Determining the Appropriate Implementation of a Repeated Measures Analysis Comparing Two Periodic Estimates on Economic Survey[1]

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Abstract
Several of the leading economic indicator programs at the U.S. Census Bureau provide estimates of monthly or quarterly change. Hypothesis tests are used to determine if a change should be noted as significant; however, several different test statistics are employed. The goal of the presented research project is to investigate the statistical properties of three different test statistics over repeated samples. We consider three variations: a direct comparison of the ratio of two concurrent estimates to 1; a repeated measures analysis of the estimates' difference using point estimates; and a repeated measures analysis that uses smoothed estimates of variance and autocorrelation in the construction of the test statistic. We consider three different variance estimators per test statistic and use two simulated population datasets.

1. Background

Several of the leading economic indicator programs at the U.S. Census Bureau provide estimates of monthly or quarterly change. For these business indicator surveys, a new sample is selected every five years, and the same cases are interviewed throughout the five-year cycle.[2] To assess whether key totals have changed between collection periods, programs test whether the current period estimate is "significantly different" from the corresponding prior period estimate, at the ten-percent confidence level. Such tests of change assume that level of the current period estimate **equals** the prior period estimate, that the variance for each period's estimate is constant $(\sigma_1^2 = \sigma_2^2 = ...\sigma^2)$ and that the lag 1 autocorrelation (ρ_1) is constant. Thus, in experimental design terminology, the survey represents one recurring sample with different measures on the same sampled units over time *t* (repeated measures).

Our analysis focuses on Horvitz-Thompson estimates of totals for a stratified simple random sample without replacement (SRS-WOR) design selected from a skewed population. Each considered test statistic requires point estimates of totals, variances, and (on occasion) covariances or correlations. Only replication methods for estimating

[1] This paper is released to inform interested parties of research and to encourage discussion. The views expressed are those of the authors and not necessarily those of the U.S. Census Bureau.
[2] To mitigate coverage bias, new business (births) are added to the original sample on an ongoing basis, and some businesses will cease to exist (die) during the course of the survey.

variances are considered. Having said that, a variety of replicate variance estimators are available, and the statistical properties of the test statistics can greatly depend on the variance estimator used. To assess the sensitivity of our results to variance estimation methodology, we look at three replication methods that are frequently used in business surveys: the method of random groups (Wolter, 1985), the delete-a-group jackknife (Kott, 2001), and the stratified jackknife (Shao and Tu, 1995).

In theory, all of the considered hypothesis testing approaches are equivalent. However, since each test statistic uses point estimates obtained from survey data, actual results and conclusions from the same data sets can vary considerably.

The simplest implementation tests whether the change estimate ($\hat{\theta} = \frac{\hat{\mu}_t}{\hat{\mu}_{t-1}}$) is significantly different from 1. Hereafter, we refer to this approach as the **ratio test**, formally stated as

$$H_{0,1}: \ \theta = \mu_t / \mu_{t-1} = 1$$
$$H_{A,1}: \ \theta \neq 1$$

To perform the hypothesis test, we obtain **single** point estimates of μ_t, μ_{t-1}, and θ, respectively and use direct replication with method *m* (random group, delete-a-group jackknife, stratified jackknife) to obtain the variance estimate of the change estimate, denoted $\hat{v}_m(\hat{\theta})$. The test statistic for $H_{0,1}$ is given by $\tilde{t}_m = \frac{\hat{\theta} - 1}{\sqrt{\hat{v}_m(\hat{\theta})}}$, distributed as t_{df} where *df* are the degrees-of-freedom of the variance estimator[3].

The "textbook" approach uses a **repeated measures analysis**, i.e.,

$$H_{0,2}: \ \mu_t = \mu_{t-1}$$
$$H_{A,2}: \ \mu_t \neq \mu_{t-1}$$

To test $H_{0,2}$, we obtain the same point estimates of μ_t and μ_{t-1}, along with point estimates of their associated variance estimates, covariances, and lag 1 autocorrelations, denoted $\hat{v}_m(\mu_t), \hat{v}_m(\hat{\mu}_{t-1}), C\hat{o}v_m(\hat{\mu}_t, \hat{\mu}_{t-1})$, and $\hat{\rho}_{1m}$, again using replicate variance estimation. We consider two different test statistics:

$$t_m^* = \frac{\hat{\mu}_t - \hat{\mu}_{t-1}}{\sqrt{\hat{v}_m(\hat{\mu}_t) + \hat{v}_m(\hat{\mu}_{t-1}) - 2C\hat{o}v_m(\hat{\mu}_t, \hat{\mu}_{t-1})}} \sim t(df) \text{ under } H_{0,2}$$

$$T_m^{2*} = \frac{(\hat{\mu}_t - \hat{\mu}_{t-1})^2}{2\overline{\sigma}_m^{\ 2}(1 - \overline{\rho}_{1m})} \sim F(1, df) \ under \ H_{0,2}$$

$$\text{where } \overline{\sigma}_m^{\ 2} = \frac{1}{q} \sum_{t-q+1}^{t} \hat{v}_m(\hat{\mu}_t) \text{ and } \overline{\rho}_{1m} = \frac{1}{q} \sum_{t-q+1}^{t} \hat{\rho}_1.$$

[3] The different critical values used can be found in Table 1.

Note that the test statistic used for the t-test (t^*) uses individual point estimates, and that the T^{2*} test statistic (Johnson and Wichern 1988) uses **averaged** estimates of variance and autocorrelation from time period *t-q+1* to *t*. The T^{*2} is justified under the assumption in the introductory paragraph and should profit from the smoothing of the (perhaps unstable) point estimates of variance and autocorrelation. Our objective is to find a variant that has the best statistical properties over repeated samples regardless of variance estimation method for a "typical" periodic business survey, using the usual measures:

- Type I error rate (probability of rejecting the null hypothesis, when the null hypothesis is true), measured by coverage rate. With α=0.10 (the standard employed at the U.S. Census Bureau), the nominal coverage rate is 90%.
- The power of each test (i.e., the probability of rejecting the null hypothesis when the alternative is true), computed as 1 – Type II error rate

In almost all survey settings, analysis of Type I and Type II error rates are confounded: the properties of the implemented test will be affected by biases in the estimates and biases in the variances estimates, as well as violations of the parametric assumptions needed for the construction of the test statistic. This analysis employs an "ideal" survey, where the design precludes biased variance estimates, all units respond, and the population is completely known. Therefore, any differences in statistical properties between the varying test statistics are strictly due to sensitivity to alternative variance estimators.

This report presents the results of a Monte Carlo simulation study that uses two simulated population datasets. Section 2 describes the simulation study. Section 3 presents our results, which are further discussed in Section 4. We conclude in Section 5 with some general observations and ideas for future research.

2. Simulation Study

Data reported to the Census Bureau in economic surveys is protected, confidential information. In order to have a transparent public evaluation of our procedures, we use a synthetic population modeled on the data held internally by the Census Bureau. For convenience, we began with the business population developed by Mulry and Oliver (2009). This data set served as the frame for the simulation conducted to obtain Type I error estimates, hereafter referred to as MO(I). We later modified this population to obtain a new population to investigate Type II error rate/power (MO(II)).

The MO(I) population data is generated from one month of empirical sample data collected by the Monthly Retail Trade Survey (MRTS) for a particular industry. Population values for "Month 1" were generated by applying the nonparametric resampling algorithm described in Thompson (2000) to the (real) training data separately in each MRTS design stratum. Subsequent data for Months 2 –20 were generated as a stationary time series forecast going forward from Month 1. The series was generated using an ARMA series with historical standard errors and autocovariances to develop an AR(1) model. The AR(1) model for the stationary time series for Months 2 to 16 is given by

$$y_t - \zeta = \Phi * (y_{t-1} - \zeta) + a_t \text{, for } t = 2, \ldots, 19.$$

where $y_1 - \zeta = 0$, *s* ζ is the series mean, the noise component a_t is distributed $N(0, \sigma^2)$ and Φ is estimated using the sample-based lag one autocovariance for the selected industry. More details on the design of simulated data sets are available in Mulry and Oliver (2009).

With MO(I) population, the null hypothesis (no change) is always true. Computing coverage rates for samples selected from this population produces Type I error rates for each test; in Section 3, we summarize results for all 19 months of change estimates. To compute power, we need a population where the alternative is true. To ensure that the variance and autocorrelation estimates for each month are unchanged (a necessary condition), we created a second population (MO(II)) by adding a constant amount to each record in even months of the series. The final two months of the series represented a shift in total sales of 0.5%, which is easily recognized as significant regardless of test employed. We calibrated the constant backwards toward 0 for the remaining months i.e. we added 577 to each population record value in month two, we added 1154 in month four, 1731 in month six, etc. Thus, any two consecutive months illustrated a particular level of change, positive or negative. This halved the number of paired months available for coverage rate comparisons but allows us to easily produce power curves.

From each population, we selected 5,000 stratified SRS-WOR samples. Initially, we attempted to use the actual MRTS strata with a Neyman allocation of $n = 300$. Unfortunately, not all strata were represented in each random group, leading to biased variance estimates. To prevent this, we re-stratified the non-certainty units in our population into fourteen strata using the Dalenius-Hodges cum-root F rule (Cochran, 1977), and then determined the minimal overall sample size (n = 643) under Neyman allocation that yielded a minimum of 15 units per strata. A byproduct of this large sample size is that it allows us to detect very small changes in consecutive estimates of total, regardless of population.

After sample selection, we assigned each unit to one of 15 random groups. In each sample, we computed estimates of the total and the period-to-period change. For each estimate of total, we computed three corresponding variance and autocovariance/autocorrelation estimates (one per replication method); for each change estimate, we also computed three variance estimates. For consecutive months, we computed three test statistics: ratio test ($\tilde{t}$); repeated measures test with "monthly" point estimates (t^*); repeated measures test with smoothed point estimates of variance and lag 1 autocorrelation (T^{*2}). Our smoothing uses **all** point estimates of variance and autocorrelation. In reality, this would be impossible. As mentioned in Section 1, the smoothing should be implemented using a rolling average of computed from prior and current estimates (e.g., a six-month average).

We compared each to its associated critical value: t_{14} or $F_{1,14}$ for test statistics constructed with random group and delete-a-group jackknife variances and *z* or $F_{1,629}$ [4]for test statistics computed with stratified jackknife variance estimates. Table 1 presents the critical values used with each test statistic by variance estimation method.

[4] 629 = number of PSUs – number of strata

Table 1: Critical Values for Hypothesis Tests

	Ratio Test ($\tilde{t}$)	Repeated Measures	
		"Monthly" Point Estimates (t^*)	Smoothed Point Estimates (T^{*2}).
Delete-a-Group	1.761	1.761	3.1022
Random Group	1.761	1.761	3.1022
Stratified Jackknife	1.645	1.645	2.7135

Lastly, we computed Type I error rates and power using the repeated samples from the two populations. Type I error rates are available from the first population; estimates of power are available from the second.

As mentioned above, the original and revised simulated populations were generated as stable series. However, although stable, the original series does become more variable as the time period *t* increases, a function of the generating AR(1) model. This "variance creep" has very little effect on the Type I or Type II error computations presented in Section 3 because all simulations use adjacent pairs of months and because the sample size is more than adequate. The increase in variance levels would have impacted the Type II error computations in particular, had we used one month as a baseline and applied our level shift to all subsequent months, since the condition of equal variances would have been violated in later months (see Appendix 2). In addition, the models used to generate the month 1 populations for both series use different strata than our designs. This may affect our estimates, since the "true" population totals are generated from a different model than our stratified population totals, which cannot be accounted for by our survey design.

3. Results: Type I Error Rates and Power Curves

Table 2 summarizes our Type I error rates by form of hypothesis test. The presented rates were computed separately for each sample month from the 5,000 samples. The rates presented in Table 2 average the error rates from all but last set of comparisons[5]. Appendix 1 presents the individual coverage rates from each month.

Table 2: Average Type I Error Rates by Hypothesis Test Method (in Percents)

	Ratio Test ($\tilde{t}$)	Repeated Measures	
		"Monthly" Point Estimates (t^*)	Smoothed Point Estimates (T^{*2})
Delete-a-group Jackknife	11.35	11.96	10.28
Random Group	0.00	12.07	9.88*
Stratified Jackknife	11.97	12.43	11.02

[5] The final difference test (between months 19 and 20) is omitted due to the disproportionately large variance estimates in month 20.

With 95,000 (5,000 samples x 19 pairs) samples and a nominal error rate of 10%, an error rate between 9.84 and 10.16 (indicated by an asterisk) is itself nominal[6]. Thus, the Type I error results can be summarized as follows:

- The smoothed repeated measures test (T^{*2}) exhibits nominal or nearly nominal performance for the test statistics constructed with delete-a-group and random group variance estimates. Moreover, using smoothed variance/covariance estimates in the repeated measures test formulation consistently improves the Type I error rates. Given the number of repeated samples, the Type I error rates for the repeated measures with "monthly" point estimates are sufficiently far from the nominal 10% to cause concern.
- The ratio test Type I errors are comparable to the others when using delete-a-group jackknife or stratified jackknife variance estimates. However, when combined with random group variance estimates, error rates are too conservative.

Table 3 shows the levels percent change in the simulated population total from month to month using the second population where the alternative is true. Recall that these populations are modeled from the MRTS survey, so that the displayed levels have realistic magnitudes. Consequently, a small percent change in the estimated level represents hundreds of millions of dollars.

Table 3: Percent Change in the Alternative Population

Month i	Sales month I	Sales month i+1	Sales month i+2	% change i to i+1	% change i+1 to i+2
1	48384468971	48380339475	48362761536	-0.0085	-0.0363
3	48362761536	48382364251	48317496863	0.0405	-0.1341
5	48317496863	48374757840	48315382965	0.1185	-0.1227
7	48315382965	48409421319	48313099721	0.1946	-0.1990
9	48313099721	48428346993	48332684857	0.2385	-0.1975
11	48332684857	48490950474	48340295775	0.3275	-0.3107
13	48340295775	48498879787	48346939572	0.3281	-0.3133
15	48346939572	48515763624	48353624932	0.3492	-0.3342
17	48353624932	48591441053	48397745250	0.4918	-0.3986
19	48397745250	48582750831	.	0.3823	.

Figures 1 through 3 present "power curves" for each method by type of variance estimator, with the ratio test results indicated as green dots, the repeated measures "monthly" test results indicated by blue dots, and the repeated measures with "smoothed" point estimates indicated by red dots. These power curves represent a slightly atypical analysis. Instead of measuring the effect on power of increasing sample size, we measure the effect on power by increasing the **absolute difference** between adjacent monthly estimates, given a fixed sample size. In the graphs presented below, the power (1 – Type II error rate) is plotted against the values of percentage change displayed on the x-axis (see also Table 3).

[6] Using a binomial test.

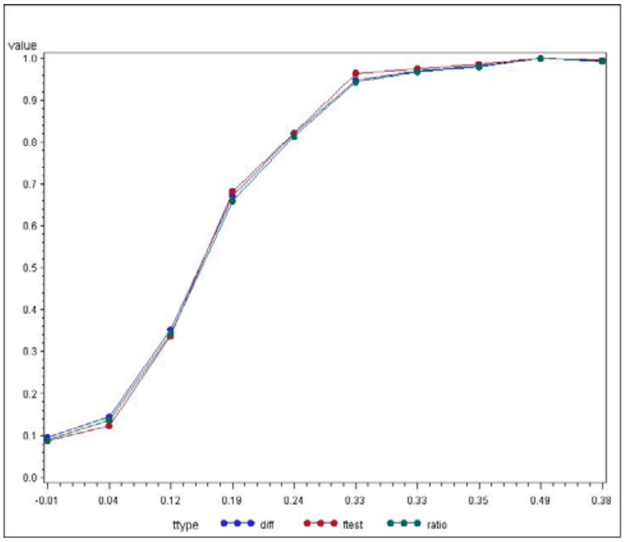

Figure 1: Power Curves with Delete-a-Group Jackknife Variances

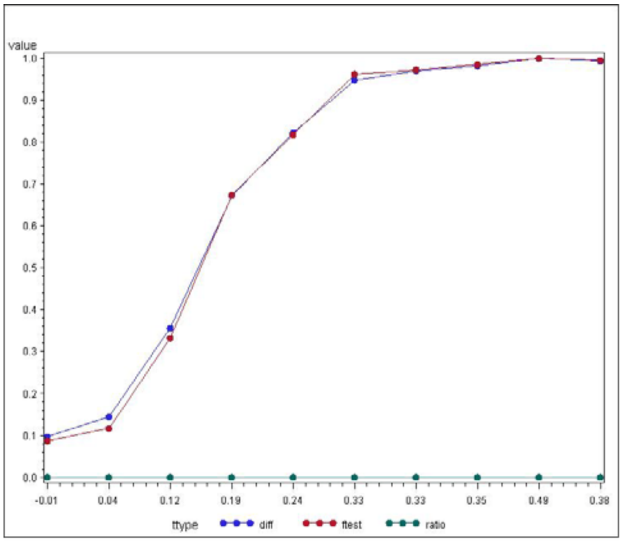

Figure 2: Power Curves with Random Group Variances

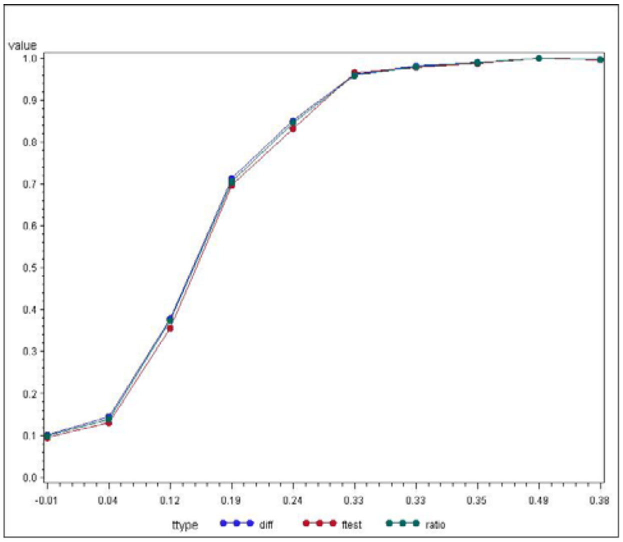

Figure 3: Power Curves with Stratified Jackknife Variances

Examining the power curves, we note the following:

- With random group and stratified jackknife variances estimates, none of the considered hypothesis testing methods appears to have an advantage over the others.
- Consistent with the Type I error rate results presented above, the random group results are not acceptable with the ratio test.

Recall that our survey design was "ideal" for the random group and delete-a-group jackknife variance estimators, since we ensured that each sample stratum is represented in each random group. In this context, we expected results using these variance estimators to be "unrealistically" good, especially given the known instability of the random group estimator. We believe that this is the case. If so, then it is quite likely that the ratio test would exhibit even worse performance with a less-than-ideal design for the random group method.

3. Analysis of the Test Statistics Using the Monte Carlo Limit Estimates of Variance

As mentioned in Section 3, the complete series of available variance and covariance estimates were averaged within replicate to compute the T^{*2} statistics. Thus within sample, we obtained a "close-to-unbiased" variance estimate, essentially reducing the number of random variables in T^{*2} to the point estimates alone. For this test statistic, our averaging method could therefore lead to overly optimistic estimates of Type I error and power for the two variants of the repeated measures tests.

To assess this, we examine the minimum absolute difference in point estimates detectable in our simulation study by each variant of test statistic using the "true" variance obtained from the Monte Carlo simulation for each variance estimator. Figures 4 through 6 plot minimum absolute difference in level for detection of "significance" by variance estimator. In each graph, the blue line represents the minimum absolute difference that could be detected using the t^* test, the red line represents the minimum absolute difference that could be detected using the T^{*2} test with a 6-month average variance (representing what would be used in practice) and lag 1 covariance, and the green asymptote minimum represents the minimum absolute difference that could be detected using the T^{*2} test with the 20-month average variance and 19-month average lag 1 covariance (labeled "all month average variance").

In one sense, the green asymptote represents a target for these repeated measures tests. However, these target values are affected by the "variance" creep in the stationary MO (I) series described in Section 2. As mentioned above, the population variances in the stable series increase as a consequence of the generating time series model. Including the larger variance estimates and covariance point estimates at the end of our series increases the denominator of the T^{*2} statistic, and consequently increases the minimum detectable **target** difference. However, omitting these final estimates would not adequately represent the impact of atypically large point estimates in the T^{*2} statistic computed with 6-month averages.

In addition, because the magnitude of each of the "true" variance estimates depends on the variance estimator, the scale of the graphs differ; using random groups variance estimates, the target minimum absolute difference is 80,266,911; using delete –a-group jackknife variance estimates, the target is 75,790,721; and using the stratified jackknife variance estimates, the target is 73,561,470. Simply put, our tests that use the delete-a-group and stratified jackknife variance estimates can accurately detect differences that are 6-percent and 8-percent smaller, respectively, than the corresponding tests constructed with the highly unstable random group variance estimates.

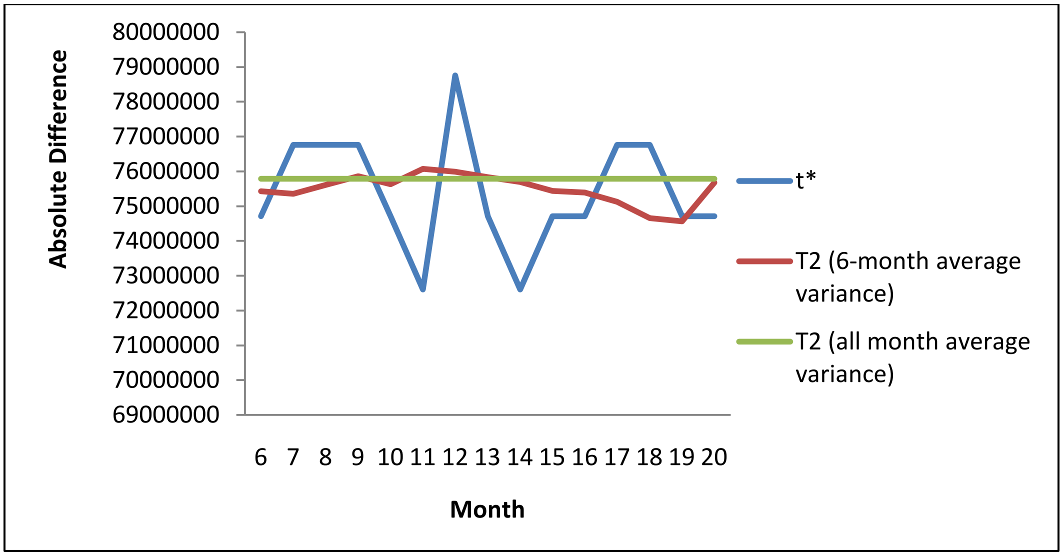

Figure 4: Minimum Significant Absolute Difference Detectable with Delete-a-group Jackknife Variance Estimator

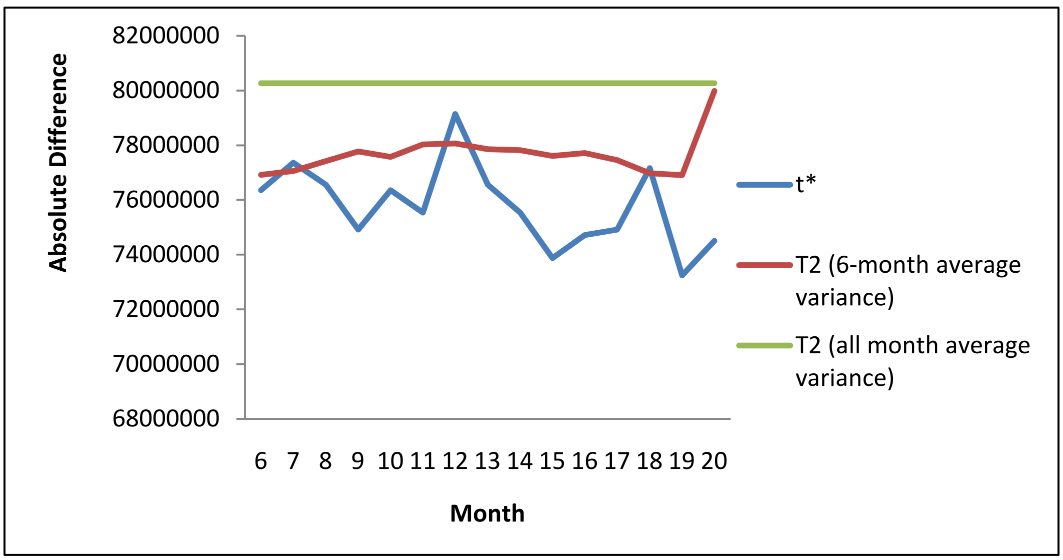

Figure 5: Minimum Significant Absolute Difference with the Random Group Variance Estimator

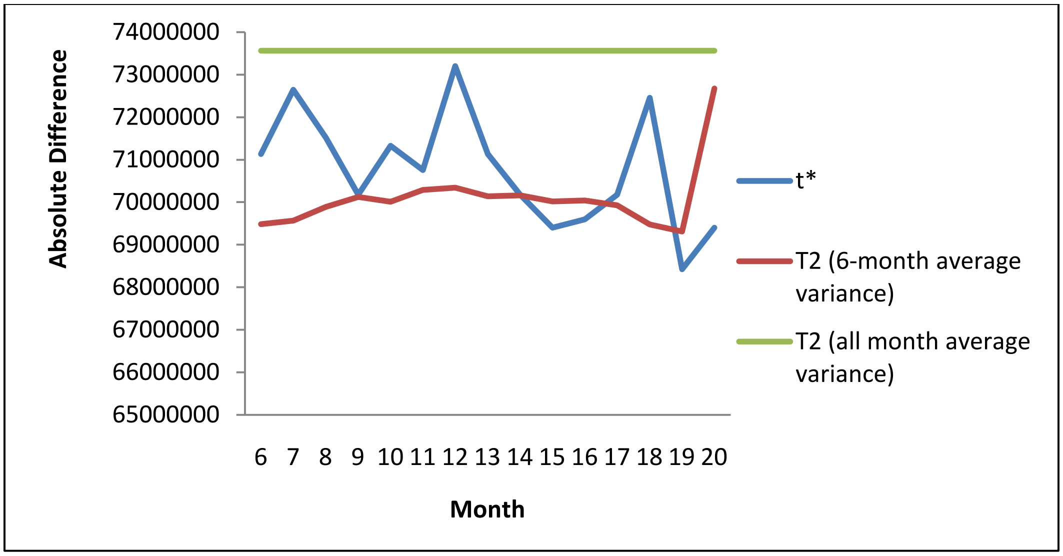

Figure 6: Minimum Absolute Difference Detectable with the Stratified Jackknife Variance Estimator

With all three variance estimators, the smoothing effect of using averaged variance and covariance estimates is evident. The benefits are most visible with the delete-a-group jackknife variance estimates, where the $T*^2$ test using the 6-month average closely follows the target. This pattern is mimicked somewhat with the random group variance estimates, where the minimum significant absolute difference detectable by the 6-month average $T*^2$ test is consistently slightly smaller than the target. However, the difference in detectable level with a 6-month average versus the target is less than one-percent. With the stratified jackknife variance estimates, the smoothing greatly improves the consistency of the test. It should be noted, however, that using the 6-month average would increase the Type I error rates in this case over those presented in Table 2.

Ultimately, the practical impact of the choice of repeated measures test is quite minimal in our simulation. The average detectable minimal percentage change for the t^*, $T*^2$ (6-month average), and $T*^2$ (all month average) is 0.16% with the delete-a-group jackknife variance estimator (all test statistics); 0.16%, 0.17%, and 0.16% respectively with the random group variance estimator; and 0.15%, 0.14%, and 0.15% respectively with the stratified jackknife variance estimator.

4. Conclusion

A change in an estimated total for a leading indicator can impact the economy. This limited research study demonstrates that the ability to precisely detect a small but "significant" change is not only a function of both the survey design and the employed variance estimator, but is also directly related to the test statistic used to detect the difference. Via our simulations, we substantiate the usage of a repeated measures approach, obtaining nearly nominal error rates regardless of variance estimator. Moreover, we demonstrate that error rates can be improved by using smoothed variances and autocorrelations, especially when the estimates are unstable (c.f., random group estimates). Perhaps most important, we demonstrate the sensitivity of the ratio test to the choice of variance estimator. The extremely poor performance of the ratio test with the random group variance estimator alone (with an "ideal" design) justifies discontinuing the practice, especially when the repeated measures approaches are less sensitive.

We admit that our simulation approach is an oversimplification. We consider one sample design, where stratification and allocation were determined for our convenience. We ignore the possibility of nonresponse. However, this scenario ensures that the differences in hypothesis testing results are attributable only to the form of the test statistic and the alternative variance estimators. The next stage of research – using our simulated populations – would use a more "realistic" design, perhaps using more strata, fewer sampled units, or both. After that, we would like to assess the robustness of our results by repeating this experiment with an alternative population that has a more realistic model for change.

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Appendix 1

Table 1 Percentage of samples accepting the hypothesis when the hypothesis is true.

	month 1			month 2			month 3		
vartype	ratio	diff	ftest	ratio	diff	ftest	ratio	diff	ftest
delagp	0.890	0.884	0.890	0.906	0.902	0.923	0.886	0.880	0.896
rgroup	1.000	0.883	0.894	1.000	0.901	0.924	1.000	0.879	0.902
stratj	0.882	0.878	0.884	0.904	0.899	0.918	0.880	0.876	0.891

	month 4			month 5			month 6		
vartype	ratio	diff	ftest	ratio	diff	ftest	ratio	diff	ftest
delagp	0.849	0.841	0.853	0.909	0.905	0.917	0.903	0.897	0.908
rgroup	1.000	0.839	0.859	1.000	0.904	0.921	1.000	0.896	0.913
stratj	0.834	0.831	0.840	0.901	0.898	0.910	0.903	0.898	0.903

	month 7			month 8			month 9		
vartype	ratio	diff	ftest	ratio	diff	ftest	ratio	diff	ftest
delagp	0.902	0.897	0.907	0.901	0.894	0.916	0.906	0.900	0.912
rgroup	1.000	0.896	0.913	1.000	0.893	0.918	1.000	0.899	0.917
stratj	0.896	0.892	0.900	0.894	0.890	0.909	0.900	0.898	0.908

	month 10			month 11			month 12		
vartype	ratio	diff	ftest	ratio	diff	ftest	ratio	diff	ftest
delagp	0.887	0.881	0.904	0.850	0.843	0.849	0.885	0.879	0.892
rgroup	1.000	0.879	0.907	1.000	0.842	0.854	1.000	0.878	0.893
stratj	0.886	0.882	0.896	0.838	0.832	0.839	0.876	0.870	0.883

	month 13			month 14			month 15		
vartype	ratio	diff	ftest	ratio	diff	ftest	ratio	diff	ftest
delagp	0.909	0.904	0.924	0.901	0.895	0.918	0.886	0.880	0.906
rgroup	1.000	0.903	0.927	1.000	0.894	0.921	1.000	0.879	0.911
stratj	0.905	0.899	0.916	0.895	0.890	0.914	0.884	0.879	0.899

	month 16			month 17			month 18		
vartype	ratio	diff	ftest	ratio	diff	ftest	ratio	diff	ftest
delagp	0.877	0.870	0.894	0.821	0.813	0.823	0.891	0.885	0.918
rgroup	1.000	0.869	0.895	1.000	0.811	0.831	1.000	0.884	0.921
stratj	0.871	0.866	0.885	0.807	0.801	0.810	0.889	0.884	0.912

	month 19		
vartype	ratio	diff	ftest
delagp	0.749	0.741	0.766
rgroup	1.000	0.738	0.770
stratj	0.726	0.720	0.753

Appendix 2

Averages over the Monte Carlo (where hypothesis is true) of variances, covariances between m and m+1, correlations between m and m+1 and the variance of ratio m/m+1. "1" is delete a group, "2" is random group and "3" is jackknife.

M	Total	V1	V2	V3	Cov1	Cov2	Cov3	corr1	corr2	corr3	ratiovar1	ratiovar2	ratiovar3
1	48385811990	9.496E+16	2.041E+16	1.446E+16	9.501E+16	2.044E+16	1.446E+16	0.9901	0.9538	0.9400	8.543E-07	1.670E-04	8.344E-07
2	48370080136	9.699E+16	2.237E+16	1.636E+16	9.688E+16	2.234E+16	1.639E+16	0.9911	0.9604	0.9504	7.825E-07	1.531E-04	7.719E-07
3	48363864876	9.854E+16	2.406E+16	1.818E+16	9.862E+16	2.413E+16	1.820E+16	0.9909	0.9619	0.9533	8.186E-07	1.601E-04	8.063E-07
4	48347623735	1.005E+17	2.605E+16	2.007E+16	1.004E+17	2.590E+16	2.001E+16	0.9907	0.9629	0.9557	8.424E-07	1.648E-04	8.309E-07
5	48319285154	1.021E+17	2.765E+16	2.186E+16	1.021E+17	2.758E+16	2.172E+16	0.9910	0.9655	0.9593	8.347E-07	1.634E-04	8.210E-07
6	48316075532	1.039E+17	2.939E+16	2.345E+16	1.039E+17	2.931E+16	2.344E+16	0.9909	0.9665	0.9613	8.636E-07	1.692E-04	8.528E-07
7	48317874043	1.058E+17	3.116E+16	2.538E+16	1.054E+17	3.072E+16	2.504E+16	0.9912	0.9684	0.9638	8.439E-07	1.651E-04	8.317E-07
8	48327485107	1.069E+17	3.217E+16	2.659E+16	1.067E+17	3.211E+16	2.656E+16	0.9916	0.9708	0.9675	8.103E-07	1.585E-04	7.955E-07
9	48316160324	1.084E+17	3.386E+16	2.835E+16	1.083E+17	3.375E+16	2.828E+16	0.9914	0.9712	0.9685	8.419E-07	1.649E-04	8.186E-07
10	48322696682	1.100E+17	3.552E+16	3.009E+16	1.101E+17	3.559E+16	3.007E+16	0.9918	0.9736	0.9708	8.177E-07	1.601E-04	8.051E-07
11	48335090504	1.119E+17	3.750E+16	3.190E+16	1.119E+17	3.745E+16	3.184E+16	0.9912	0.9725	0.9704	8.959E-07	1.757E-04	8.690E-07
12	48361709720	1.139E+17	3.942E+16	3.376E+16	1.135E+17	3.897E+16	3.330E+16	0.9918	0.9748	0.9726	8.374E-07	1.638E-04	8.176E-07
13	48343960046	1.149E+17	4.041E+16	3.471E+16	1.149E+17	4.024E+16	3.440E+16	0.9921	0.9764	0.9742	8.181E-07	1.602E-04	7.996E-07
14	48346842226	1.166E+17	4.191E+16	3.591E+16	1.163E+17	4.164E+16	3.569E+16	0.9925	0.9778	0.9758	7.886E-07	1.543E-04	7.783E-07
15	48352510560	1.178E+17	4.313E+16	3.725E+16	1.176E+17	4.289E+16	3.704E+16	0.9924	0.9780	0.9765	8.066E-07	1.579E-04	7.865E-07
16	48340485364	1.192E+17	4.445E+16	3.862E+16	1.188E+17	4.417E+16	3.831E+16	0.9925	0.9785	0.9771	8.104E-07	1.584E-04	7.932E-07
17	48360508289	1.203E+17	4.570E+16	3.982E+16	1.202E+17	4.553E+16	3.957E+16	0.9921	0.9780	0.9764	8.544E-07	1.672E-04	8.439E-07
18	48392908894	1.220E+17	4.728E+16	4.126E+16	1.214E+17	4.674E+16	4.076E+16	0.9929	0.9805	0.9793	7.685E-07	1.502E-04	7.567E-07
19	48403699057	1.226E+17	4.793E+16	4.199E+16	1.224E+17	4.779E+16	4.182E+16	0.9927	0.9803	0.9792	7.997E-07	1.567E-04	7.814E-07
20	48360245883	1.240E+17	4.944E+16	4.343E+16									