiveness of transitional bilingual education ultimately must be determined in English. Testing a LEP child in a bilingual program in Spanish for his or her understanding of social studies and comparing it to the understanding in English of a LEP child taught in an all-English program is in a sense cheating since we do not know whether the child taught in Spanish and tested in Spanish will ever be able to compete in that subject in English. Since virtually no one would declare a bilingual program a success that produced children who only understood history in their native tongue, we rejected the few studies that compared only Spanish test results for bilingual program children to English test results for LEP children in an all-English program.

Similarly, it is inappropriate to assess program effectiveness by comparing the Spanish language proficiency of a LEP child in a bilingual program with the English language proficiency of a LEP child in an all-English program. High proficiency in Spanish for a LEP child in a bilingual program and low proficiency in English for a LEP child in an all-English program cannot be used as evidence that the bilingual program is more effective, not only because two different languages are being compared but because English, not Spanish, proficiency is the goal of the bilingual program. By the same token, the relative Spanish proficiency of the two groups cannot be used as evidence of the superiority of one program over the other since English, not Spanish, proficiency is the goal of the program. Unfortunately, there are a number of program evaluations that have done exactly that. Although such comparisons are always interesting (and certainly legitimate to include in a program evaluation), they cannot suffice as the only means of evaluating a program whose goal is English language achievement.

Additional Educational Treatments Were Administered to Some But Not All Groups

If, for example, the children in the bilingual program went to school for more hours than the children in the all-English program, and if, as a

result of their extended day, they received as much English instruction as the children in the all-English program, such a study would be rejected unless the study controlled for that additional instructional time. Unfortunately, most studies do not provide enough information to determine whether there are additional treatments. Part of the positive effect of the bilingual education program in McConnell (1980), for example, may be due to the fact that the migrant children were not only going to school year round, but were being taught on the bus while en route to their various seasonal locations. Because the study is otherwise well designed and we are not sure how much additional English language time-on-task this additional treatment amounted to, we have included the study in our analysis.

In Appendix B are listed the 228 studies we found methodologically unacceptable for our analysis.

Results

If we consider only the 72 methodologically acceptable studies which assess alternative second language programs, using the above criteria, there is as yet, some twelve years after Baker and de Kanter (1983a, 1983b) and nine years after Rossell and Ross (1986), no consistent research support for transitional bilingual education as a superior instructional practice for improving the English language achievement of limited English proficient children.

Table 1 shows the effect of transitional bilingual education on second language (usually English) reading, language, and mathematics learning compared to a) "submersion," or doing nothing, b) ESL, c) structured immersion, and d) maintenance bilingual education. Table 2 shows the effect of TBE compared to "submersion" and ESL combined. Table 3 shows the effect of structured immersion compared to ESL pullout. (All of the studies in these tables are listed in Appendix C in abbreviated citation form in the same categories as in the tables.) Studies are repeated in more than one category of outcome if they had different outcomes at different grade levels or for different cohorts.[6]

The tables indicate the percentage of studies showing a program to be better than the alternative it is compared to, the percentage showing no difference, and the percentage showing the program to be worse than the compared alternative. This is repeated for each achievement outcome—reading, language, and math. The total number of studies assessing the particular achievement outcome for each category of comparisons are shown below the percentages.[7]

TBE v. Submersion. Table 1 indicates that for second language *reading*,[8] 22% of the studies show transitional bilingual education to be superior,

Table 1

Percentage of Methodologically Acceptable Studies Demonstrating Program Superiority, Equality, or Inferiority by Achievement Test Outcome

	READING*	LANGUAGE	MATH
TBE v. Submersion			
TBE Better	22%	7%	9%
No Difference	45%	29%	56%
TBE Worse	33%	64%	35%
Total N	60	14	34
TBE v. English as a Second Language			
TBE Better	0%	0%	25%
No Difference	71%	67%	50%
TBE Worse	29%	33%	25%
Total N	7	3	4
TBE v. Structured Immersion			
TBE Better	0%	0%	0%
No Difference	17%	100%	63%
TBE Worse	83%	0%	38%
Total N	12	1	8
TBE v. Maintenance Bilingual			
TBE Better	100%	0%	0%
Total N	1	0	0

*Oral English achievement for preschool programs.

33% show it to be inferior, and 45% show it to be no different from submersion—that is, doing nothing. Altogether, 78% of the studies show TBE to be no different from or worse than the supposedly discredited submersion technique.[9]

In a standardized achievement test of *language* (a test of a student's understanding of grammatical rules), transitional bilingual education ranks more poorly than it does in reading. Seven percent of the studies show transitional bilingual education to be superior, 64% show it to be inferior, and 29% show it to be no different from submersion—doing nothing. Altogether, 93% of the studies show TBE to be no different from or worse than doing nothing at all.[10]

These more negative findings for language than for reading suggest that a child is less dependent on school for many of the skills learned in reading—decoding, vocabulary, and understanding concepts—than they are for grammar. The fine rules of grammar, it appears, are learned

mostly in school, and because they are more complex, they are more influenced by school time-on-task. Thus, these results suggest there is a risk that bilingual education students will incur a deficit in English grammatical rules because they have spent less time on them than have LEP children in an all-English environment.

In *math,* 9% of the studies show TBE to be superior, 35% show it to be inferior, and 56% show it to be no different from TBE. Altogether 91% of the studies show it to be no different from or worse than the supposedly discredited submersion technique in developing math proficiency.[11]

TBE v. ESL. Although many so-called submersion situations probably have an English-as-a-Second-Language (ESL) program where the students are pulled out of the regular classroom and taught English in small groups for one period each day or a few times a week, it is generally not specified in the evaluations. Nevertheless, we suspect that many of the studies classified above as submersion may in fact include an ESL pullout component. In 7 studies, transitional bilingual education is specifically compared to reading achievement in the regular classroom with ESL pullout. None of these studies show TBE to be better than ESL pullout in reading. Five studies (71%) show no difference between transitional bilingual education and ESL in reading, and 2 studies (29%) show TBE to be worse than the regular classroom with ESL pullout. Of the 3 studies that examined language achievement, none showed TBE to be superior, 2 showed no difference between TBE and ESL, and 1 showed TBE to be worse. Of the 4 studies that examined mathematics, 1 showed TBE to be superior, 2 showed no difference, and 1 showed TBE to be worse.

TBE v. Structured Immersion. Table 1 also compares TBE to structured immersion. Most of these studies come from the Canadian immersion programs which come in several carefully documented types—early immersion (which means late bilingual), delayed immersion (which means early bilingual), dual immersion, and so forth. In many cases we had to "translate" the programs into United States terminology. Twelve studies had reading outcomes, 1 study had language outcomes, and 8 studies had mathematics outcomes. No studies showed TBE to be superior to structured immersion in reading, language, or math. In reading, 83% of the studies showed TBE to be worse than structured immersion and 17% showed no difference. In language, the 1 study showed no difference. In mathematics, studies showed no difference and 3 studies showed TBE to be worse than immersion.

All but 4 (Malherbe, 1946; Pena-Hughes & Solis, 1980; Ramirez and his associates, 1991; Ramos, Aguilar, & Sibayan, 1967) of the studies of structured immersion compared to TBE or ESL were conducted in Can-

ada. Most bilingual education advocates do not see the applicability of these studies to the United States. First, they argue that the studies are not relevant to the United States immigrant experience because the immersion and bilingual education students in Canada are middle class. In fact, however, the experiments were conducted with working class children as well and produced the same or better results (Bruck, Jakimak, & Tucker, 1971; Cziko, 1975; Genesee, 1976; Tucker, Lambert, & d'Anglejean, 1973). Both the middle class and working class English-speaking students who were immersed in French in kindergarten and grade one were almost the equal of native French-speaking students until the curriculum became bilingual in grade two, at which point their French ability declined and continued to decline as English was increased. The "time-on-task" principle—that is, the notion that the amount of time spent learning a subject is the greatest predictor of achievement in that subject—holds across classes in the Canadian programs.

A second argument made to dismiss the Canadian French immersion experiments as applicable to the United States is that the Canadian students were self-selected and their mother tongue was the dominant language of the country. The fact that the students were self-selected means that they were probably better language learners than other students, all other things being equal. Self-selected English language students taught bilingually after grade one were sometimes, but not always, the equal of the English controls because they heard English at home and in the rest of the non-school environment. Superior language learners hearing a language most of the time could sometimes, although not always (Popp, 1976), equal other students hearing it all of the time. Once the curriculum became bilingual, however, these students were never the equal of the French native speakers or of those English language students immersed completely in French. Thus, if self-selected, "elite" language learners have these problems, it is hard to imagine that ordinary LEP children in the United States would not encounter any of them. Accordingly, contrary to many interpretations of the Canadian experiments (Hernandez-Chavez, 1984; Popp, 1976; Swain, 1981; Tucker, 1980), we think there is much we can learn about second language learning from these experiments that is applicable to the United States experience, although clearly it is not a program that can be imported without major adaptation to the United States situation where immigrant children arrive at public school every day of the year, including the last day, and must be admitted regardless of their academic preparation.

TBE v. Maintenance Bilingual Education. The final category in Table 1 compares transitional bilingual education to maintenance bilingual education. This study (Medina & Escamilla, 1992) showed that transitional

bilingual education produced significantly higher English reading achievement than maintenance bilingual education.[12]

TBE v. Submersion/ESL. Because we suspect that many, if not most, of the so-called submersion alternatives had an ESL component, we show in Table 2 the outcomes for a category that combines submersion and ESL studies. Because of the small number of studies that specifically examine ESL pullout, there is virtually no difference in the findings—81% of the studies show TBE to be no different from or worse than submersion/ESL in reading, 94% show TBE to be no different from or worse than submersion/ESL in language, and 89% show TBE to be no different from or worse than submersion/ESL in math.

Structured Immersion v. ESL. There were also 3 studies that compared structured immersion to ESL specifically. These studies, shown in Table 3, all indicated structured immersion to be superior to ESL in reading.

Confronted with the kind of evidence presented in these tables, the advocates of bilingual education have sometimes contended that the issue is learning *in* a language, not learning a language. These data, however, do not show it to be superior in either learning a language or learning *in* a language—in this case, math. Moreover, there is no research evidence on the effects of TBE on learning other subjects such as geography, social studies, and history because standardized achievement tests are not given in these content areas.[13] Thus, any assertion regarding the superiority of TBE in these areas is anecdotal. Moreover, the math findings for TBE suggest an important problem: Subject matter is taught in the native tongue, but the student is tested on his or her understanding of that subject in *English.* It is possible that for many students the difficulty of having to translate what was learned in another language may

Table 2

Percentage of Methodologically Acceptable Studies Demonstrating Program Superiority, Equality, or Inferiority by Achievement Test Outcome

	READING*	LANGUAGE	MATH
TBE v. Submersion/ESL			
TBE Better	19%	6%	11%
No Difference	48%	35%	55%
TBE Worse	33%	59%	34%
Total N	67	17	38

*Oral English achievement for preschool programs.

Table 3

Percentage of Methodologically Acceptable Studies Demonstrating Program Superiority, Equality, or Inferiority by Achievement Test Outcome

	READING*	LANGUAGE	MATH
Structured Immersion v. ESL			
Immersion Better	100%	0%	0%
No Difference	0%	0%	0%
Total N	3	0	0

*Oral English achievement for preschool programs.

be great enough that the subject matter lost in the translation may equal or surpass what is lost in submersion before the second language is mastered enough to understand subject content. On the other hand, as discussed above, the solution is not to test LEP children in their native tongue because the goal of TBE is that students reach their potentially highest level in a subject in English.

Other Research Reviews

This review of the research is not the first to show a lack of superiority for transitional bilingual education. Baker and de Kanter (1981, 1983a, 1983b), Engle (1975), Epstein (1977), Holland (1986), Rossell and Ross (1986), and Rotberg (1982) have also concluded that there is no research support for transitional bilingual education.

Given this evidence, on what basis have some reviewers of bilingual education research claimed superiority for the program? One strategy, used by Zappert and Cruz (1977), is to simply redefine the word. As they argue:

> No significant difference should not be interpreted as a negative finding for bilingual education . . . When one adds the fact that students in bilingual education classrooms learn two languages, their native language and a second language, one can conclude that a statistically non-significant finding demonstrates the positive advantages of bilingual education. (p. 8)

The problem with this argument is that the court decisions, the federal regulations, and state laws are based on the assumption that TBE produces *greater* English language achievement and content area mastery, not the same achievement. Doing nothing is assumed to be a violation of a child's equal educational opportunity that transitional bilingual ed-

ucation will *remedy*, rather than a violation whose outcome TBE does not alter.

Another strategy used in some research reviews to make transitional bilingual education appear to be superior is to include performance in Spanish language arts. Zappert and Cruz (1977) also do this. Again, while we agree Spanish language arts are important, it is not the goal of government policy nor the stated object of the court decisions. If we examine the findings of the 12 studies reviewed by Zappert and Cruz for their effect on *English* language achievement, 63% of the findings show no difference between transitional bilingual education and doing nothing.

Willig (1985) conducted a meta-analysis of studies of bilingual education and concluded that transitional bilingual education was superior to all other alternatives. However, in order to do this, she excluded a large number of studies whose outcome measure could not be converted into an effect size (see also Endnote 7). In addition, she excluded all studies conducted in foreign countries, leaving only 16 studies for analysis, 2 of which had been rejected by Baker and deKanter (1981) as methodologically unsound. Finally, a careful reading of her results actually shows transitional bilingual education to be inferior to all-English instruction. It was only when she controlled for other variables, which partly eliminated the actual treatment effect, that TBE became superior.

There are similar problems with the conclusions of many of the research evaluations. A study by Leyba and his colleagues (1978) of Santa Fe, New Mexico exemplifies this. It begins by criticizing the AIR national study by Danoff and his associates (1977; 1978) because it failed to study their "successful" program and concluded that, contrary to the findings of AIR, Title VII bilingual education students in Santa Fe "showed over time increasing capability in English language skills, [and] . . . in the majority of cases outperformed the non-Title VII students in Reading and Mathematics" (p. ii). The study could be rejected simply because it failed to control for the lower pre-program achievement of several comparison groups of non-Title VII students. But even disregarding this problem, the data presented in the report show virtually no difference after four and five years of bilingual education between those students in the program and similar students not in the program, contrary to the author's conclusions.[14]

This study exemplifies a problem with studies of bilingual education. One cannot trust an author's conclusion to be an accurate representation of the data on which it is supposedly based. Moreover, this is as true of the studies done by supporters of bilingual education as it is of those done by its critics. This field is so ideologically charged that no one is

immune from ideological bias or preconceived notions. As a result, those attempting to make policy recommendations from the research must carefully read each study and draw their own conclusions. This does not guarantee that such conclusions will be free from bias, only that they will be free from someone else's bias.

Second Language Learning Theories

How does one make sense out of the conflicting findings of the research? If time-on-task is so important in learning, why are there methodologically sound studies that show transitional bilingual education to be superior to or no different from doing nothing? Even more perplexing, why are there methodologically sound studies that show TBE to be no different from structured immersion—called by Krashen (1985) "... the most successful language teaching program ever recorded in the professional language teaching literature" (p. 57)?

To answer these questions, we turn to two competing theories of learning a second language that lie at the center of the long debate over the value of transitional bilingual education programs. On one side of this debate are proponents of bilingual education programs who argue that children should be taught in their native tongue because there is a facilitating effect of the first language on second language learning. On the other side, critics of bilingual education programs put forth a time-on-task argument that the best way to learn English and subject matter in English is to maximize the time spent in English. Both of these competing theories have problems, although for different reasons, and there is empirical research that appears to contradict each. The facilitation effect theory is seriously flawed in its logic. The time-on-task theory, on the other hand, is limited by powerful constraints: a) school is not the only source of time-on-task; b) there are important psychological and pedagogical variables that mediate time-on-task such that it is *effective* or *engaged* time-on-task, not *simple* time-on-task, that influences achievement; and c) a corollary of b) for many if not most children is that there is a point in each day's instruction when there are diminishing marginal returns to further instruction in that subject. The problems with these theories are typically ignored, however, and virtually all bilingual education program evaluation studies are designed as a test of one or the other of these theories in their simplest forms.

The Facilitation Theory

Cummins (1978, 1981, 1985) is probably the principal proponent of the facilitation theory. Cummins' initial (1978) theoretical work was de-

signed to explain the conflicting findings in the empirical research. Although most studies had found initial instruction in the native tongue to be inferior to immersion in the second language, some found it to be superior. The facilitation theory that he hypothesized to explain this phenomenon had two components: a) the "threshold" hypothesis which states that the cognitive and academic benefits of bilingualism are mediated by the levels of competence attained in the native tongue and the second language—specifically that there is a threshold level of linguistic competence which a bilingual child must attain in order to avoid cognitive disadvantages[15]—and b) the "developmental interdependence" hypothesis that states that the development of skills in a second language is facilitated by skills already developed in the first language.

According to the first part of the facilitation theory, if bilingually taught children reach the threshold in their native language, they will attain a higher level in the second language than will students taught entirely in the second language. On the other hand, if bilingually taught children have not reached the threshold in the native tongue, they will be inferior in the second language (and the native tongue) to students taught entirely in the second language. Thus, the conflicting research on TBE was accounted for by whether or not the native tongue threshold had been reached by the bilingual learners. Studies that found all second language instruction superior to transitional bilingual education, it was conjectured, must have had students in the bilingual education program who had not reached the threshold in the native tongue, while studies where bilingual education was more effective than all second language instruction must have had students in the bilingual education program who had reached the threshold. An implication of this theory is that it takes considerable time before the threshold in the native tongue is attained and the facilitation effect is manifested. Thus, the longer a student stays in transitional bilingual education, at least up to seven years, the better they will do in the second language because they have surpassed the threshold in the native tongue.

The second part of the facilitation theory claims that once a child has learned to read in his or her native language, learning literacy in the second language is facilitated because he or she has mastered the "mechanics"—the hardest part—in the easiest language and these mechanics will allow him or her to more easily acquire the second language. Because it takes 3–4 years to acquire literacy in Roman alphabet languages,[16] the facilitating effect will not become apparent until 5–7 years after literacy instruction begins.

It is important to understand, however, that bilingual education programs in the United States (or in Canada) were not designed in response

to the facilitation theory. Indeed, it was the other way around. Although bilingual education programs officially began in 1968 with Title VII of the Secondary and Elementary Education Act, Title VII in fact was created in response to a civil rights movement whose leaders, upon observing that Spanish-speaking children on average had lower achievement than white children, concluded that the cause of this must be the then current practice of placing Spanish-speaking LEP children in all-English regular classrooms and "forcing" them to give up their native tongue.

Bilingual education programs thus preceded the facilitation theory by more than a decade.[17] Indeed, the dominant component of second language learning theory in 1968 seems to have been the time-on-task principle. Izzo (1981) summarized studies conducted in the late 60s and early 70s:

> The length of time spent in language study is, in fact, one of the most important factors in achievement . . . [Moreover] it must be the total length of time spent in contact with the language that is of importance in determining second language proficiency. (pp. 51-52)

Carroll (1976) went even further in summarizing the Canadian research evaluations as "eloquent confirmation of the statement that time is the most important factor in learning [a second language]" (p. 235). These conclusions and theories dictated the practice of all-English instruction in the 1960s. Because all-English instruction had not eliminated the achievement differences between Spanish-speaking and white children when the civil rights movement reached its peak in the late 1960s, it had to be replaced by its opposite—native tongue instruction—which, it was argued, would raise the self-esteem and motivation of Spanish-speaking LEP children and ultimately their achievement. The facilitation hypothesis then appeared after the fact through the work of Cummins and others as an educational or linguistic justification for a policy already implemented on civil rights grounds (see Rossell & Ross, 1986, for a discussion of the legal history).

Research and the Facilitation Hypothesis

Is the facilitation theory a valid theory? Much of the evidence Cummins cites to demonstrate its validity is either trivial—a study by Cummins and Mulcahy (1977) showing that fluent bilingual Ukrainians did better than either non-fluent bilingual Ukrainians or monolingual students on a test of ambiguities in sentence structure,[18] and a study by Leslie (1977) showing that Indian children who scored high on *oral* Cree scored high on English reading—or just plain contrary—for example, a study by Hebert (1976) and another by Ramirez and Politzer (1975) showing that

instruction in the native tongue has no effect on achievement in the second language.[19]

The principal evidence Cummins cites for the effectiveness of transitional bilingual education programs, however, is from the study by Skutnabb-Kangas and Toukomaa (1976), which compared two groups of students who had immigrated from Finland to Sweden, one group that immigrated before and another that immigrated after reaching the third grade. Students who immigrated after third grade, that is, those students who had been in school in Finland long enough to have first learned literacy in their native language (Finnish), supposedly performed better in Swedish than did the children who had moved to Sweden at a younger age and who presumably had begun learning Swedish at an earlier age. Both Cummins and Skutnabb-Kangas and Toukomaa argue that the greater ability of these older students in Swedish is a function of more years of instruction in Finnish.

There are major methodological problems with the study by Skutnabb-Kangas and Toukomaa, however, and with the inference that their results support the facilitation hypothesis (see Baker & de Kanter, 1981). First, Skutnabb-Kangas and Toukomaa presented no statistical analysis of their data. Second, at the time of their study, Swedish, the second official language of Finland, was a required subject in Finnish schools from the third grade on, a fact which neither Skutnabb-Kangas and Toukomaa nor Cummins mention. Thus, if the simple descriptive data presented by Skutnabb-Kangas and Toukomaa show anything, it is that students who have a chance to study a second language before immigrating to the country of that language perform better in it than do students who have had no formal instruction in it before they immigrated. In short, contrary to Cummins' assertions, there is no empirical support in Skutnabb-Kangas and Toukomaa for the facilitation hypothesis and some support for the time-on-task principle.

There are other studies that are sometimes misinterpreted as support for the facilitation hypothesis. This research shows that a) children can transfer skills learned in one language to another language (Barik & Swain, 1975; Bruck, Lambert, & Tucker, 1977; Lambert & Tucker, 1972) and b) older children are, contrary to popular belief, more "efficient" (i.e., faster) learners of languages (Ausubel, 1964; Eckstrand, 1975; Ervin-Tripp, 1974; Ramirez & Politzer, 1978; Stern, Burstall, & Harley, 1975; Swain, 1981; Taylor, 1974). Hence, when it is observed that older learners who already knew how to read in their native tongue acquired a second language faster than younger learners, some individuals have interpreted this as support for the facilitation effect (see, for example, Rosier & Farella, 1976; Rosier & Holm, 1980; Skutnabb-Kangas & Toukomaa, 1976).

When the proper analysis is conducted, however, the most important causal variable turns out to be age, not native tongue reading ability (see Izzo, 1981). Older learners are more efficient in learning *anything* (except accent) than are younger learners.

Moreover, even though it is true that it is easier to teach a second language to individuals who are literate in their native tongue, this tells us nothing about how non-literate individuals should be taught, nor the language in which they should be taught. It is probably also true that a person who has been unable to learn to ride a bike is a harder person to teach to ski, but this does not necessarily mean that the best way to teach a non-bike rider how to ski is to spend years teaching them how to ride a bike. The bilingual education literature, however, is rife with such unwarranted inferential leaps.

Collier (1987a, 1987b) has conducted one of the few studies that directly attempts to test Cummins' hypotheses and, as with many studies of bilingual education, her data contradict the theory she purports to have proved. Because it is widely cited as support for the facilitation theory, however, it is worth discussing here, although we classify it as methodologically unacceptable (see Appendix B). Collier tested Cummins' hypothesis that there is a facilitation effect of the native tongue on the second language with 20 pseudo-learning curves derived from cross-sectional achievement data (incorrectly described as "rates" of learning)[20] from students who had been in the United States for varying amounts of time. If the facilitation hypothesis is correct, these curves should be negatively accelerated—that is, the shorter the length of residence in the United States for students 8 years or older, the greater the gains in English. Of the 20 curves, however, only 2 (or perhaps 3) clearly show negative acceleration. About 8 curves show *positive* acceleration. If one simplifies the problem of interpreting the curves by asking only whether the two end points of the curve show negative acceleration, there are 8 that support the hypothesis and 12 that contradict it.

In addition, there is a second way in which Collier's results contradict Cummins' theory. Collier claimed evidence of the facilitation effect for children aged 8–11 years because she could not find it in children aged 12–16, which is where Cummins says it occurs. Thus, one of the few researchers to directly test Cummins' theory finds contrary evidence, but because of the importance of the facilitation effect for transitional bilingual education, urges us to discount her findings instead.

Several other researchers have directly or indirectly tested the facilitation effect. The Eastman Project (Krashen & Biber, 1988) is perhaps the major effort to demonstrate the facilitation hypothesis. Although Krashen and Biber claim the results of their study support the facilitation hy-

pothesis, their analysis is so severely flawed that no conclusions can be drawn from the study (see Baker, 1990, for a critique of Krashen & Biber).

In addition to the lack of empirical support for the facilitation hypothesis, it is a poorly cast theory. There is no underlying psychological mechanism that accounts for the facilitation effect. Rather than being deduced from well established mental processes, the facilitation effect has to be accepted as a fundamental characteristic of the brain itself. At least parts of it, if not the whole hypothesis, are untestable. It can be determined if the native tongue-second language sequence is superior to simultaneous teaching of the native tongue and the second language or to the second language alone, but if it should be shown that the native tongue-second language sequence is superior, it is impossible to say why. That is, exactly what knowledge transfers from the native tongue to the second language that cannot be learned simply from the second language learning process alone? In short, we simply do not know what particular mental processes or items of literacy-related knowledge possessed by someone who learns to read in his native tongue enable him or her to surpass the child who learns to read or write in the second language.

This is an important methodological point which we encounter elsewhere in other forms. An overly obvious example should make it clear. Imagine a study that set out to identify the characteristics of a good teacher and found that *all* good teachers erase the blackboard (this is not just imaginary; all good teachers do erase the black board). It does not follow from this confirmed empirical fact that all teachers who erase the blackboard are good teachers, and therefore the way to improve achievement is to direct all teachers to spend all day erasing the blackboard. Likewise, if the native tongue-second language sequence is superior, nothing follows about what particular components of the native tongue accounted for the superiority of that sequence and there is nothing in the literature that tells us. Indeed, the drawings that often accompany Cummins' articles show empty heads with no more than two descriptive labels and no discussion whatsoever of any mental processes involved in the theory he advocates.

Perhaps the most important recent test of Cummins' facilitation theory is the national study by Ramirez and his associates (1991). Unlike Collier (1987a, 1987b), this is a methodologically acceptable study (see Appendices A and C) with a large national sample of 1,054 students. Although not much discussed in the final report, its design was specifically structured to test Cummins' facilitation theory.[21] The study sampled early-exit TBE classrooms, immersion classrooms (all-English special instruction), and late-exit bilingual (maintenance) classrooms across the United States. Only the early-exit and immersion classrooms were di-

rectly compared to each other in statistical analysis. Learning curves over time were made a central part of the analysis to test both Cummins' theory and its alternative, the time-on-task theory that predicts immediate and continuing superiority for all-English programs.

In the early-exit and immersion program comparisons, Ramirez and his associates found a significant effect over two years (kindergarten to grade one) favoring early exit bilingual education programs in reading, but not in language or math where there was no difference between programs. This advantage of bilingual instruction, however, had vanished by the end of four years of schooling. Now structured immersion was favored in language arts, but not in math or reading where there was no significant difference between programs.

One of the greatest failings of this study is that it did not statistically compare the late-exit programs directly to the early-exit and immersion programs. It tried to do so indirectly with simple descriptive line graphs (TAMP curves).[22] But the theoretical framework of the study—a comparison of the facilitation and time-on-task hypotheses—led Ramirez and his associates to misinterpret the merely descriptive line graphs and to overlook a finding with stronger empirical support which fits neither theory. Ramirez and his associates concluded that late-exit programs have a more positive effect than early-exit or immersion programs because they have a positive curvature over time; that is, the rate of gain increases from year to year. However, the report's conclusion that this difference in curvature represents an advantage for late-exit programs does not necessarily follow. Late-exit students may be playing catch-up. They appear to have fallen well behind their immersion and early-exit peers through the third grade, and it is indeed fortunate for them that they do begin to catch up later on. Unfortunately, the available data are not sufficient for determining if they ever actually overcome the handicap that they appear to have acquired during the first three years of the late-exit program.

While the results of the study by Ramirez and his colleagues should be interpreted with great caution,[23] the fact that the early-exit program did as well as it did in comparison to immersion suggests the following possibility: Bilingual education may be superior to all-English instruction in the very beginning when a student literally knows no English, but as the student's English language knowledge increases and English becomes more comprehensible, time-on-task in English becomes more important because it becomes *effective* time-on-task. On the other hand, their study shows *no* support for the facilitation effect because, contrary to their claims, its descriptive portion—the TAMP curves—shows that students who stayed in bilingual education the longest did the worst. While

this apparent negative finding may not be real given the lack of statistical control for student and classroom characteristics, it is definitively *not* positive evidence.

The study by Burkheimer and his associates (1989), also funded by the Department of Education and critiqued by Meyer and Feinberg (1992), shows similar findings. It too is a methodologically acceptable study (see Appendices A and C) with a large national sample of more than 8,000 students. Not only is there no facilitation effect, but the only positive effect for bilingual education in English language arts and math was found in the very beginning of a student's English language acquisition. Although overall, more instruction in English and less in Spanish language arts was positively related to increased English language arts achievement over a one-year period, this relationship varied by initial English proficiency. First graders whose proficiency in English was sufficiently high (not defined) had an increase in achievement when there was more instruction in English. Those who were less proficient in English initially had greater end of year achievement with more hours of native language arts instruction and more English language arts in the native language, i.e. using the native tongue for assistance in English instruction (p. 5.43).

Overall, more instruction in English was associated with higher math achievement, but this effect was greatest for those who had higher math scores on the math pretest. In the second grade, however, children had higher math achievement if they received most of their math in Spanish, *provided they were given sufficient instruction in English language arts.* If mathematics was taught primarily in Spanish, everything else needed to be taught in English in order for the child to do well in math. This was not true for third grade children, however. For these children, the study found an "unqualified positive contribution from greater instruction in English language." The study concluded, "For success in mathematics, there is an increased requirement for proficiency in English" (p. 6.27). This is, however, contrary to the facilitation theory.

Instruction in ethnic heritage *decreased* both the overall effect of more English language arts and the relative advantage for higher pre-achievers. This is because, as the study notes (but which is seldom acknowledged by others)

> . . . within a framework that constrains total instructional hours and is further constrained by legislated requirements for some courses, increased instruction in one particular subject area is typically accomplished at the expense of reduction in another subject area . . . As examples: maintenance of the child's native language skills is accomplished at the expense of reduction in another area; more hours devoted to Ethnic Heritage instruction

> reduced the remaining hours that could be devoted to other subjects. This reality is not a value judgment of what should or should not be taught (which is best determined by local conditions and goals), but simply a recognition that trade-offs are required. (pp. 5.42–43)

Finally, this study found a seemingly contradictory finding: having a bilingual certificated teacher had a *negative* effect on English language arts achievement for the first grade cohort, but familiarity with the child's native language had a *positive* effect. It is not clear what this means, but if we assume that being bilingual certified means a more advanced knowledge of the native tongue than not being bilingual certified, it may mean that some native tongue ability is good, but too much is not. This hypothesis is suggested by other research as well. Fillmore (1980), for example, examined different kinds of bilingual education classes and found that the teacher who was most successful in raising the English language achievement of her Chinese students knew the children's native tongue, but taught 90% of the time in English. Similarly, the Austin Independent School District's TBE program (Carsrud & Curtis, 1980) found to be superior to submersion had teachers who used English as the medium of instruction 82% of the time.

Tickunoff's (1983) descriptive study of successful bilingual instruction (58 teachers from 6 nationally representative sites) identified the following characteristics of successful programs: a) 80% of time allocated to academic learning tasks, b) the native tongue used by teachers primarily to clarify instructions, and c) content areas such as mathematics and social studies taught in *English*. Two studies of the achievement gains of LEP children taught by bilingual and monolingual teachers (Curtis, 1984; Ligon, 1974) found no difference between the two. Similarly, the AIR national survey of bilingual education by Danoff and his colleagues (1977; 1978) also found no relationship between whether a teacher was bilingual and the performance of his/her students. Rossell (1990) found no difference in achievement between whether a teacher was bilingual certified and his or her student's achievement.

Even more amazing, Moore and Parr (1978) found that teachers in the bilingual education program who were rated as *less* competent had better student performance. This finding is not as strange as it sounds if, as seems likely for a bilingual education program, the competence rating is primarily an evaluation of the teacher's bilingual ability. What all these studies suggest is that the psychological and perhaps initial pedagogical advantage one may gain from having a fluent bilingual, same ethnic group teacher may be offset by the tendency of such teachers to teach too much and too long in the native tongue—in other words, to teach according to the facilitation theory.

English Time-on-Task

Opponents of bilingual education programs argue that learning English is determined almost entirely by the time spent studying English (see Porter, 1990a; 1990b, for the most recent example of this). This theory proposes that bilingual education programs are inferior to all English instruction because bilingual education programs reduce the time spent on the task of learning English.

The time-on-task hypothesis, like the facilitation theory, has a major problem in its failure to address the inconsistent research results. If time-on-task were as important as its proponents suggest, all-English instruction would always be superior to any form of bilingual education, even programs where the native tongue is used only in small amounts, and monolingual English teachers would consistently produce greater achievement in their students than bilingual teachers. The methodologically sound studies cited above indicate, however, that this is not the case.

Any acceptable theory of teaching English to LEPs must account for the contradictory research evidence, as Cummins tried, but failed to do, with bilingual education. Why is it that bilingual education programs with up to 30% native tongue instruction often do no worse than and sometimes do better than all-English programs? Why is it that monolingual teachers do no better than bilingual teachers?

The problem with the time-on-task theory is that it fails to take into account important mediating variables. Although researchers such as Clauset and Gaynor (1980), Rosenshine (1979), and Wiley (1976) find time-on-task to be the single greatest predictor of achievement in a subject, it is nevertheless only one of many instructional factors such as classroom atmosphere, pace of instruction, and curriculum content that influence academic achievement. Karweit (1983), in a review of the time-on-task literature, concluded that the time-on-task effect, while significant, had been greatly overblown in importance. In reanalyzing the Beginning Teacher Evaluation Study by Fisher and his colleagues (1980), Karweit estimates that an additional 60 minutes per day in time allocated to reading comprehension alone would be needed to increase reading comprehension scores by .25 standard deviations. If that were *engaged* time-on-task, however, only a 10 minute increase would be required to increase mathematics achievement by .25 standard deviations.

As Rossell and Ross (1986) suggested almost a decade ago, there are some possible mediating factors for time-on-task that explain why many methodologically sound research studies show TBE with less English language time-on-task to be no different from or superior to submersion with more English language time-on-task. The first of these factors is the

nature of the time spent in an English language environment in each alternative. Because much of the learning in a submersion situation is, at least initially, not *effective* learning because the students do not understand what is going on, a bilingual program which gives the children half of their education in English, but structures the English so that it is understandable, may provide more effective time in the English language in the beginning than a program which is completely in English, but only a small part of it is comprehensible. As English becomes more understandable over time, the greater time spent on English in the submersion situation then would give these children an advantage (*see also* Krashen, 1980; Long, 1981; Swain, 1983). At the end of three years, students in both the submersion situation and bilingual education may end up with the same amount of effective learning time in the English language with TBE producing more at the beginning and submersion more at the end.

A second factor is that the supporters of bilingual education may be right—bilingual education may have important psychological effects which compensate for the reduced English language learning time. That is to say, if students in submersion programs often feel alienated or inferior, and if a special program regardless of its educational utility makes school more enjoyable, then they may come to school more often and stay longer. If in the submersion situation, they were taught 100% in English, but only came to school 75% of the time and only half of that was comprehensible in the first year, they will have less effective English language learning time for that year than if they had been in a bilingual program which taught them 50% in comprehensible English, but motivated them to come to school 85% of the time.

Porter (1990a), by contrast, appears to acknowledge none of these factors but asserts simply that "education research, we have seen, makes the compelling case for time-on-task as the most important single determinant of success in learning *anything*" [emphasis in the original] (p. 125). Porter provides two justifications for her sole reliance on the time-on-task principle. The first is her own experience running a mostly English "bilingual" program in Newton, MA. But her assertion that the Newton program is superior to TBE is unsubstantiated by empirical evidence such as test scores, dropout rates, or other school success indicators. Indeed Porter offers only her personal opinion that she is right and that the bilingual education advocates are wrong, resting her claim solely on the validity of the time-on-task principle.

Porter's second justification for her assertion that "the more time spent learning a language, the better you do in it" is the results of the Canadian immersion studies. It is true that these studies show an extraordinarily

strong relationship between time-on-task and achievement as shown in Figure 1. However, it is only because the Canadian immersion schools are the sole source of French for the English-dominant students who attend them that the relationship is as strong as it is. We would not expect the relationship between French instruction in school and French achievement to be this strong if French were spoken outside the school. The relationship between the extent of English instruction and English language achievement, for example, is not going to be as strong in the United States (nor was it in Canada) because students hear and speak English outside the classroom. Because we cannot control for out of school time-on-task, the school time-on-task relationship for English instruction in the United States will be attenuated in comparison to French instruction in the Canadian studies.

Rossell's (1990) comparison in Berkeley, CA of TBE and all-English regular classroom instruction with ESL pull-out has some interesting findings that are relevant to this discussion. This study is a methodologically acceptable study of one school district (see Appendices A and C). Rossell found no difference in student achievement between TBE and ESL in the first year's analysis. Interviews with teachers indicated that they used Spanish in the TBE program about 30–50% of the time in kindergarten and first grade and not much after that except for individ-

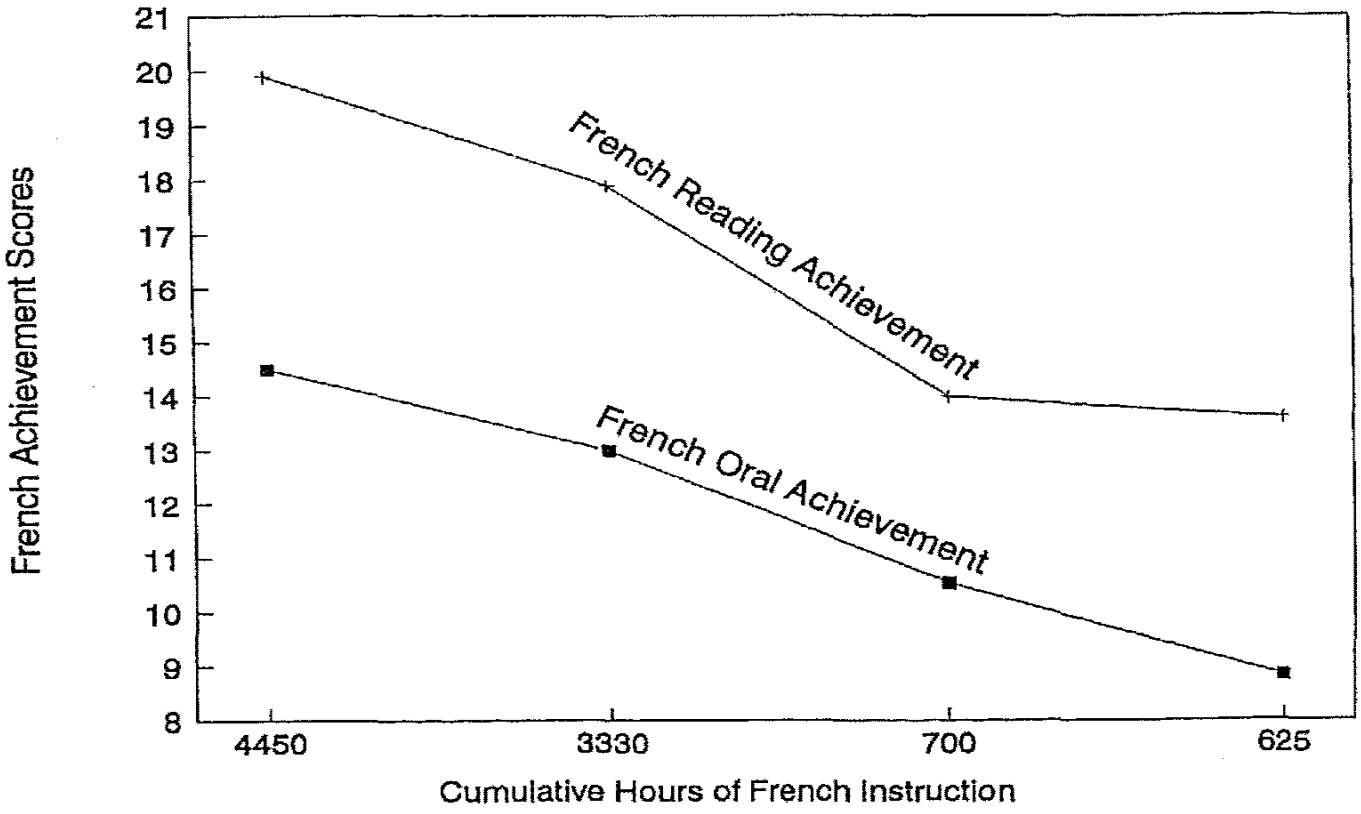

Figure 1. French Achievement by Instruction Hours (Swain & Laplan, 1982)

ualized instruction with new non-English-speaking (NEP) students who entered in the later grades. In addition, the one bilingual program in Berkeley that outperformed all the other school programs, including regular classroom ESL pull-out programs, was specifically cited in the original court complaint as using too much English in its bilingual program. But contrary to what would be predicted from a strict time-on-task theory, it did have some native tongue instruction.

The second year evaluation Rossell (1990) conducted was after the Berkeley Unified School District had increased the use of Spanish in its bilingual program in response to pressure from the California State Department of Education. The analysis of the second year found the bilingual program to be worse than all-English instruction by about 12–15 points in reading, language, and math. The first year findings, in comparison to the second year's, suggest some possible theories: a) There is some threshold below which native tongue instruction does not harm children, or b) there is some initial period of time when native tongue instruction actually benefits students. We do not have enough evidence at the moment to confirm or rule out either possibility, but we can theorize about what is at work here.

A well established principle in learning theory is the differential effects of massed versus spaced learning trials. One classic demonstration of the difference is Duncan (1951), who studied the acquisition of the skill of keeping a pen point on a moving target. One group of subjects practiced for the entire learning time while the other group was periodically interrupted for rest periods so that they were resting for two thirds of the practice session. The group with less practice and more rest actually learned better.

Kientzle (1946) found that printing upside down was learned better with a rest of seven days between trials than with continuous practice or with as much as a 20 second rest between trials. Lyon (1917) looked at the average length of time it took to memorize a nonsense syllable in lists of different lengths comparing continuous practice and once a day practice. For a list of 40 nonsense syllables, the average learning time per syllable was four times longer for continuous practice than when the list was practiced just once a day.

A probable explanation for the superiority of spaced practice over continuous practice is that it takes time for the memory process to work. A constant barrage of material to learn overloads the memory process and interferes with learning. Rest, or doing something else, between practice sessions gives the memory process the time it needs to operate, thus resulting in more efficient learning results.

Although learning a language is not exactly identical to learning a boring, repetitive task, there is enough similarity to suggest what might

go on in language learning. Consider language learning for the school aged, English monolingual child. The child already knows most of the words the teacher uses on any particular day. The few new words to be learned are interspersed with periods of no learning of new words—that is, rest. Therefore, language development in the child takes place in a setting of spaced, not continuous, practice.

However, the situation is initially quite different for the child learning a second language. Because all words are new at first, the situation is one of continuous learning. The student may actually make more progress learning the second language if rest is introduced into the constant stream of exposure to the unknown language.

How can rest occur in exposure to a new language? One way, although by no means the only way, is to change the language of instruction to one the child already knows. Eventually, enough of the second language will be learned if it is part of the instructional program[24] so that the second language learner will be able to get some "rest" between new words in an all-English instruction.

Thus, it is possible that bilingual education programs, because they provide a needed rest from constant exposure to the new language, can produce better learning at the early stages of learning a second language. Later on, however, instruction entirely in the second language probably works better than bilingual education when English is comprehensible enough so that new words are a minority.

Implications

The research we have reviewed is limited and, as suggested by the number of studies in Appendix B, most of it is methodologically unsound. The implications of this finding are that additional, methodologically sound research needs to be conducted in order for the courts and policymakers to make intelligent decisions. School districts should be required to keep the kind of data on their programs for LEP children that would allow social scientists to analyze variations in programs. Not all bilingual education programs use the same amount of instructional time in English, nor do they keep students in the program for the same amount of time. Many programs called transitional bilingual education are closer to the structured immersion model, while others are closer to a maintenance program. If program administrators were required to keep this kind of program information, social scientists both local and national could evaluate these variations. The federal and state governments, however, must fund more high quality research by enlisting the aid of nationally respected social sci-

entists in designing Requests for Proposals and evaluating proposals. The quality of research in this field is highly inadequate, a situation that should not be allowed to continue given the increasingly competitive economic environment into which American young people graduate. Assuming that the goal of bilingual education in the United States is to make LEP students successful in English and in an English language environment,[25] we suggest two new hypotheses to be tested by research:

1) Native tongue instruction should be minimal and used only in the beginning when a LEP student's English language knowledge is very low. The time period for the superiority of or need for native tongue instruction may be a matter of *months;*

2) Teachers who are familiar with, but not fluent in, the child's native tongue are better teachers of LEP students than are fluent native tongue speakers because the former will emphasize transition to English, and the latter will emphasize maintenance of the native tongue and development of the native tongue language arts which ultimately will work to the detriment of academic achievement in English.

We must emphasize that these are hypotheses, not conclusions. There is not enough reliable or consistent evidence to generate anything but hypotheses at the present time. If these are accepted as viable hypotheses, however, researchers would have to design very different research studies from the ones we have seen in the past. A research study to test these hypotheses would look something like this:

1. *Treatment Groups*: Truly LEP[26] students in classrooms categorized not by nominal program type but by the amount of English language instruction, by subject matter, and by the varying credentials and native tongue ability of the teachers and aides;

2. *Covariates*: Student background characteristics, including use of English at home and neighborhood, intelligence (nonverbal such as Raven's Progressive Matrices), and prior English language ability level and program.

3. *Outcome variables*: Academic achievement by subject matter (as well as attitudes, drop out rates, etc.) measured every *three months* during the academic year, not simply at the beginning and end of the year,

It is important that outcome variables be measured in months, not years. As we have suggested, English may become sufficiently compre-

hensible to most or many LEP students sometime during the first year to make the positive time-on-task in English in the all-English classroom a better situation for the latter, but not the first, part of the year.

One of the many serious limitations of the research on bilingual education, and the programs themselves, however, is that no one looks at the future educational success of graduates of bilingual or immersion programs, as well as their life chances. Thus, this review, because it relies on this research, suffers from these defects as well.

It is quite possible, for example, that *maintenance* bilingual education, that is, bilingual education for an entire school career, reduces English language achievement in comparison to educating a child in the regular English language classroom or structured immersion but increases life chances for these students. This is because it might better maintain an adequate ability in the native tongue, which might result in greater economic gains in later life than would be predicted from the English language achievement of these students.

Even if this does not increase one's life chances in any material way, it certainly ought to do so in an intellectual sense. Although there are problems, particularly self-selection biases, with the analyses that conclude bilingualism increases cognitive development (Cummins, 1978; Kessler & Quinn, 1980; Lambert, 1978), at the very least, being able to converse in another language is a valuable skill. Rather than viewing limited English proficient children as a burden, we ought to view them as an opportunity to develop bilingual adults.

There are two elements of consumer protection, however, which must be considered if we are to adequately protect the rights of limited English proficient children. First, not every LEP child will want to, or be able to, maintain their mother tongue. Nor should they be forced to do so by the courts and other policymakers. Second, children and their parents must be advised as to the probable consequences of being raised bilingually within the constraints of the normal school day. Such children will be like decathlon athletes, and it is false advertising to promise them they will not only win the decathlon, but also come in first in each individual event against those who specialize in it. Unfortunately, that is what bilingual education advocates have been promising, primarily because that is what the courts and other policymakers have been demanding.

Obviously, then, bilingual maintenance programs will be most successful in increasing life chances, if, as with the year-round transitional bilingual education program studied by McConnell (1980), time in both languages is increased by either increasing the length of the normal school day and school year or by cutting out non-academic subjects. In the latter case, such students would become academic specialists.

Of course, LEP students in immersion programs could also benefit from an extended school day or year. There is no reason why schools should not offer "compensatory" education that truly compensates children for the time they have lost. If children are deficient in English, they should have more, rather than the same amount of or less, English language instruction. If English fluent children attend school for five hours, then LEP children should attend it for six or seven. That is truly a remedy which might come close to producing a level playing field for LEP children.

If we are ever going to come to a conclusion on this issue, however, we will need the courage and cooperation of school administrators in not only agreeing to studies such as the one described above, but to *random* assignment to alternative language instruction programs, thus eliminating the need for a pretest—one of the biggest problems of research in this field. Given the inconsistency in the research, it seems to us that we have reached the point where it would be ethical to randomly assign students to alternative treatments. In fact, we believe that the ethical position is to determine what is in the best interest of LEP students. Random assignment would go a long way toward producing a research study that could do this.

Conclusions

More than 15 years of research and literally thousands of studies since Cummins first proposed the facilitation theory have confirmed neither the theory nor the predicted effectiveness of bilingual education programs. Unfortunately, most recent methodologically sound research project, the Ramirez study (1991), cost millions of dollars and made only a small contribution to our understanding of this issue.

The hundreds of ethnographic studies of bilingual classrooms tell us a great deal about what happens in these classrooms, as have our own classroom observations, but they unfortunately cannot tell us whether the treatment we are observing is more or less effective than an alternative since there is no attempt to link the classroom process to educational outcomes, such as academic achievement, in alternative settings. Thus, although we learn much from these studies, they are excluded from this review because they cannot answer the question we are interested in: Is bilingual education more or less effective than doing nothing, ESL pull-out, or structured immersion in promoting academic achievement?

Thus, the research continues to be as inconsistent as it was when Cummins in 1978 first tried to explain its inconsistencies. Nevertheless, the facilitation hypothesis has been overwhelmingly accepted by bilingual

educators as a proven fact and as the reason why TBE is superior to all other forms of second language acquisition approaches, despite the lack of evidence that superior ability in the second language results from reaching a certain threshold level of ability in the native tongue. Indeed, the Ramirez and the Burkheimer studies suggest to us that the threshold theory may work the reverse of the way that Cummins hypothesizes it works. It seems more likely that a threshold in the second language, not the native tongue, needs to be passed before the second language instruction is consistently superior to native tongue instruction. Indeed, native tongue skills (after controlling for intelligence, something almost no study does) could be *irrelevant* to this process.

Our discussion of these issues has been limited to the American educational system in which all-English regular instruction with ESL pull-out is the alternative to transitional bilingual education. We believe that the evidence to date indicates that the best program is "structured immersion" modeled after the Canadian immersion programs where the entire classroom consists of LEP students, the pace of instruction is structured to their level, and instructors teach completely in the second language, although they know the child's native tongue.

Nevertheless, it cannot be emphasized enough that the research clearly shows, as with all other educational interventions, that the intervention itself is only one of many important factors explaining achievement. Indeed, the most important factors in a child's acquisition of English and other subjects are the child's family characteristics, his or her intelligence, the characteristics of his or her classmates, and the intelligence and talent of his or her teacher. For most students, at least in an educational system in which all programs provide substantial amounts of English, the exact percentage of each language has, on average, explained only a small portion of the variance in achievement (see the study by Burkheimer and his associates, 1989). Even in the worst cases, we are struck by how small the differences in academic achievement are—a maximum of about 15 points—between programs with very different amounts of English instruction. For any single student, however, there could be serious consequences to having little English instruction. As Table 1 indicates, substantially more studies show harm from TBE, compared to all-English instruction, than show a benefit. This disparity widens when TBE is compared to structured immersion, an all-English program in a self-contained classroom for second language learners. Thus, the *risk* of academic deficiency in English is greater for TBE than for all-English instruction according to the empirical comparative research.

We do not claim that this review will end debate on the question of whether transitional bilingual education is the most effective instruc-

tional alternative, given the limitations of the studies we have summarized. We have a more modest claim than this. We believe our study will keep the controversy alive because we have shown that the case for transitional bilingual education is not based on the soundly derived research evidence that its supporters claim.

Endnotes

1. This article is a preliminary version of a chapter in the book *Bilingual Education Reform in Massachusetts*, forthcoming, by Christine Rossell and Keith Baker, to be published by the Pioneer Institute in Boston.
2. For a discussion of what goes on in the classroom, see Chapter 2 of *Bilingual Education Reform in Massachusetts.*
3. The study by Ramirez and his associates (1991) is the only large scale study of which we are aware that has both carried out an ethnographic analysis of a large number of classrooms and measured educational outcomes. Unfortunately, the two are not linked in any way.
4. In *Bilingual Education Reform in Massachusetts*, we also assess the effect of alternative programs on grade point average, dropout rates, attendance rates, retention rates, suspension rates, self-concept, and self-esteem.
5. The initial list of studies on bilingual education was obtained from a search of the Educational Research Information Clearinghouse (ERIC) documents, card catalogues in the Boston Public Library and the libraries at Boston College, Boston University, and the Massachusetts Institute of Technology, Language and Language Behavior Abstracts, and the bibliographies of other reviews of the literature. The studies actually reviewed were those that could be obtained from 1) ERIC; 2) University Microfilms International; 3) the journal and book holdings of the libraries at Boston University, Massachusetts Institute of Technology, and Boston College, and of the Boston Public Library; 4) the National Clearinghouse on Bilingual Education; 5) the Center for Applied Linguistics; 6) the Department of Education, 7) the authors themselves; 8) inter-library loan; and 9) program evaluations for 1991-93 obtained by writing to school districts in the United States. Not all studies are documented, nor could all documented studies be obtained.
6. A *cohort* is a group of students that is followed across grades as they progress through school. Thus, a group of students who started kindergarten in 1960 and were graduated from high school in 1974 would be one cohort. A second cohort might be a group of students who started kindergarten in 1961 and were graduated from high school in 1975.
7. We use the "voting method" to evaluate the literature's findings. In the voting method, the percentage of studies showing each program's outcome is calculated. Another, increasingly common method is meta-analysis—a statistical analysis of the effects of bilingual education across all studies. Almost all attempts at meta-analysis of these studies that we are aware of have failed, however, because there are too few programmatic data available in the bilingual education reports to estimate a common outcome measure (see Okada, 1983). The one apparent exception is Willig (1985), which purports to be a reanalysis of Baker and de Kanter (1981, 1983a, 1983b). However, a group of experts in meta-analysis headed by Richard Light at the Harvard Graduate School of Education had already attempted to do such an analysis

of the Baker and de Kanter sample in 1983 and reported that it was impossible to calculate a common measure of effect given the lack of data in the studies (personal communication with second author, 1984). Willig was apparently able to do a meta-analysis by analyzing only 13 of the 39 acceptable studies of transitional bilingual education in Baker and de Kanter and by treating all effect sizes in every study equally so that one study (Cohen, 1975) with only 90 children in it produced 38% of the outcomes analyzed. She also excluded all studies conducted outside the U. S. and thus had only one study of structured immersion—the Pena-Hughes study, which found it to be superior to transitional bilingual education—but which she misclassified as "bilingual" education. (See Baker, 1987, for a detailed critique of Willig's study.) Moreover, it should be noted that there are a number of articles that are quite critical of meta-analysis and the biases it introduces. See, for example, Cook and Levitan, 1980; Eysenck, 1978; Slavin, 1984; Slavin, 1986; and Wilson and Rachman, 1983.

8. We included oral progress in preschool or kindergarten in this category because a reading test for these grades is obviously inappropriate. These two studies are noted in Appendix C with a double asterisk.
9. This is slightly more negative than Baker and de Kanter's 1981 findings that 33% of the studies found TBE to be superior, 17% found it to be inferior, and 50% found it to be no different from submersion. Altogether, 67% of their studies found TBE to be no different from or worse than submersion. It is also slightly more negative than Rossell and Ross's 1986 findings that in second language learning 29% of the studies show transitional bilingual education to be superior, 21% show it to be inferior, and 50% show it to be no different from submersion—doing nothing. Altogether, 71% found TBE to be no different or worse than the supposedly discredited submersion approach.
10. Neither Baker and de Kanter (1981, 1983a, 1983b) nor Rossell and Ross (1986) examined language because at that time there were too few studies that examined this outcome.
11. This is also slightly more negative than Baker and de Kanter (1981, 1983). They concluded that 14% of the studies found TBE to be superior, 21% found it to be inferior, and 64% of the studies found TBE to be no different from submersion. Altogether, 85% of their studies found TBE to be no different from or worse than submersion. These findings are pretty much the same as Rossell and Ross (1986), however. They found that in mathematics, 7% of the studies showed TBE to be superior, 27% showed it to be inferior, and 67% showed it to be no different from TBE. Altogether 93% of the studies showed it to be no different or worse than the supposedly discredited submersion approach in developing math proficiency.
12. Ramirez and his colleagues (1991) also examined maintenance bilingual education (late-exit bilingual education), but did not directly compare it to transitional bilingual education (contrary to media reports and his own conclusions). Although his graphs appear to show that the students in late-exit bilingual education were doing worse than the students in transitional bilingual education, no statistical analysis was performed to verify that.
13. Qualitative studies do not address the effectiveness of bilingual education on achievement in these areas because they never have a comparison group of students receiving an alternative treatment.
14. Powers and Rossman (1983) did a statistical analysis of the effects found in Leyba's study because they also were perplexed as to how it could call the TBE program successful when only 21% of its comparisons of the students

in bilingual education with those not in bilingual education yielded statistically significant results. Its analysis, which also had no control for pre-treatment group differences, found no positive effect for reading, but one for mathematics. Because the treatment group showed a large pre-treatment advantage in mathematics, this effect would have disappeared if Leyba had controlled for it.

15. The second language part of this proposition is basically circular reasoning. In essence it says that a certain level of second language competence is necessary to have a certain level of second language competence. This part of the theory will thus be ignored in future discussions.
16. Perhaps one of the more serious flaws of the facilitation theory is its lack of attention to the hieroglyphic languages—that is, the Asian and Middle Eastern languages—that not only have no similarity to English in appearance, but take much longer to master. In other words, learning to read in the native tongue may actually be harder in these languages than in the second language, if the latter is English or another Roman alphabet language. We know of no non-Roman alphabet bilingual programs in the United States that actually teach initial literacy in the native tongue, although many of them are nevertheless called bilingual education and receive bilingual education funding.
17. Cummins cites a UNESCO study, *The Use of the Vernacular Languages in Education* (Monographs on Fundamental Education, 1953), but there is no reference to this study in any of the legislation or literature of the 1960s.
18. Aside from the issue of the importance of the task, this study can tell us nothing about the kind of instruction LEP children should receive because the fluent Ukrainian bilingual students not only did better than the monolingual controls, but they also did better than the non-fluent Ukrainian bilingual students who were receiving *identical* instruction languages.
19. Although Cummins' theories are widely cited in the United States as evidence for the superiority of TBE, the research he relies on is research on *bilingualism* conducted in Canada where the educational process of creating bilinguals is the reverse of that in the United States. There, students are taught completely in the second language in kindergarten and first grade and gradually transitioned to mostly the native tongue by high school. Although everyone believes these programs to be quite successful, Cummins continues to be cited as evidence for the superiority of the United States version of bilingual education: the native tongue first and a gradual transition to the second language.
20. Because they are cross-sectional, not longitudinal, data, she cannot show rates of learning, only levels of achievement at a particular point in time.
21. Keith Baker was the project officer at the Department of Education for the Ramirez study.
22. Use of the descriptive TAMP curves is invalid because there was no control for student and other characteristics, as Ramirez admits earlier in the report. See Rossell (1992), Baker (1992), and Meyer and Feinberg (1992) for a discussion of these and other problems. See Collier (1992) for a complete failure to understand that one can draw no conclusions from these curves.
23. One of the two greatest errors of the Ramirez study was the use of nominal program designation—early-exit TBE, late-exit TBE and structured immersion--as the treatment variable rather than the percentage of English used in instruction, which varied considerably within nominal program categories

and by subject matter. The second greatest error, as mentioned above, was the failure to directly compare with statistical analyses the late-exit program to the early-exit and immersion programs.

24. Because of the influence of the facilitation theory, however, it is not always the case that English will be part of the instructional program. Christine Rossell has personally toured dozens of kindergarten, and to a lesser extent first grade, classrooms in Massachusetts where almost no English at all is used in instruction. The teachers justify this on the ground that their students need a long time to develop a high level of native tongue proficiency—a prerequisite to future academic success in the all-English classroom they believe. In short, they are true believers of the facilitation theory even if they may not know its name.
25. This is, of course, not the goal of all advocates of bilingual education. For example, a bilingual maintenance program whose goal is the highest level of skills in two languages regardless of the cost to either would look very different from a transitional bilingual program. See Rossell and Ross (1986) and Rossell (1990) for a description of the characteristics of an ideal bilingual maintenance program.
26. The procedures currently used consistently overidentify LEP students. See Rossell and Baker (1988) for a lengthy discussion of how and why this happens, and Rossell (1992) for a discussion of how to avoid inadvertently analyzing the effect of second language acquisition programs on students who are not really LEP.

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Appendix A: Methodologically Acceptable Studies (N = 72)

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Authors' Note: * From Rossell and Ross, 1986.
** From Baker and de Kanter, 1983b.

Authors' Note: *** In Krashen and Biber (1988), each individual school district's program was evaluated separately, so we also did this. All seven program evaluations (Baldwin Park, USD: Eastman Avenue; San Jose; Fremont; San Diego; Rockwood; Carpinteria) were methodologically unacceptable.

Appendix C

Effects of TBE on Second Language Reading, Language, and Math Compared to Other Instructional Programs as Found in Methodologically Acceptable Studies*

	READING (or ORAL**)	LANGUAGE	MATH
TBE v. Submersion			
TBE Better	AIR (Corpus Christi) (1975) Bacon, Kidd, & Seaberg (1982) Burkheimer et al. (1989) Campeau et al. (1975) Carsrud & Curtis (1980) Covey (1973) Kaufman (1968) Legaretta (1979) McConnell (1980) Morgan (1971) Olesini (1971) Plante (1976) Zirkel (1972)	Burkheimer et al. (1989)	Cohen (1975) Bacon, Kidd, & Seaberg (1982) Burkheimer et al. (1989)
	(N=13)	(N=1)	(N=3)

Appendix C Continued

	READING (or ORAL**)	LANGUAGE	MATH
No Difference	AIR (Corpus Christi) (1975) Alvarez (1975) Ariza (1988) Barclay (1969**) Campeau et al. (1975) Carsrud & Curtis (1980) Ciriza (1990a**) Cohen (1975) Cottrell (1971) Elizondo (1972) Huzar (1973) Kaufman (1968) Lampman (1973) Legaretta (1979) Maldonaldo (1977) Matthews (1979) McSpadden (1979, 1980) Morgan (1971) Plante (1976) Powers (1978) Prewitt-Diaz (1979) Rothfarb, Ariza, & Urrutia (1989) Stebbens et al. (1977) Skoczylas (1972) Vasquez (1990) Zirkel (1972)	Ariza (1988) Ed. Op. Concepts (1991b) Maldonado (1977) Rothfarb, Ariza & Urrutia (1989)	Alvarez (1975) Ariza (1988) Bates (1970) Carsrud & Curtis (1980) Cohen (1975) Covey (1973) Danoff et al. (1977, 1978) Ed. Op. Concepts (1991b) Elizondo (1972) Layden (1972) Maldonado (1977) McSpadden (1979, 1980) Moore & Parr (1978) Powers (1978) Rothfarb, Ariza, & Urrutia (1989) Stebbins et al. (1977) Vasquez (1990)
	(N=27)	(N=4)	(N=19)
TBE Worse	Bates (1970) Burkheimer et al. (1989) Cohen, Fathman, & Merino (1976) Curiel (1979) Curiel, Stenning, & Cooper (1980) Danoff et al. (1977, 1978) Ed. Op. Concepts (1991a, 1991b) El Paso (1987, 1990, 1992) Layden (1972) McSpadden (1980) Melendez (1980) Moore and Parr (1978) Stern (1975) Teschner (1990) Valladolid (1991) Webb, Clerc, & Gavito (1987)	Burkheimer et al. (1989) Curiel (1979) Curiel, Stenning, & Cooper (1980) Ed. Op. Concepts (1991a) El Paso (1987, 1990, 1992) Teschner (1990) Valladolid (1991)	Burkheimer et al. (1989) Cohen, Fathman, & McSpadden (1980) Ed. Op. Concepts (1991a) El Paso (1987, 1990, 1992) Maldonado (1977) Merino (1976) Skoczylas (1972) Stern (1975) Teschner (1990) Valladolid (1991)
	(N=20)	(N=9)	(N=12)

Appendix C Continued

	READING (or ORAL**)	LANGUAGE	MATH
TBE v. ESL			
TBE Better			Ames & Bicks (1978)
	(N=0)	(N=0)	(N=1)
No Difference	Ames & Bicks (1978) Balasubramonium et al. (1973) Lum (1971) Rossell (1990) Yap, Enoki, & Ishitani (1988)	Rossell (1990) Yap, Enoki, & Ishitani (1988)	Rossell (1990) Yap, Enoki, & Ishitani (1988)
	(N=5)	(N=2)	(N=2)
TBE Worse	Lum (1971) Rossell (1990)	Rossell (1990)	Rossell (1990)
	(N=2)	(N=1)	(N=1)
TBE v. Structured Immersion			
No Difference	Ramirez et al. (1991) Ramos et al. (1967)	Ramiriz et al. (1991)	Barik, Swain, & Nwanunobi (1977) Barik & Swain (1975) Lambert & Tucker (1972) Ramiriz et al. (1991) Ramos et al. (1967)
	(N=2)	(N=1)	(N=5)
TBE Worse	Barik, Swain, & Nwanunobi (1977) Barik & Swain (1978) Bruck, Lambert, & Tucker (1977) Day & Shapson (1988) Genessee & Lambert (1983) Genessee, Lambert, & Tucker (1977) Genessee et al. (1989) Gersten (1985) Malherbe (1946) Pena-Hughes & Solis (1980)		Genessee & Lambert (1983) Genessee et al. (1989) Gersten (1985)
	(N=10)	(N=0)	(N=3)
TBE v. Maintenance Bilingual			
TBE Better	Medina & Escamilla (1992)		
	(N=1)	(N=0)	(N=0)

Appendix C Continued

	READING (or ORAL**)	LANGUAGE	MATH
Immersion v. ESL			
Immersion Better	Barik & Swain (1975) Becker and Gersten (1982) Lambert & Tucker (1972)		
	(N=3)	(N=0)	(N=0)

*Studies are listed in more than one category if there were different effects for different grades or cohorts.

**Oral English achievement gains for preschool programs.