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EVALUATING THE EFFECT OF EDIT SYSTEMS ON DATA QUALITY: TWO CASE STUDIES

Invited Paper

Submitted by U.S. Bureau of the Census, United States[1]

I. INTRODUCTION

1. It can be difficult to convince analysts that there are tangible benefits in migrating from a program-customized data processing system to a generalized (centralized) data processing system. Institutional knowledge is built into a program-specific processing system. Systems designers may add program-specific Abells and whistles@ that make their systems easier to use. Generalized systems usually cannot offer such options. Moreover, generalized systems often require large amounts of program-specific parameter files, which can be time-consuming and burdensome to develop. Custom-coding minimizes the number of analyst-developed parameters.

2. On the other hand, customized systems require a great deal of resources and can be difficult to maintain. Moreover, they may be difficult to modify: for example, a simple change in data collection content might require complicated changes in database structures or processing code. Less measurable, but equally important, skills developed creating one processing system may not be transferable to another. However different the survey content and reporting unit, most economic programs have similar data processing requirements. Customized systems are limited to the processing options used by one (or a handful) of programs. Because generalized systems are designed to process several different data sets, they may feature previously unavailable processing options for the same situations (e.g., different imputation models). For these reasons B maintenance, flexibility, portability B the U.S. Bureau of the Census has developed a generalized survey processing system (the Standard Economic Processing System, or StEPS) for many of its economic surveys and generalized editing modules for the Economic Census (Plain Vanilla, or PV).

3. Migrating to a generalized processing system gives analysts an opportunity to review their current data processing procedures and perhaps improve them. This paper discusses two separate evaluation studies which did just that, examining editing and imputation procedures. The first study addressed the question of whether the StEPS editing module should be enhanced to include Fellegi-Holt editing capabilities, summarizing a study presented in Garcia and Thompson (2000). The second study compared two alternative PV ratio edit module implementations for the services sectors portion of the Economic Census, summarizing

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a study presented in Thompson and Adeshiyan (2001). Sections II and III describe how we evaluated the quality of edited and imputed data in each study. Section IV discusses the relative merits of the different approaches used. Section V provides my general conclusions.

II. CASE STUDY 1: ANNUAL CAPITAL EXPENDITURES SURVEYS (ACES) AND AGGIES

A. Background

4. The Annual Capital Expenditures survey (ACES) data are the primary source of information about domestic capital expenditures for federal agencies, private industry organizations, and academic researchers. ACES is a mail-out/mail-back survey of companies. Different forms are mailed to ACES sample companies, depending on whether they are employer companies (ACE-1 form) or non-employer companies (ACE-2). Approximately 75-percent of the ACES sample are employer companies, hence analyst review concentrates primarily on ACE-1 form responses.

5. The ACE-1 form respondents report fixed assets and capital expenditures for the calendar year in all subsidiaries and divisions for all operations within the United States. Total capital expenditures data are collected in three different sections of the questionnaire. First, total capital expenditures is reported separately (Item 1). Next, the same total capital expenditures data are collected broken down by type of capital expenditures (Structures, Equipment, and Other) cross-classified by new/used status (Item 2). Finally, the respondent company reports the same information for each industry in which the company operated and had capital expenditures for the survey year by completing a separate row for each industry in Item 6 of the questionnaire. The data totals and details that are reported in Item 6 by industry activity must balance to the reported capital expenditures data reported in Item 2, which in turn must agree with the data reported in Item 1. So, although the individual edits are straightforward (each set of detail items must agree with its associated reported total), the hierarchical combination of edits is complicated with several nested levels of simultaneous balance requirements. Additionally, ACES sets sample-industry specific limits on the ratio of total capital expenditures reported in Item 2 to administrative payroll.

6. ACES does not perform item imputation. Certain types of item non-response are handled automatically prior to machine editing. All other edit-failures are resolved clerically. ACES analysts use deterministic (logical) edits to automatically replace missing values or obtain new information directly from the companies (Willimack et al, 2000).

7. In collection year 2000, the ACES began using StEPS. StEPS is composed of integrated SAS modules that perform several survey processing activities, including data editing, imputation, and estimation (Ahmed and Tasky, 2000). The StEPS editing modules are used successfully by several other economic surveys at the U.S. Census Bureau. However, none of these surveys collect items that must satisfy simultaneous ratio edit and balance requirements. The StEPS edit software would require some enhancements to automatically edit **all** of the ACES data.

8. One option was to incorporate all or part of the National Agricultural Statistics Service (NASS) AGGIES system (Todaro, 1999) into StEPS. AGGIES solves simultaneous linear-inequality edits using Chernikova-type algorithms for determining the minimum number of fields to change so that a record satisfies the edits. Because both AGGIES and StEPS are written in SAS, a successful AGGIES application to ACES data could justify including all or part of AGGIES in StEPS.

B. Evaluation Study

9. Our first step was to conduct a feasibility study applying the ACES edits to a test deck of 2,230 records. Run times were prohibitively slow. While reviewing the edit-failing cases that exceeded allowable run-times, we realized that Felligi-Holt editing approach was not optimal for resolving all types of balance

edit failures. For example, raking details to the total has statistical advantages when the percent difference between the reported totals and aggregated details is Asmall@ (Sigman and Wagner, 1997). The feasibility study served as a launching point for discussions with ACES subject-matter-experts on **how** ACES edit failures **should** be resolved. Together with the subject-matter experts, we determined which edit-failing cases could be submitted to AGGIES and which edit-failing cases should be either clerically corrected or resolved using other StEPS imputation options (such as raking or substituting the sum of details for a total).

10. The evaluation study examined the **quality** of the AGGIES edit results. ACES analysts make very little attempt to simultaneously resolve all edit failures: Willimack et al (2000) reports that ACES analysts generally resolve edit failures by sequentially replacing reported totals with aggregated lower details (e.g., replace Item 2 totals with aggregated Item 6 details). Consequently, directly comparing AGGIES results to the historical production data was not reasonable. Instead, we used test data with simulated errors to perform the evaluation, creating three different files B a file of Atrue@ data values; a file of Araw@ data (contaminated by errors); and a file of Acleaned@ data (edited data) B and evaluating the edit procedure by comparing the true and clean data files, as recommended by Granquist (1997). To examine AGGIES effect on the edited data quality, we used the Manzari and Della Rocca (1999) accuracy indices shown in Figure 1. Here, modified data are raw data that do not equal true data (Acontaminated@ data items), *a* is the number of flagged modified data items, *b* is the number of unflagged modified data items, *c* is the number of flagged true data items, and *d* is the number of unflagged true data items. Note that we did **not** conduct any micro-level (questionnaire-level) evaluations prior to this macro-level comparison.

Figure 1: Manzari and Della Rocca Accuracy Indices

Index	Calculation
I1: fraction of true data correctly handled	d/(c+d)
I2: fraction of modified data correctly handled	a/(a+b)
I3: fraction of total data handled correctly	(a+d)/(a+b+c+d)

11. Often, certain items are reported more reliably than others. For example, the total capital expenditures reported in Item 2 B directly reported for the entire company B is often more reliably reported than the total capital expenditures reported in Item 6 (often unreported or aggregated incorrectly by the reporting unit). In general, reported totals are often considered more reliable than reported details, and the **managing** analysts preferred to change detail items instead of totals. Attempting to control the AGGIES edit outcome, we considered three sets of reliability weights (higher weights associated with higher reliability). The first set of weights was provided by subject-matter-experts, with weights ranging from ten to one. With this set of weights, AGGIES was more likely to flag a total item for deletion than two details even if both details were incorrectly reported. The second set of weights assigned a reliability weight of one to all details and a weight of two to all totals. The third set of weights were the AGGIES default weights (all equal to one.)

12. Regardless of weighting scheme, AGGIES performed quite well in terms of the I1 index for most data items, indicating that the software almost never flagged true values as edit failures. AGGIES also performed quite well in terms of the I3 index (fraction of total data handled correctly), with values greater than 90% for totals items and greater than 63% for detail items in single-industry companies and values greater than 75% for all items in multi-industry companies.

13. The AGGIES results were not nearly as strong in terms of the I2 index (probability of flagging an incorrect value), regardless of item reliability weighting scheme. With single-industry companies, the values of the the I2 index for the total capital expenditures reported in Items 1 and 2 were not completely discouraging (greater than 69% and greater than 85% respectively). For the detail items, the I2 indices were less than 50%, showing that the modified values were not consistently flagged. Multi-industry companies showed the same patterns (high I2 indices for total capital expenditures, generally low I2 indices for detail items).

14. Low I2 indices alone were not sufficient to conclude poor edited data quality. Often, this statistic is **not** a good indicator of edited data quality with a Fellegi-Holt edit system. Recall that this type of edit system attempts to preserve the maximum amount of reported data. Thus, a successful edit application would yield Ahigh@ (close to 1) I1 and I3 indices. However, low I2 indices would not necessarily indicate AGGIES failure, since the algorithm searches for a **minimal** deletion set, not the set of **all** incorrect items. For example, if an edit involving two erroneous values can be satisfied by correcting one item, AGGIES will select only one item for correction.

15. What did these results tell us? The only real conclusion that we could draw was that with this particular set of edits, reliability weights did not influence edit outcome. This was a problem for our analysts. Otherwise, we did not know whether AGGIES was yielding acceptable quality edited data. The irregular I2 indices were disturbing. If the solution sets were reasonable, then the low I2 indices did not indicate a problem. But, this aggregated analysis simply could not tell us whether the minimal solution sets were in fact reasonable solutions. The subject-matter experts felt the same way: they needed to review edited micro-data to characterize AGGIES performance.

16. A micro-review of a small test deck of edit-failing records confirmed our suspicions. In our first review, we found two records where the AGGIES solution did not yield a consistent record (at least one more data field needed to be deleted). NASS confirmed that the version of AGGIES that we were testing did **not** correctly handle balance edits and recommended that we use a previous version of AGGIES. With the earlier version of the software, the run-time was prohibitively long (at least one minute per record), and the AGGIES solution was non-minimal for one of our edit-failing records, although it did produce a consistent record. We decided to abandon further efforts with the AGGIES software.

III. CASE STUDY 2: SERVICES SECTORS PORTION OF THE ECONOMIC CENSUS AND PV

A. Background

17. The U.S. Census Bureau conducts an Economic Census in years ending in 2 and 7, mailing out over four million census forms to business establishments that provide commercial services to the public and other businesses. The services sectors portion of the Economic Census (hereafter referred to as the Aservices censuses@) comprises the majority of the Economic Census, collecting establishment data from five trade areas: Retail Trade; Wholesale Trade; Service Industries; Transportation, Communication, and Utility Industries (Utilities); and Finance, Insurance, and Real Estate (FIRE). All services sectors trade areas collect a core set of basic data items, such as annual payroll and sales [Note; the specific set of basic data items varies slightly by trade area]. In addition, the services censuses collect different types of industry-specific data called trailer data.

18. For the 1997 Economic Census, the Census Bureau developed and employed a generalized editing and imputation subsystem, called Plain Vanilla (Wagner 2000). PV consists of three separate edit and imputation programs: a ratio edit module (using the Fellegi-Holt edit model); a balance edit module; and a verification module. Program areas customize these modules by developing edit script files that describe how PV processes a particular program=s edits.

19. For the services censuses, the editing methods employed by PV were quite different from those used in previous censuses. The main difference between the two-edit systems was the ratio edit methodology. The PV ratio module tests the complete set of ratio edits simultaneously, determining the minimum number of reported data fields that must be changed to satisfy **all** of the edits. This methodology has been used successfully at the Census Bureau by other programs since the 1980s (Greenberg et al., 1990). Although the services censuses had always used ratio edits, 1997 was the first processing year in which these programs tested all ratio edits simultaneously. Misconceptions about the PV ratio edit module methodology led to some implementation problems in 1997. Consequently, we conducted a quality audit of each services

sector=s PV implementation at the conclusion of the 1997 production processing. Based on the audit results, we recommended several modifications to the 1997 production ratio edit procedures (Thompson et al, 2000).

20. To determine whether the revised edit procedures would provide acceptable imputations, we conducted a test on a subset of industries and basic data items using full-year reported cases from the 1997 census. Subject-matter-experts selected the industries. After processing, we had three competing values for each edited data item: the final published value of the item in the production database (our Agold standard@); the value obtained from the production (old) edit procedures (old script); and the value obtained using the new (recommended) procedures (new script).

B. Evaluation Study

21. In the ACES/AGGIES study, **we** provided our evaluation criterion to our customers (the analysts) along with the results. We discussed how to edit the AGGIES-ineligible cases with our customers; the AGGIES test used the ACES edit specifications. For this project, we discussed **how** we would compare the different data with our customers before beginning the evaluation. Why? These subject-matter-experts were not fully satisfied with their 1997 census PV edit results. We believed that if properly implemented, the PV software would yield high quality edited/imputed data for the most of the services censuses establishments. To convince the analysts, we needed comparison methods that the subject-matter-experts understood and agreed with.

22. This study was a joint project, coordinated by a team of statisticians, analysts, and programmers. The statisticians developed the ratio edits and provided imputation parameters. Then, one analyst developed the modified PV scripts for each trade area that implemented these recommended methods, incorporating the subject-matter-experts= revisions. For each trade area, she verified B by hand B new PV edit results for a small test deck of cases. Whenever she discovered that the modified procedures were not correctly implemented, she worked with the programmers to resolve the difference. In the end, the new-scripted PV edit produced the same edit/imputation results as expected on the test decks. So, a key difference between this evaluation study and the ACES/AGGIES study is that the edit being tested was verified at the micro level prior to conducting macro-level comparisons.

23. After this verification, we performed two separate analyses. Our macro-level analysis compared data item tabulations from the old and new script results to the tabulations based on final 1997 publication data (our Agold standard@). Figure 2 presents the format of these tabulations. We used Ablind-testing@ B which had no gold standard B for micro level comparisons. Because of this, all parties emphasized macro-level comparison results over micro-level comparison results.

24. For the macro-level comparison, we examined the ratios of old/final and new/final displayed in Figure 2. In each industry, we compared the two alternative tabulations for each item (columns 5 and 6 of Figure 2) to the final data tabulation (column 4 in Figure 2) and selected the Abetter@ tabulation as the one with the ratio (in columns 7 and 8) closer to 1 (unity). When **both** ratios were within five-percent of the final value, we said that the two scripts tied (i.e., performed equally).

Figure 2: Table Shell for Comparison of Original and New Script Edited Tabulated Data with Final 1997 Tabulated Data

Industry Code (1)	Data Item (2)	Establishment Size (3)	1997 Published Data Total (Final) (4)	Original PV-Script Edited Data Total (Old) (5)	New PV-Script Edited Data Total (New) (6)	Ratio of Old/Final (7)	Ratio of New/Final (8)
		Small					
		Large					
		Total					

25. In four of the five trade areas, the new script tabulations were generally closer to the final published values than the corresponding old scripts tabulations for all data items, often by a wide margin. This pattern was repeated at the industry level, providing evidence of overall improvement in edited/imputed data quality in those four trade areas.

26. However, the new script tabulations were not consistently better in all of our Retail trade industries. In the two Retail industries where the old script yielded better tabulations, the new script-imputed tabulations were between two and three times as large as the final tabulations. We tried to address this by tightening the ratio edit limits in these industries and re-editing. The new tabulations were not noticeably different. Clearly, this was not a parameter problem.

27. To characterize the cases that were poorly edited with the new scripts, we examined the records with the largest new-script-imputed values for each data item. In both industries, the difference between the two sets of tabulations were caused by a few establishments having the following reporting problems: all dollar value items were reported in the wrong units (Arounding@ errors); or only **one** basic data item was reported, and it was obviously incorrect (unreasonably large or small). Further macro-level analysis revealed that these two reporting and editing problems were not unique to Retail trade. In general, the macro-level analysis showed marked improvement in data quality over the old procedures, but also revealed deficiencies in the modified procedures that must be corrected before editing the 2002 Economic Census.

28. Comparing tabulations is a fairly objective way to determine if there is a systematic difference between the two sets of edits in terms of effect on tabulations. However, large establishments are very influential in this type of comparison. Although all Economic Census forms are machine edited, analyst review of edit-failing cases generally concentrates on large establishments because of time constraints, and subsequent stages of data review usually focus on the large establishments that most impact the tabulations. Because the final edited data **could** contain small establishments with erroneous data, we did not want to use it as a Agold standard@ for any micro-level analysis. Furthermore, the subject-matter-experts wanted to review the micro-data from both edit scripts. So, we conducted blind testing. We provided analysts from each trade area with certain basic information for each edited data item for 200 randomly selected cases and were asked to select which -- if either -- edit outcome (edit A or edit B) was acceptable. The label for edit A and edit B was randomly assigned, so that neither the analysts nor the evaluators knew which script was used to obtain either outcome. To avoid potentially biasing the outcome, analysts were **not** able to identify a particular establishment and were not given any edit flags. Also, the macro-level comparison results were not provided until blind testing was completed. Analysts were asked to review at least 100 of the 200 cases.

29. To test whether analysts preferred one script outcome over another, we used standard categorical analyses, first testing for association using the standard Pearson chi-squared test (Agresti, 1990) with the count data shown in Figure 3. Rejecting the hypothesis of independence allowed us to conclude that the analysts tend to prefer one script, but it did not tell us **which** script was preferred. To determine the analysts= preferences, we focused on the highlighted cells in Figure 3, where the analyst made a clear choice between the two scripts, using one-sided t-tests (H_0: $p_{21} <= p_{12}$).

Figure 3: Tabulation of Blind Testing Data

		New PV Edit		
Old PV Edit		Acceptable	Not Acceptable	Total
	Acceptable	Both Acceptable (N_{11})	Only Old Acceptable (N_{12})	N_{1+}
	Not Acceptable	Only New Acceptable (N_{21})	Neither Acceptable (N_{22})	N_{2+}
	Total	N_{+1}	N_{+2}	N_{++}

30. In all but one trade area (Services), we had evidence of association in analysts' old and new edit choices at the 5% significance level. For FIRE and Utilities, the old edit was preferred to the new edit; for Wholesale and for Retail, the new edit was preferred to the old edit; and we were unable to make a

conclusion about direction of preference for Services. Overall, these results were very interesting in that they appeared to conflict with the macro-level results. For example, the blind test analysis provided evidence that the analysts preferred the edit results using the new Retail script, but in half of our Retail industries, the new script was clearly worse than the old script at the macro-level. These preferences were not a function of the sample (e.g., the majority of reviewed Retail cases were not selected from the two industries where the new scripts had clearly better results).

31. We found the apparent contradiction between macro-level and the micro-level results in the FIRE, Utilities, and Retail trade areas perplexing, so we conducted an exploratory review of the records where the analysts clearly preferred the old script results (N_{12} cases). This led to two major findings (both confirmed by the analysts). First, the analysts usually preferred the script that changed fewer reported values or used administrative data for imputation, even when final edited data contained ratios that fell outside of the industry-specific limits. Second, analysts did not always provide their complete requirements for ratio edit limits. For example, by design, our ratio edit limits guaranteed that sales had to be greater than or equal to annual payroll. After reviewing the blind test results, we learned that the FIRE analysts, in addition, required that the ratio of sales to annual payroll could not exceed five (explicitly accounted for in the old script parameters).

IV. DISCUSSION

32. These two evaluation studies had very different outcomes. In the first study, our data quality concerns, in conjunction with prohibitively long run-times and extensive data preparation requirements ultimately led us to recommend against any further efforts with AGGIES. In contrast, the second study yielded a set of **initial** PV edit scripts for the 2002 Services Census, along with recommendations for improving pre-PV processing.

33. These two studies were motivated by very different circumstances. Prior to StEPS migration, ACES analysts resolved all edit failures clerically. Consequently, ACES analysts benefitted most from discussing how to resolve the edit-failing cases that would **not** be edited by AGGIES. Automatic edit solutions for these cases could use existing StEPS software, saving valuable processing time. Since analyst time was already budgeted for clerically resolving the AAGGIES-eligible cases,@ expected processing time would not be increased without AGGIES. The stakes were higher in the second study: the services censuses analysts did not have any alternative edit software available for 2002, and it was unlikely that resources would be available for developing other options. Consequently, these subject-matter-experts wanted an effective **PV** implementation.

34. There were other key differences between the two studies. In the ACES study, we began with macro-analyses and were left unconvinced by our results, as were our customers. In the PV study, the edit implementation had been verified using test decks before any macro-level comparisons, giving more validity to our analyses. With the benefit of hindsight, we should have conducted the same type of preliminary test deck micro-review on the ACES data. This first crucial step B which we skipped B is obvious, and is often omitted from edit evaluation studies.

35. In the PV study, tabulation comparisons were far more useful in terms of assessing alternative edit scripts= impact on data quality than the blind test micro review. The macro-level analysis in which resulting tabulations were compared were effective at both evaluating the overall edit/imputation results and at indicating systematic implementation problems, especially when combined with a micro-review of Aproblem@ records. Of course, in this study we had a widely-accepted Agold standard,@ we compared edit outcomes that used the same software with different input parameters, and we had hand-verified some of the results. In contrast, micro-level review in the ACES study revealed serious processing problems; the macro-level review based on accuracy indices only indicated potential problems (and we might have had the same types of results with reasonably edited data).

V. CONCLUSIONS

36. Ultimately, the best way to convince analysts of the benefits of using a generalized editing and imputation system is to show how the Anew@ system offers measurable improvements in data quality from the current procedures. Analyst Abuy-in@ is essential. The PV study accomplished this. Subject-matter-experts were involved in all aspects of the study, from selecting test industries to reviewing/approving all edit and imputation parameters to determining evaluation criteria. We never obtained such buy-in from the ACES analysts. For them, the AGGIES edit was a black box, and they did not trust the simulated data results.

37. Choosing **what** data to use for evaluation is perhaps the most difficult part of any evaluation study. There are several advantages to using available historical data. It allows the evaluator to reasonably estimate the amount of time actually required by the edit process. It uses a Agold standard@ accepted by the customers (subject-matter experts generally have great confidence in their publication data). But historical data has many disadvantages. First, it assumes that the edited data are entirely correct. Second, it is difficult to examine relationships between the edit and specific data anomalies (for example, studying the effect of varying levels of item intercorrelation in a Felligi-Holt edit system). These two problems can be addressed by using simulated data (modeled on real data) or real data with induced errors.

38. However, using simulated data to perform an evaluation has a price in terms of analyst Abuy-in@: no matter how realistic the models used, it is difficult to convince analysts that edit/imputation systems tested with Amade-up@data will work with their live data. The choice of data then becomes a trade-off: is it better to conduct a controlled experiment (use simulated data) or to obtain analyst acceptance (historical data)? When time is not an issue, it is probably best to do both, repeating the simulation study on historical data, thus satisfying the analysts and allowing the evaluator more analytical options. Otherwise, I have found that it is better to simply use historical data (selected by the analysts), sacrificing some analytical control to achieve analyst acceptance.

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