THE PRE–ENUMERATION SURVEY OF THE 1986 CENSUS OF CENTRAL LOS ANGELES COUNTY

Glenn Wolfgang, Bureau of the Census*
Statistical Research Division, Washington, D.C. 20233

I. BACKGROUND and PURPOSE of the PRE–ENUMERATION SURVEY

The Pre–Enumeration Survey (PrES) conducted in conjunction with the 1986 Census of Central Los Angeles County was the first of its kind in coverage measurement research. Coverage measurement is evaluation of how many persons are missed (undercounted) or duplicated (overcounted) in census enumerations. A related and better known coverage measurement survey is the Post Enumeration Survey (PES). In a PES, the survey data is collected after Census Day. The PrES collects coverage measurement data before the census.

The PrES collects names and characteristics (kinship, sex, race, ethnicity, age, and marital status) of persons living in sample households. It also seeks other addresses where the sample persons might be enumerated by the census. At each of these addresses, PrES data is matched person–to–person against names and characteristics collected in the census. A match status code (matched, not matched, out–of–scope, or unresolved) is assigned for each survey sample person. Counts in those code categories are used to compute coverage rates and estimates. More detail on the operations is presented in Section III of this paper.

In 1986, the PrES was a test of the feasibility and the advantages, relative to a PES, of its timing and the unique operations that this timing requires. Since the PES has been more extensively developed, it is a good basis for comparison. Indeed, the PrES has evolved out of and as a variation of the PES. The two enumeration surveys share the same purpose and kinds of operations, but the difference in timing dictates differences in conducting those operations. For example, PES respondents are asked, "What was . . .'s address on (Census Day)?" PrES respondents don't always know where they will be on Census Day. They are asked, "What is the address where . . . may move?" Tentativeness about this address is a major problem that, if not remedied by tracing procedures, could introduce a new bias in results.

Interest arose in conducting the coverage measurement survey before Census Day because of possible benefits to schedule and data quality. Since its operations begin earlier, PrES results might be obtained earlier than PES results (Citro & Cohen, 1985). Meeting early deadlines could become important if coverage measurement estimates gain priority. Early results do depend on the census data being prepared as early as the survey data. This becomes increasingly feasible as census automation progresses, but it was not tested in 1986.

Another advantage of the PrES comes from conducting it closer to Census Day. Census followup interviews continue for at least three months after Census Day and keep PES interviewers out of the field for that time. PrES interviewing, on the other hand, can be conducted up to the week before Census Day. The shorter interim between survey and census means fewer movers, people who change address between survey and census. Fewer movers generally leads to fewer persons to followup, fewer matching problems, and lesser followup costs.

On the other hand, there were concerns about the PrES. A more difficult followup situation counterbalances the data quality and cost advantages of the fewer movers. As already noted, census enumeration address cannot be as reliably collected by the PrES as by the PES. That address is important for matching data records, and also for locating sample persons in order to followup. That means other not–matched persons besides movers must be included in the followup. Also, in many cases, followup interviewers had to visit more than the sample address in the effort to locate respondents. This was called tracing. Tracing is expensive and, if unsuccessful, leaves cases unresolved, which translates to uncertainty in the data. The comparison of PrES and PES estimates, presented in Section IV, addresses the question of whether the results of PrES followup and matching are on a par with those of PES. However, some caution must be exercised in interpreting these comparisons because of the PrES's small sample size and unrefined procedures.

Another issue is the possibility that the PrES may have an effect on the Census results. Besides its impact on data quality or costs, such interference could distort or invalidate the main PrES purpose––evaluation. As an example of such impact, if PrES respondents did not understand that answering the PrES was not responding to the census, they may believe they need not submit their census form. The effect might also be to enhance census response. A PrES interviewer who convincingly elicits cooperation for the survey may be promoting cooperation with the census as a side effect. Comparisons of PrES to Non–PrES blocks on various census response variables are presented in Section V to investigate this concern.

II. SAMPLE DESIGN

For the 1986 PrES, a sample of 96 blocks (101 city blocks, with small ones clustered together to guarantee a minimum block size) was drawn from the originally planned site of The 1986 Census of Central Los Angeles County. In fact, PES and PrES samples for the area were designed together to permit the analyses presented in this paper. The blocks had been stratified by their predominant race, hispanic origin, and housing type. Some strata were more heavily sampled in order to improve the representation of groups which traditionally have had large undercount estimate variance. In this original sample, blocks were paired within strata and enough pairs drawn to supply the desired PES sample size for that stratum. Final selections were made from the blocks designated for the PrES in the 186 pairs.

The sample was reduced, however, by a Census Bureau decision to reduce the size of the test site. PrES interviewing was over, and supplemental PrES blocks could not be added, as for the PES, within the reduced site in order to maintain the desired sample size. The final PrES sample had 33 blocks (34 city blocks).

Fourteen large PrES blocks, with 70 or more households, were subsampled to an average size of about 43 households. The subsampling reduced interviewing caseloads with little effect on sampling variance.

III. OVERVIEW OF OPERATIONS

After the sample of blocks was designed, trained residents of the test sites used Census Bureau maps to locate the block, walk the block perimeter, and list addresses for each household. Subsampling was done as

needed, using accepted address listings. These operations were completed during January, 1986.

Interviewing began on January 22, 1986. All interviewing and quality control contacts with respondents were completed by March 8, 1986, seven days before Census Day.

Most of the subsequent PrES operations were processing phases similar to and adapted from ones designed for the PES. PrES keying and matching were done after the corresponding task was completed for the PES. This was to make sure that the PrES work did not delay or interfere with the higher priority deadlines of the PES. PrES keying was postponed until mid September. In early November, matching, first by computer and later by trained clerks, assigned match codes to PrES persons.

Field followup began on December 5 to collect more data where needed: to find the Census Day address of PrES sample persons not found in the census data, and to confirm or refute imperfect matches, and to collect missing characteristic data. Some innovations in tracing followup persons, including interviewer visits to additional addresses, were tested.

The results of the followup interviews were reviewed along with all census and survey records for the cases to determine a final match code assignment. The final review and match code assignments were completed by January 8, 1987.

IV. PrES–PES COMPARISONS

This analysis focuses on determining if the Pre–Enumeration Survey can produce results that are in keeping with Post Enumeration Survey results. The PES has been tested and refined. The PrES results should be reasonably close, if it is measuring the same thing––even on this trial run when operational procedures are still being shaped.

Coverage measurement surveys produce counts of survey persons enumerated (and those not) in the census. These tabulations are the core to estimates of the population, as shown in this general dual system estimator, comparable to $\hat{x}_D$ in Wolter (1986):

$$DSE = \frac{N^*_c}{(M / N_p)}$$

where

DSE = dual system estimate of the population size

N^*_c = census count minus estimated unmatchable or erroneously enumerated persons

M = estimate of PrES persons in the census, i.e. count of PrES data records matched to census

N_p = number of persons in the PrES

The denominator, a ratio of persons found by matching in both sets of data among all PrES cases, is known as the match rate. Match rates are useful for interpretation on their own. In fact, match rates are a better estimator for a focus on survey differences rather than census count issues, which are much the same for either survey. PrES match rates are rough because this was a trial run. That imprecision is evident in the differences between alternative estimates of the PrES match rates.

Raw match results, listed in Table 1, illustrate the need for more than one PrES match rate estimator. There are two lines on the table for unresolved match statuses. In computing match rates, to what degree should these cases be counted matched or not? The answer is not clear. Any one decision, given so many unresolved, could bias the estimate considerably and would misleadingly convey confidence in its precision.

TABLE 1
Matching Tabulations (persons)

Match Status	Followup Cases	Final Totals
Persons in scope	598	4463
Matched	40	3894
Not matched	389	394
Not traced	140	140
Other unresolved	29	35

Relative to the PES, PrES unresolved match statuses introduced more uncertainty into its data. The PrES had a 3.9% unresolved rate; the PES had 0.8%. A large part of the difference was due specifically to PrES respondents not traced during followup. The not–traced rate (among completed interviews) was 0.1% for the PES and 3.1% for the PrES. The rate of other unresolved for both surveys is then about the same. The PrES clearly needs development on the tracing of cases requiring followup.

Two match rate estimates were developed for the PrES data in order to represent the variation in the rates caused by unresolved match statuses. Neither is as extreme a treating the unresolved as if they were 0% or 100% matched. They may be viewed as a range of reasonable values in which the precise match rate should fall. The high rate excludes unresolved cases from both the numerator (matches) and denominator (total) counts. This is like imputing matches for the unresolved at the rate of matches among all resolved in the group (e.g. 90.9% for the raw total sample). The low rate counts unresolved persons in the base of the rate and augments the match count at the same rate as matches found among resolved followup cases (9.3%).

Both PrES rates, weighted to adjust for probability of selection and for noninterview rates, are listed in Table 2 for the total samples and for major subgroupings. Also, PES match rates computed from weighted counts reported by Wolter (1987), are presented for comparison. PrES standard errors are around 1.5 for the overall sample and vary up to 2.8 among subgroupings and 5.7 for the missing characteristic rates. PES standard errors were generally about half the PrES values. The PES imputed for age and sex characteristics missing in about 2.5% of its cases, so it has no data for the missing characteristic line of the table. About 9.8% of PrES cases had missing age or sex data.

The PES rates were affected very little by imputation procedures. Imputation of incomplete characteristic data and unresolved match statuses and use of proxy data make a difference of only 0.2% in the overall PES data reported here (Schenker, 1987). Considering the 3% difference between PrES high and low match rate estimates, comparison of unimputed PES match rates to the PrES rates would look very much the same.

As the bar graph in Figure 1 also shows, the PES match rates are very close to the low PrES rate, generally just below it, for race and ethnicity subgroups as well as the aggregate. The high PrES rates are about 3% higher. That pattern runs across the estimates for age by sex subgroups as well, displaying a consistency in how PrES and PES rates relate. In fact, a correlation of .89 or more ($p < .01$) between the PrES (high or low)

and PES match rates across age–sex groups supports a view that they are measuring the same thing.

TABLE 2
Weighted Match Rates

	PrES		PES
	High	Low	
All	92.16	89.40	88.56
Race/Ethnicity Groups			
Hispanic	91.69	88.78	87.64
Asian	92.99	92.36	90.39
Other	94.78	91.96	91.79
Age by Sex Groups			
0–14/M	91.13	88.08	88.61
0–14/F	92.74	90.11	87.59
15–29/M	87.68	85.27	83.67
15–29/F	90.40	86.67	83.97
30–44/M	93.53	90.23	86.71
30–44/F	93.00	90.06	91.54
45–64/M	96.12	93.95	92.94
45–64/F	96.60	93.93	93.71
65+ /M	97.34	97.34	93.19
65+ /F	96.41	94.54	94.85
? / ?	88.66	86.25	

Figures 2 and 3 show scatterplots of sample block pair match rates: PES against PrES (low or high). The axis scales are cut off at 50% on these plots to better see the dispersion, clustered in one quadrant. There was one outlier, however, not shown in each plot; it was located above the diagonal. Plot points would center on the diagonal if the match rates differed only randomly. PrES high rates seem to lie higher than PES rates, suggesting a difference. The statistical tests, the t–test and the Wilcoxon Matched–Pair Test (Marascuilo & McSweeney, 1977), done using the same data as in the plots and summarized in Table 3, both show the PrES high (but not the low) match rates are significantly higher than the PES rates.

TABLE 3
Test Comparisons of Weighted Match Rates

	Mean Diff	Observed Stat	(Prob)
t–Tests			
PES vs PrES (High)	−5.35%	−2.30	(0.03)
PES vs PrES (Low)	−2.38%	−0.99	(0.33)
Wilcoxon Matched–Pairs Test			
PES vs PrES (High)		−2.48	(0.01)
PES vs PrES (Low)		−0.07	(0.94)

A procedural error observed during review of the PrES clerical work could account for higher PrES match rates. In final stages of matching, some followup cases were assigned out–of–scope or unresolved when they should have been coded not matched. It appears that a clerk or two used PES rather than PrES decision charts. It had not been long since PES work. The errors were corrected conservatively. Refined procedures––clearer instructions and lack of interfering tasks––could easily avoid such problems in the future.

In summary, the uncertainty surrounding the unresolved cases qualifies any conclusion about PrES and PES comparability. When trace procedures are developed so that there are few unresolved cases to impute and one estimate is sufficient for the PrES, analysis of a possible bias or difference in biases will be possible. Also, for this initial test, it is likely that problems in unrefined procedures led to some systematic difference between PrES and PES estimates. The high correlation of match rates across subgroups of the sample does support a conclusion that the PES and PrES are measuring the same thing, although possibly at slightly different levels.

V. PrES EFFECT ON THE CENSUS

The Pre–Enumeration Survey contacts people before the census. There is some possibility that something in that contact changes how people view or respond to the census. The ideal investigation would be, "Did the PrES affect the census coverage in the sample blocks?" Since our only gauge of census accuracy in this test census is the coverage measurement results now in question, other variables drawn strictly from census data were used.

If the PrES had an effect on the census, it could show up in initial census response: census mail–back rates and failed edit rates. Home visit interviews and telephone followup repair most household nonresponse and edit failures before final census data are tabulated, so these rates do not necessarily reflect final census accuracy or coverage. If any influence on initial census response is found, it is a warning, rather than proof, that census accuracy might have been affected.

The mail return rate presented here is the percent of occupied households on the block's mail–out list that mailed back their census form before nonresponse followup. The overall failed edit rate is the percent of occupied households' forms that failed any response edit check at any stage of processing. The coverage edit failure rate is the percentage of mail returns with key answers that were missing. The content edit failure rate is the percentage of mail returns with dubious answers (e.g. out of range or inconsistent with other answers).

The plots in Figures 4–7 illustrate the initial census response rates for blocks where the PrES did or did not have a chance to affect those rates. PES blocks, which were paired with the PrES blocks in the sampling design, were used to represent those not affected by the PrES. It is appropriate to view PES blocks as controls in this analysis since, until the census enumeration is finished, respondents there know no more about census evaluation than those in other non–PrES blocks. Each plot point represents a specific pair of PES and PrES blocks as assigned by the sampling design. Those points would center along the diagonal if there were no effect.

Table 4 summarizes comparisons of the rates using the t–test and the Wilcoxon Matched–Pair Test. As before, the different tests closely agreed for a given comparison, testifying to the robustness of the first and the power of the second test on this data.

The mail return plots show an effect most clearly. Most of the plot points lie below the diagonal, illustrating that PES blocks had higher proportions of mail returns. PrES blocks yielded significantly lower mail return rates. The lower rate of mail returns in PrES blocks could be explained in several ways. Having given responses once to Census Bureau employees, residents may think mail response is not important or needed. Respondents may be tired or suspicious of repeated questioning.

The overall failed edit rates tend to look better in PrES blocks than in non–PrES blocks. The plot points generally lie below the diagonal, showing fewer edit failures per occupied household in PrES areas. Both tests of the differences are significant at the 90% but not the 95% confidence level. PrES blocks required fewer edit followup contacts, but this may be due to a positive

relation between mail returns and edit failures: if a census enumerator collects the data, there are fewer mistakes to find.

TABLE 4
Test Comparisons of Census Cooperation Rates:
PrES vs. Non–PrES Blocks

	Mean Diff	Observed Stat (Prob)
---------- t–Tests ----------		
Mail Return Rates	4.93%	2.58 (0.01)
Overall Failed Edit	3.88%	1.78 (0.08)
Failed Coverage Edit	1.91%	0.65 (0.52)
Failed Content Edit	−0.65%	−0.30 (0.77)
------ Wilcoxon Matched–Pairs Test -----		
Mail Return Rates		2.38 (0.02)
Overall Failed Edit		1.91 (0.06)
Failed Coverage Edit		−0.09 (0.93)
Failed Content Edit		−0.18 (0.86)

To remove the effect of mail returns from edit failure ratios, the number of mail returns can be used in the denominator as in the rates for the two kinds of edit failures: coverage and content. The plots and the tests show no effect on edit failures when mail response is controlled suggesting that people were giving equally accurate responses in PrES and non–PrES blocks, when that response was obtained by mail.

Initial response rates may have been affected, but what about final response rates? The census imputes persons from available information when final responses are inadequate. An equal number (18 or 55%) of the investigated PrES and non–PrES blocks had no census imputes at all. The differences in paired–block imputation rates were not significant (t=0.1, p=0.90).

Another clue to any impact on final census results might be in within–household coverage. The average number of persons per occupied household was also not significantly different from PrES to non–PrES blocks (t=0.6, p=0.54). It does not appear likely that a PrES suppresses (or enhances) reporting of residents within a household.

These results do not show that final census results were distorted by the presence of a PrES. The comparison of initial census response rates may have implications for census costs, since fewer mail returns means more expensive door–to–door followup interviewing, but that does not preclude a PrES if overriding benefits can be realized.

VI. CONCLUSIONS

The overall results of comparing PrES estimates to PES estimates do not clearly show that they are different. The lower end of the reasonable range of PrES match rates does not significantly differ; the upper end does; the true PrES match rate may or may not. Evidence of differences may be due in part to unrefined PrES trace procedures that left cases unresolved and necessitated use of high and low match rate estimates. It also may be due to unrefined operational procedures that may have biased results to some small degree.

The high priority and greatest challenge for further development of the PrES is to continue innovative development of trace procedures. Other operational procedures adapted from the PES can be revised to ensure the efficiency and accuracy of data collection and processing.

The lower mail return rate in PrES areas is evidence that a PrES effect on the census results is possible. Since nonresponse followup compensates for fewer mail returns, guaranteeing nearly complete housing coverage, and since analyses of failed edit, imputation, and within–household coverage rates showed no differences, it is likely that the final census counts are not seriously affected.

In summary, the Pre–Enumeration Survey is an alternative coverage measurement survey that could be implemented if refined further. This trial run has helped clarify its advantages and disadvantages relative to the Post Enumeration Survey.

REFERENCES

Citro, C.F. and Cohen M.L. (1985), The Bicentennial Census, New Directions for Methodology in 1990, Washington, D.C.: National Academy Press.

Marascuilo, L.A. and M. McSweeney (1977), Nonparametric and Distribution–Free Methods for the Social Sciences, Monterey, CA: Brooks/Cole.

Schenker, N. (1987), "Report on Missing Data in the 1986 Test of Adjustment Related Operations," Statistical Research Division Report Series No. CENSUS/SRD/RR–87/09.

Wolter, K.M. (1986), "Some Coverage Error Models for Census Data," Journal of the American Statistical Association, 81, 338–346.

Wolter, K.M. (1987), "Issues in Considering the Technical Feasibility of Adjustment," Joint Advisory Committee Meeting, April 1987, Rosslyn, Virginia.

* This paper reports the general results of research undertaken by Census Bureau staff. The views expressed are attributable to the author and do not necessarily reflect those of the Census Bureau.

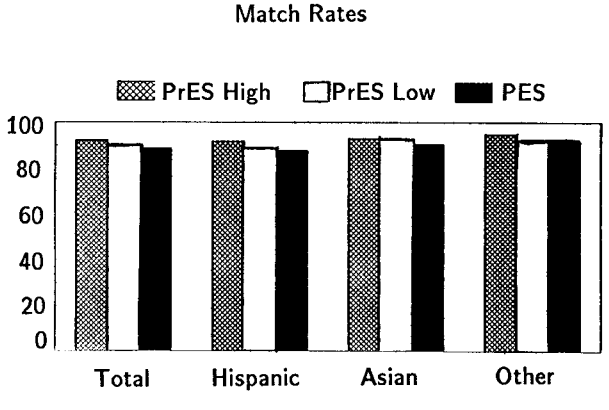

FIGURE 1

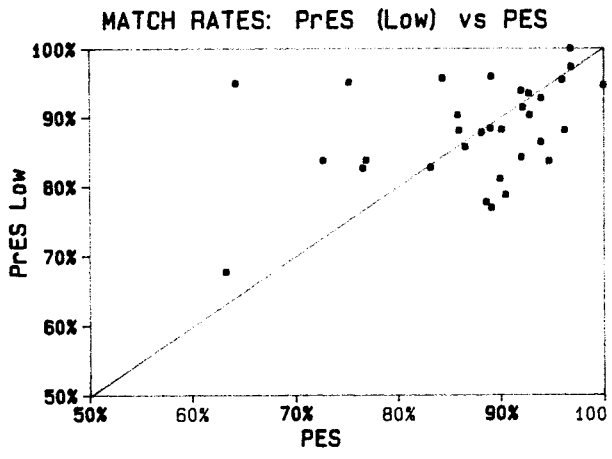

FIGURE 2

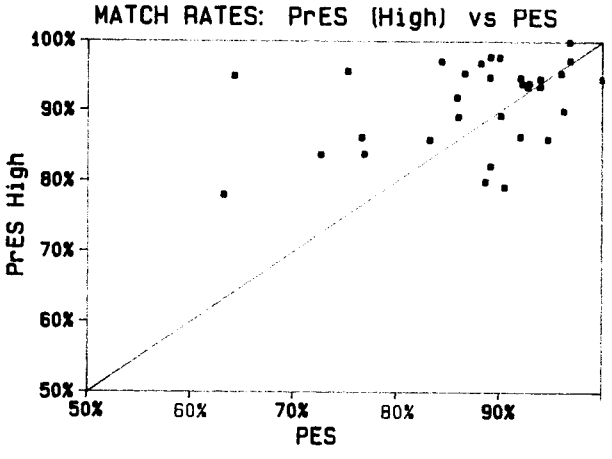

FIGURE 3

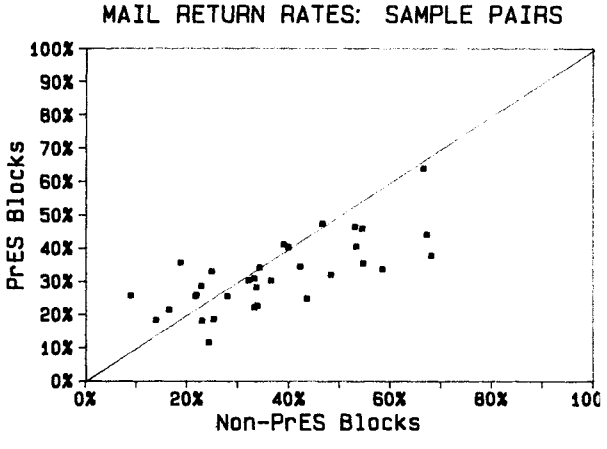

FIGURE 4

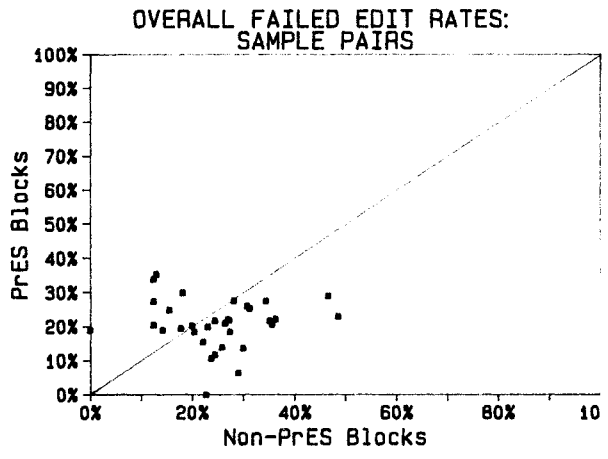

FIGURE 5

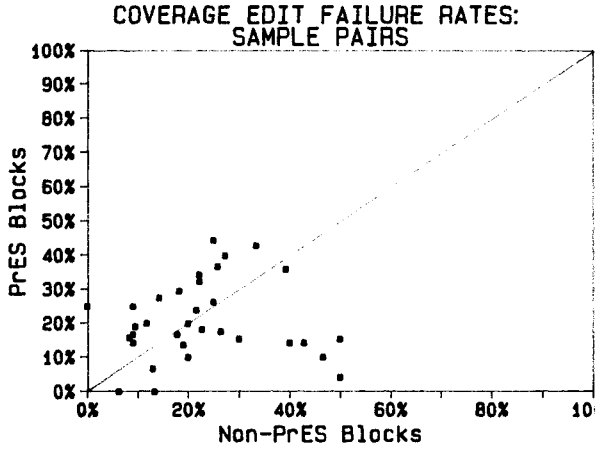

FIGURE 6

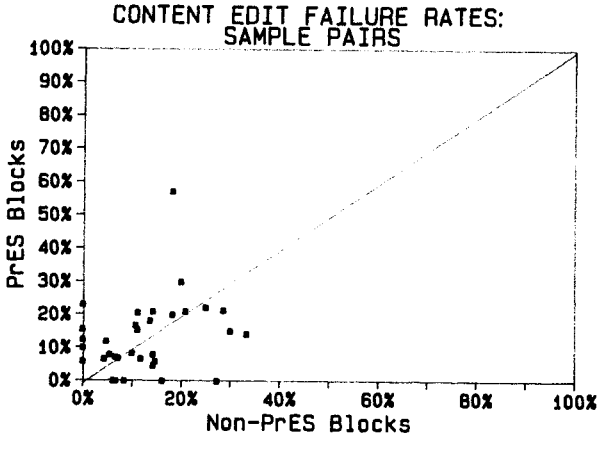

FIGURE 7