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Reducing Volatility of Labour Force Seasonal Adjusted Estimates
by correcting non-sampling survey effects

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EXECUTIVE SUMMARY

Recently there has been concern that the volatility of some Labour Force survey (LFS) estimates appears to be increasing. Intervention analysis was used to investigate whether the increase in volatility might be due to calendar related effects, whether it might be because of the impact of running particular supplementary surveys in conjunction with the LFS, whether it might be due to the introduction of different questionnaires as part of the LFS survey redesign from April 2001, or whether it might be due to some combination of the above influences. Australia level series were used to identify significant effects and estimate appropriate correction factors. The same correction factors were also applied to State series. The Australia level correction factors resulted in a reduction in volatility of the seasonally adjusted Australia level series of somewhere around 5% to 15%. Generally the Australia level corrections also reduced the volatility of the State series, but by a lesser amount.

Questions for MAC to consider

Q1: Are the justifications for the corrections sufficient?

Q2: What alternative methodologies exist for identifying and estimating the size of calendar and supplementary effects?

Q3: Is the adjustment forcing to low levels justifiable?

Q4: Should the supplementary survey effects be removed from seasonally adjusted estimates?

Q5: Are there strategies for conducting supplementary surveys that would minimise problems with seasonal adjustment?

1. Introduction

In recent times users of ABS Labour Force Survey (LFS) data have raised concerns that the volatility of the monthly seasonally adjusted estimates appears to be increasing. The results from the survey at a given point in time represents the estimates of the labour force conditions for that month. As with any other survey, the LFS is subject to measurement errors including sample and non-sampling errors. This paper focuses on three types of non-sampling errors which may be the sources of increased volatility. They are survey period effects, supplementary survey effects and questionnaire redesign effects. These effects are discussed in more detail below.

Investigations to date have mainly concentrated on the 6 Australia level series which together make up the Australia Employed persons series. These series are Adult Females full time, Adult Males full time, Junior Females full time, Junior Males full time, Females part time, and Males part time.

An outline of the paper is as follows: Following this introduction section 2 describes the intervention analysis methodology used to estimate the significance of various effects and remove significant effects to enhance seasonal adjustment. Section 3 describes how the various survey effects can arise in the context of the LFS. There is a discussion of which effects are found to be significant in section 4, followed by section 5 assessing the quality improvement as a result of the intervention analysis. Section 6 discusses the suitability of using Australia level correction factors on corresponding State series. Section 7 discusses some issues raised in relation to the intervention effects and seasonal adjustment. Finally we draw some conclusions from the investigations in section 8.

2. Intervention analysis methodology

Intervention analysis is a technique to identify the impact to a regular time series system from certain known condition changes. This technique is widely used in economic time series analysis. For example, Box and Tiao (1975), Hillmer, Bell and Taio (1983), Buszuwski and Scott (1988), Marial (1996) and Findley et al (1998) used this technique to identify outliers and calendar related effects for seasonal adjustment purposes.

The impact of an intervention can be estimated using a time series model consisting of a system filter representing the nature of the time series system, and a regression vector (explanatory variable) which represents the timing and the nature of the intervention. Since the impact of an intervention cannot be captured by the system filter, it can be estimated from the coefficient of the intervention regressor. In trying to assess the significance of each of the various intervention effects the picture could become confused due to the various effects overlaying each other, and so a methodology which attempts to estimate all of the various effects simultaneously was considered likely to yield superior results to one which considered each effect in isolation. An additive impact approach to estimating multiple intervention effects was considered the most feasible way of conducting the study.

The effect for variations of LFS interview date in relation to calendar (public holidays and starting date), supplementary surveys, and questionnaire redesign can be considered as measurement interventions to the "normal regular" labour force time series. These known interventions taken together resulted in approximately 85 components of possible impacts that were included in the study. Many intervention effects may be needed for a single series. Choosing a set of interventions can be labour intensive and somewhat subjective.

D13 intervention analysis:
The original time series, Y_t, can be decomposed into estimate of Trend (T_t), Seasonal (S_t) and Irregular (I_t), ie. $Y_t = T_t \times S_t \times I_t$. Intervention analysis can be carried out on the "D13" irregular component (or I_t) from a time series decomposition using the X-11 seasonal adjustment program. The D13 model is $\log(I_t) = \boldsymbol{\beta}'\mathbf{X}_t + \varepsilon_t$, where I_t is the Irregular estimate at time t, $\mathbf{X}_t$ is the regression matrix for a set of interventions, ε_t white noise of mean zero, and $\boldsymbol{\beta}'$ are the intervention coefficients. In this case the implicit X-11 model can be considered as a system filter.

The idea is that the irregulars (D13 series) derived from X-11 seasonal adjustment contain all the unexplained variations additional to trend and seasonal patterns. The impact of a set of interventions can be revealed when the D13 series is regressed on the set of regressors which are constructed for the set of interventions (referred to as the D13 method in this paper) subject to the normal noise level. Each estimated coefficient represents the estimated impact of the intervention with its statistical inference. However, the estimated intervention impact may not be accurate because X-11 uses moving averages iteratively without utilising information regarding interventions, so intervention impacts may leak into trend and seasonal components, and successive D13 series values are also correlated, which violates the standard regression assumption of independence.

RegARIMA intervention analysis:
Unlike the X-11 seasonal adjustment package which estimates the seasonal, trend and irregular components via moving averages, RegARIMA in X-12-ARIMA (Findley, 1998) or TRAMO (Gomez and Maravall, 1996) utilises an ARIMA model framework as a system filter from which the trend and seasonal factor can also be derived. RegARIMA models a multiplicative original series as $\log(Y_t) = \boldsymbol{\beta}'\mathbf{X}_t + z_t$, where Y_t is the original survey estimates at time t, $\mathbf{X}_t$ is the regression matrix for a set of interventions, z_t follows an ARIMA process describes the nature of time series, and $\boldsymbol{\beta}'$ are the intervention coefficients.

In addition, RegARIMA puts the system filter model estimation and intervention analysis in the same estimation framework, so that good estimates of both the system filter model parameters and the intervention parameters can be obtained. Although most RegARIMA programs available have the capability to detect certain types of outliers based on Bell's (1983) method from a time series automatically without prior knowledge of interventions, the regressors constructed for the investigation used available information and so should give better results than an automatic outlier approach.

The principal drawback of the RegARIMA package is the lack of an automated intervention analysis fitting capabilities to select a subset of interventions from a set of known intervention candidates. Each subset of potential intervention must be manually input by the analyst. Therefore, we used the D13 intervention analysis with a stepwise regression method (Miller,1984) as a sieve method to screen all the possible known interventions, and produced a subset of the interventions for refinement in the RegARIMA framework.

There are a number of statistical criteria that were used in the two phase intervention analysis. In the D13 intervention phase, we used Akaike's Information Criterion (or AIC) to select the best model. The AIC penalises minus twice the log-likelihood by twice the number of parameters. In the RegARIMA intervention phase, we used AICC information criteria (essentially the Akaike Information Criterion corrected for F-distribution) for RegARIMA model selection with the additional condition that each included intervention regressor had a reasonably large t-value.

note: Although the program did not fit the same ARIMA model to each of the series it was found that in practice the ARIMA model chosen had very little impact on the estimated regression coefficients, and so an "airline model" $(0\ 1\ 1)(0\ 1\ 1)_{12}$ was used for all series without sufficiently compromising other model validity criteria.

3. Non-sampling Survey Effects

(A) Survey Period Effects

The LFS is conducted over a two week period starting on a Monday, with the reference period being one week prior to the interview period. Since the survey begins on a Monday, the start date for the interview period will typically range over the days between the 6th of the month and the 12th of the month inclusive, although adjustments are often made to this timing for the months of December and January in order to reduce non response due to respondents often being absent from their usual address over the Christmas/New year period.

Start date effect

One possible calendar related effect resulting from this survey methodology is that there may be a relationship between the start date of the interview period and the resulting estimates. This could be for example due to seasonality which varies throughout the year, so that slightly different levels of seasonal activity are measured as the survey timing shifts relative to the start of each month from one year to the next. In addition there appears to be a start date effect which impacts particularly in the month of January and affects some series.

January Start Date effect

The January interview period falls close to the Christmas/New year holiday period. Employment conditions change markedly after this period, so a changing start date for January may be significant. The Interview period for January is later than other months due to the fact that the holiday period could cause high non-response rates. Therefore, the changing interview date could lead to differing coverages of the survey.

Interval effect
A consequence of the way the LFS is conducted is that there are always a whole number of weeks between successive surveys, most commonly 4 or 5 weeks although the adjustments in December and January can result in other spacings between successive interviews. If there is trend growth (or fall) in the activity being measured, then the difference between successive data values might be expected to be larger if there is a 5 week gap than a 4 week gap.

Easter effect in April
The timing of Easter in relationship to the interview fortnight can impact the estimates for some series in the month of April. Five different timings of Easter Monday in relation to the start of the survey fortnight for April are possible, namely that Easter Monday is a week before the survey period (Type 1), Easter Monday coincides with the start of the first week of the survey reference period (Type 2), Easter Monday coincides with the start of the second week of the survey reference period (Type 3), Easter Monday immediately follows the end of the reference period (Type 4), and Easter Monday falls a week after the end of the reference period (Type 5). The following diagram shows the situation schematically.

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  Timing of Easter Monday

 Type 1        Type 2        Type 3        Type 4        Type 5
 |             |             |             |             |
 V             V             V             V             V
|M T W T F S S|M T W T F S S|M T W T F S S|M T W T F S S|M T W T F S S|
              |<-Survey reference period->|
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It can be seen that there are no public holidays in the survey reference period for types 1 and 5, that there is 1 public holiday for type 2 (Easter Monday), that there are 3 public holidays for type 3 (Friday, Saturday and Monday), and that there 2 public holidays for type 4 (Friday and Saturday).

(B) Supplementary survey effects
One of the features of the LFS is that respondents may be asked supplementary survey questions on a range of topics, in addition to the standard LFS questions. It appears that the asking of supplementary survey questions can influence the responses given to the LFS questions in some circumstances.

(C) LFS questionnaire redesign effects
The questions asked in the LFS were altered starting in April 2001, and a slightly different interview regime was introduced, whereby one month in three (Feb, May, Aug, Nov) a more extended set of questions is asked than in the remaining months.

Two types of possible impact from the introduction of the new questionnaire were considered. The first type of impact to be tested for was a level shift effect, whereby the underlying level of particular LF series may shift up or down abruptly at the point in time where the new survey was introduced. This could happen, for example, if survey respondents interpreted questions slightly differently in the old and new surveys due to a change in wording between the two surveys.

The other type of impact that was investigated was the possibility that the new survey may have a particular pattern of seasonal variation in the collected data that was not present in data from the old survey. The reason for this is that the use of different sets of questions in different months could have an impact on the responses obtained.

In the X11 seasonal adjustment program used by the ABS, seasonal adjustment factors are estimated using a seasonal moving average which takes a weighted average of several years worth of detrended observations, grouped by month. One consequence of this methodology is that if the seasonality exhibited by a series changes suddenly at a particular point in time, the estimated seasonal factors will not accurately capture the change until several subsequent years worth of data is available to be averaged over. This problem can be reduced by applying a "seasonal break" correction to the affected series, however this still usually requires at least 3 years of data after the change in seasonality to enable reasonably accurate estimation of the size and direction of the correction factor. Given that the new survey commenced in April 2001, the application of seasonal break corrections (if warranted) would require data up to at least March 2004 to be available.

In the interim, the seasonal adjustment factors used to adjust the LF series are mainly representative of the seasonality of the old survey rather than of the new survey. If the new survey does have a different seasonal pattern then the seasonal adjustment factors may well fail to remove all of the seasonal pattern of the new survey data. The remaining seasonality can manifest itself as an apparent increase in the volatility of the adjusted series. Under the long form/short form hypothesis, the seasonal level of the long form months would be either raised or lowered relative to the seasonal level of the short form months. In other words the long form/short form effect (if present) should impact on the seasonal pattern in a specific way that can be tested for.

Appendix 1 describes how the various intervention regressors were constructed to test the significance of the effects.

4. Identified significant Interventions

Due to the large number of regressors to be tested (approximately 85 in total), we used the following methodology. A stepwise methodology was adopted for the D13 intervention analysis. The stepwise procedure in the S-plus computer package was used. The procedure used both forward variable selection and backward variable drop-one approaches and then finds the best model based on the AIC. This gave a list of possible significant regressors for each series. The last step was done in RegARIMA as the intervention estimates from the RegARIMA method were considered superior to the estimates from the "D13" method. A backward variable drop-one approach was then used within RegARIMA to reduce the final list of explanatory variables.

One slight complication to the analysis is that it appears that the new survey may have introduced structural changes to the nature of the time series (eg. an increase in the volatility of the estimates). This makes it difficult to estimate accurate calendar related and supplementary survey regression parameter estimates if the whole data span is used. Given that the new survey commenced relatively recently, it was decided to estimate the calendar related and supplementary survey regression parameters using the old survey data only (ie. up to March 2001). Then, under the assumption that the calendar related and supplementary survey estimates would at least approximately hold good for the new survey, correction factors based on the old survey were applied right to the current end of the data.

Finally, regressors designed to test whether the change from old to new survey had caused a level shift or change in seasonal pattern were applied as a separate exercise to the data after correction for calendar and supplementary survey effects.

Table 1 below gives the t-values for all 6 series. The following points are noted.

- Of the interview timing regressors, only the January Start Date Regressor (JSDR) was found to be significant in four of the 6 series. The 4-5 week and interview start date were not significant.
- Easter was significant in 3 of the 6 series when it fell in the middle of the reference week (Easter type 3).
- Some of the significant supplementary surveys had only been run once or twice. These surveys were most probably found to be significant because they coincided with an unusual observation.
- Most significant effects can be easily explained. For example, a supplementary survey on job search experience for Unemployed persons (SS38), lead to an increase in employment in the Adult Males Full Time, and Male Part time Series. Another example is the Supplementary survey on Employment and Earnings (SS72), lead to a decrease in employment for both the Male and Female Adult Full Time employment series.

Table 1: Significant Interventions & their estimated coefficient (t-value)

Intervention	No. of times run	Variable	Adult Males FT	Adult Females FT	Junior Males FT	Junior Females FT	Males PT	Females PT
January	24	JSDR	0.0009 (3.04)	0.0014 (2.34)			-0.0033 (-1.52)	-0.0035 (-3.66)
Easter type 1	2	EA01	0.0047 (2.29)					
Easter type 3	5	EA03	-0.0042 (-2.90)	-0.0076 (-2.67)				-0.0155 (-3.49)
Easter type 4	5	EA04	0.0029 (1.92)					
Cult. & Leis.	3	SS05		-0.0088 (-1.90)				
Career Exp.	3	SS07		-0.0107 (-2.24)				
Ch. Migrants	7	SS09				-0.0129 (-1.20)		
Emp. Ben.	16	SS19				-0.0173 (-1.52)		-0.0102 (-2.56)
Energy use	2	SS21	0.0103 (1.84)					
Environ.	4	SS22	-0.0068 (-2.21)		0.0628 (2.97)	-0.0252 (-0.97)		-0.0209 (-2.12)
Exits L.F.	1	SS23			0.0375 (1.96)		0.0437 (2.07)	0.0235 (2.61)
Ex-serivice	2	SS24					-0.0278 (-1.83)	
Health Ins.	11	SS29		-0.0047 (-2.24)				0.0085 (2.48)
Hearing	1	SS30						-0.0269 (-2.70)
Job.Se.UEmp	18	SS38	0.0035 (3.18)				0.0153 (1.81)	
Labour Mob.	19	SS40				0.0102 (0.78)	0.0109 (0.96)	
Loc. Work	1	SS44					-0.0438 (-1.72)	
Mult. Jobs	14	SS46			-0.0073 (-1.04)			
Cease.FT.Em	2	SS48	0.0063 (3.24)	0.0107 (2.60)			-0.0524 (-3.49)	-0.0255 (-3.95)
Emp@Home	3	SS50						-0.0098 (-1.82)
Per. NILF	34	SS51					0.0128 (1.18)	
Ret. FT work	1	SS53				0.0573 (2.04)		
Rent. Invest.	2	SS56				-0.0358 (-2.02)		
Rent. Tenant	1	SS57		-0.0137 (-2.38)				
Retire. intent.	5	SS58	0.0032 (2.45)					
Rubella Im.	1	SS60	-0.0054 (-1.98)	0.0125 (2.11)			0.0347 (1.64)	
Natural nrg	2	SS62					0.0191 (1.27)	
Super.	5	SS64				-0.0512 (-3.48)	0.0366 (2.91)	
Phone Con.	2	SS65				-0.0445 (-2.45)		
Trade Union	9	SS66			0.0153 (1.64)			
Under Emp P.	12	SS68				0.0193 (1.56)		
Vol. Workers	1	SS69				0.0430 (1.59)		
Wst,tr&mot.v.	1	SS70			-0.0591 (-1.38)			
Water	2	SS71			0.0823 (2.34)			
Wk.Earn.Emp	24	SS72	-0.0040 (-2.71)	-0.0028 (-3.66)				

From the above table it can be seen that although many of the regressors are common to more than one series there are also considerable differences between the 6 series in terms of which regressors are significant. The mechanisms that generate an impact on LFS estimates by some of the supplementary surveys are a little hard to fathom. It is not immediately obvious for example why questions regarding Rubella immunisation should impact on employment estimates, yet supplementary survey 60 dealing with this issue turns out to be a significant regressor for three out of the six series studied.

5. Quality Assessment

A primary objective of the prior correction of known intervention effects is to estimate seasonal factors accurately. Therefore, the reduction in volatility for seasonally adjusted estimates can be potentially achieved. Two primary measures are used to assess the improvement. (1) The average absolute deviation of the irregular (AADI) from 1 (calculated over the whole series span) is used to assess the seasonally adjusted level estimates quality and (2) the average absolute percentage change (AAPC) in the irregular is used to assess the period-to-period movement of the seasonally adjusted estimates.

Table 2 shows the reduction in the deviation of the irregulars as a result of applying prior corrections to adjust for the significant effects.

Table 2: Comparison of average absolute deviation of irregulars

Series	**No Correction**	**Prior Corrected**	**Relative % improvement**
Adult Female FT	0.00348	0.00301	13.5%
Adult Male FT	0.00143	0.00122	14.9%
Junior Female FT	0.01264	0.01146	9.39%
Junior Male FT	0.01052	0.01003	4.64%
Females PT	0.00519	0.00459	11.5%
Males PT	0.01125	0.01008	10.4%
Female FT	*0.00344*	*0.00308*	*10.4%*
Male FT	*0.00151*	*0.00134*	*11.8%*
Persons FT	*0.00171*	*0.00145*	*14.9%*
Persons PT	*0.00555*	*0.00474*	*14.6%*
Female	*0.00261*	*0.00231*	*11.6%*
Male	*0.00152*	*0.00130*	*14.6%*
Persons	*0.00162*	*0.00137*	*15.2%*

Note: The series in italics font are indirectly seasonally adjusted from their components.

Table 3 shows the reduction in the size of the period-to-period movements as a result of applying the prior corrections.

Table 3: Average Absolute Percentage Change period-to-period in the Irregular component

Series	No Correction	Prior Corrected	Relative % improvement
Adult Female FT	0.574	0.494	13.8%
Adult Male FT	0.248	0.205	17.3%
Junior Female FT	1.973	1.814	8.07%
Junior Male FT	1.692	1.597	5.63%
Females PT	0.822	0.729	11.2%
Males PT	1.836	1.621	11.7%
Female FT	*0.546*	*0.484*	*11.3%*
Male FT	*0.247*	*0.208*	*15.6%*
Persons FT	*0.278*	*0.228*	*18.1%*
Persons PT	*0.892*	*0.771*	*13.6%*
Female	*0.405*	*0.343*	*15.4%*
Male	*0.245*	*0.198*	*19.2%*
Persons	*0.256*	*0.206*	*19.3%*

Note: series in italics font are indirectly seasonally adjusted from their components.

Both AADI and AAPC measure show more than 10% percent improvement for relative less volatile series (Adult Female FT, Adult Male FT, Females PT, Males PT) while the two junior full time employment series achieve less than 10%. This result implies that some potential known interventions are likely to be buried in the "normal" high level of noise present in more volatile series. Also, the corrections may be hard to estimate accurately because the estimation of the effect is subject to a high level of noise. On the other hand, some statistically significant intervention effects may also be incidental.

Appendix 2 contains graphs of the 6 Australia level seasonally adjusted series before and after correction for calendar related, supplementary survey, and LFS redesign effects.

6. Treatment of the lower aggregates

The State level component series will contain more noise than the Australian level series, and therefore estimating the effect for these series may be risky. This problem can be overcome by applying the Australian level corrections to the State level component series. The State level component series however have broader categories than the Australian series. For example, there is no Part Time series at the State level. Implicit factors can still be derived by the following method. The implicit factor for Employed Females is equal to the sum of all three Australian level female series divided by the sum of the three prior corrected female series.

Table 4 below gives the average improvement in the volatility (Average Absolute Deviation of Irregular from one) of the irregular series once the factors have been applied to the State level component series and then indirectly seasonally adjusted. New South Wales shows a large improvement, whilst the smaller states (NT, ACT and Tasmania) have little or no decrease in volatility.

Table 4: Average Percentage improvement in AADI

State level total employed	Relative improvement	State level employed full-time	Relative improvement
NSW	7.36%	NSW	7.48%
VIC	1.71%	VIC	4.44%
QLD	4.11%	QLD	3.19%
SA	2.45%	SA	1.62%
WA	5.37%	WA	3.39%
TAS	0.79%	TAS	0.92%
NT	0.01%	NT	-0.58%
ACT	-1.75%	ACT	-0.20%

State level component series improvements are given in Table 5, which shows that the adjustment forcing method, in some cases, leads to a slight volatility increase. The Full time employment series also shows less improvement than the total employment series for both Male and Female series. If we assume that the calendar and supplementary effects do not depend on geographical differences, the small improvement in lower level aggregates implies the effects are likely to be buried in the relatively higher noise of these aggregates, and may not be able to be estimated directly from the lower level aggregates.

Table 5: Average Percentage improvement in AADI for State component estimates

Series	AADI for current method	AADI for forced method	Percentage Improvement
Employed NSW Females	0.004304592	0.004070786	5.43%
Employed NSW Females FT	0.00634969	0.006003956	5.44%
Employed NSW Males	0.002346748	0.002096833	10.65%
Employed NSW Males FT	0.002590867	0.002423398	6.46%
Employed VIC Females	0.004248184	0.004156881	2.15%
Employed VIC Females FT	0.005754806	0.005839619	-1.47%
Employed VIC Males	0.002796939	0.002639799	5.62%
Employed VIC Males FT	0.003154789	0.003019524	4.29%
Employed QLD Females	0.004663201	0.004570167	2.00%
Employed QLD Females FT	0.007359041	0.007153687	2.79%
Employed QLD Males	0.002869956	0.002784214	2.99%
Employed QLD Males FT	0.003263959	0.003255602	0.26%
Employed SA Females	0.004986235	0.005005299	-0.38%
Employed SA Females FT	0.00797568	0.007923578	0.65%
Employed SA Males	0.003259609	0.00315901	3.09%
Employed SA Males FT	0.003710891	0.003581224	3.49%
Employed WA Females	0.00543485	0.00517018	4.87%
Employed WA Females FT	0.008740986	0.008425731	3.61%
Employed WA Males	0.002818677	0.002769735	1.74%
Employed WA Males FT	0.003391116	0.003410592	-0.57%
Employed TAS Females	0.007231432	0.00706416	2.31%
Employed TAS Females FT	0.011493721	0.01131433	1.56%
Employed TAS Males	0.00429715	0.004357595	-1.41%
Employed TAS Males FT	0.004601714	0.004636289	-0.75%
Employed NT Females	0.0208526	0.0208959	-0.21%
Employed NT Females FT	0.0264176	0.02635963	0.22%
Employed NT Males	0.01362564	0.01371025	-0.62%
Employed NT Males FT	0.01546897	0.01556977	-0.65%
Employed ACT Females	0.008137276	0.008142861	-0.07%
Employed ACT Females FT	0.01406119	0.014024337	0.26%
Employed ACT Males	0.006054177	0.006190565	-2.25%
Employed ACT Males FT	0.006919578	0.006956901	-0.54%

7. Issues to be considered

There are two type of prior corrections in the seasonal adjustment.

1. prior corrections of non-calendar related or non-systematic effects for more accurate seasonal factor estimation, in which case the effects will be included in the published seasonally adjusted estimates.
2. prior corrections of calendar related and systematic effects for more accurate seasonal factor estimation, in which case the effect will be removed from the published seasonally adjusted estimates.

By definition, survey period related effects are systematically related to the calendar. Therefore, the effect should not be presented in the seasonally adjusted estimates. We propose to introduce calendar related correction factors (eg. Easter, Interview interval, January start date effects) in future seasonal adjustment process if the effects have been found to be statistically significant.

The situation with regard to supplementary surveys is less clear. A supplementary survey effect is already taken into account in seasonally adjustment, and will be removed from seasonally adjusted estimates if this supplementary survey is conducted on the same month every year. However, if a survey is not conducted regularly, its significant effect will not only effect seasonal factor estimation but it also remains in the seasonally adjusted estimates which can be misinterpreted as a real world labour market change.

The investigations have identified a number of supplementary surveys which appear to have a statistically significant impact on the collected data. What is lacking though is a plausible explanation as to why particular supplementary surveys impact on the data in particular ways. Lack of replication also makes it difficult to validate some of the effects identified in the model. The findings from the investigation need to be looked at further to see if any such explanations can be found.

In the interim, we propose to correct the significant supplementary effects for purposes of better seasonal factor estimation only. In other words, the estimated effect will be retained in the seasonally adjusted estimates.

With regard to the new survey redesign effects the results from the investigation are also somewhat inconclusive. Whilst certain of the survey redesign effects that were investigated were found to be statistically significant, it is also true that removing these effects did not remove all of the increase in the volatility of the data at the current end. It appears that there may be some other as yet unidentified effect in play which has caused the volatility of some of the series to increase in the last few years. A detailed analysis on this issue has been conducted and will not be discussed here in detail. Investigations into this issue by various areas of the ABS are continuing.

Due to limited data available from the new survey form, a strict adherence to modelling and seasonal adjustment quality control statistics is not advisable when deciding whether or not an intervention represents a seasonal shift. Rather, prior information is often crucial in identifying the nature of a suspected pattern changes. Unfortunately, information on such changes is not enough or the changes may or may not be permanent, depending on many factors. In the meantime it would seem rather premature to apply the new survey redesign effects that were identified as significant by this study. We suggest no correction should be implemented until more data (at least three years worth of data under new survey) is available for further assessment.

8. Conclusion

The current LFS seasonal adjustment had calendar-related effect and supplementary limitations which could affect the accuracy of seasonal factor estimation and consequently also affect the seasonally adjusted estimates. This research conducted at ABS has resulted in the development of a more refined seasonal adjustment process. With this refinement, the volatility of the seasonally adjusted estimates can be somewhat reduced and provide improved measurement of underlying economic trends.

RegARIMA modelling techniques are used to estimate the calendar and supplementary effects. Modelling information criterion (AICC), t-statistics and joint tests for inclusion of the intervention (exploratory) variables provide supporting evidence for the 6 Australia level employment aggregates.

We recommend that

1. All the identified calendar related (Easter, January interview date) effects should be corrected in the seasonal adjustment process and remove them from seasonally adjusted estimates.
2. All the identified supplementary survey effects should be further investigated for the their possible reason and justification. Prior correction should be made for seasonal factor estimation only. The effects will be presented in the seasonally adjusted estimates until a final decision can be made that excluding them from seasonally adjusted estimates is justified.
3. For lower level aggregations, the prior correction factor should be derived from the more reliably estimated prior correction from the 6 Australia level aggregates.
4. Although the new survey design effects have been identified, an accurate assessment cannot be made because the new survey time series are of short span. No correction for the new survey design effect should be implemented in very near future unless other information can be used in identifying the nature of the suspected intervention.

At least three years worth of post new survey data is needed to accurately assess the seasonal pattern changes using the current X-11 with RegARIMA modelling techniques. ABS is currently conducting a research project to estimate seasonal factors from a short time series span by using multi-level modelling methodology. This method potentially can be used to estimate seasonal factors using post new survey time series.

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Appendix 1: Constructed regressors for the intervention analysis

Start date effect
One possible calendar related effect resulting from this survey methodology is that there may be a relationship between the start date of the interview period and the resulting estimates. This could be for example due to seasonality which varies throughout the year, so that slightly different levels of seasonal activity are measured as the survey timing shifts relative to the start of each month from one year to the next. In addition there appears to be a start date effect which impacts particularly in the month of January and affects some series.

The start date regressor was constructed by noting that since the start date of the survey period typically ranges over the days between the 6th of the month and the 12th of the month inclusive, the middle date in this range is the 9th of the month. Then the actual start date was coded relative to the 9th, so if the survey period started on the 9th the regressor value is 0, if on the 8th the regressor value is -1, if on the 7th the regressor value is -2, if on the 10th the regressor value is +1, etc.

January Start Date effect
The January interview period falls close to the Christmas/New year holiday period. Employment conditions change markedly after this period, so a changing start date for January may be significant. The Interview period for January is later than other months due to the fact that the holiday period could cause high non-response rates. Therefore, the changing interview date could lead to differing coverages of the survey.

The start date regressor for January was constructed by noting that since the start date of the survey period typically ranges over the days between the 8th of the month and the 14th of the month inclusive, the middle date in this range is the 11th of the month. Then the actual start date was coded relative to the 11th, so if the survey period started on the 11th the regressor value is 0, if on the 10th the regressor value is -1, if on the 9th the regressor value is -2, if on the 12th the regressor value is +1, etc. The regressor is set to 0 for non January months.

Interval effect
A consequence of the way the LFS is conducted is that there are always a whole number of weeks between successive surveys, most commonly 4 or 5 weeks although the adjustments in December and January can result in other spacings between successive interviews. If there is trend growth (or fall) in the activity being measured then the difference between successive data values might be expected to be larger if there is a 5 week gap than a 4 week gap.

The interval regressor was constructed by first calculating the length of an average month. This is equal to 365.25/12 = 30.4375 days, and is the average interval between successive surveys. Then the regressor is constructed by subtracting the average interval (in days) from the actual interval (in days). So if the gap between successive surveys is 4 weeks (4 * 7 = 28 days), the regressor takes the value (28 - 30.4375) = - 2.4375, whereas if the gap is 5 weeks the regressor would take the value (35 - 30.4375) = 4.5625, etc.

Easter effect in April

The timing of Easter in relationship to the interview fortnight can impact the estimates for some series in the month of April. Five different timings of Easter Monday in relation to the start of the survey fortnight for April are possible, namely that Easter Monday is a week before the survey period (Type 1), Easter Monday coincides with the start of the first week of the survey reference period (Type 2), Easter Monday coincides with the start of the second week of the survey reference period (Type 3), Easter Monday immediately follows the end of the reference period (Type 4), and Easter Monday falls a week after the end of the reference period (Type 5). The following diagram shows the situation schematically.

```
  Timing of Easter Monday

 Type 1         Type 2         Type 3         Type 4         Type 5
 |              |              |              |              |
 V              V              V              V              V
|M T W T F S S|M T W T F S S|M T W T F S S|M T W T F S S|M T W T F S S|
              |<-Survey reference period->|
```

It can be seen that there are no public holidays in the survey reference period for types 1 and 5, that there is 1 public holiday for type 2 (Easter Monday), that there are 3 public holidays for type 3 (Friday, Saturday and Monday), and that there 2 public holidays for type 4 (Friday and Saturday).

Since the number of public holidays in the survey reference period is not monotonic increasing or decreasing with type, the relationship between effect and type is not expected to be linear. Consequently each type of April Easter was treated as a separate categorical variable. A regression vector was constructed for each type, consisting of all 0's except for months where an April Easter of the specified type has occurred. In those months the regression variable takes the value of 1.

Supplementary survey effects
One of the features of the LFS is that respondents may be asked supplementary survey questions on a range of topics, in addition to the standard LFS questions. It appears that the asking of supplementary survey questions can influence the responses given to the LFS questions in some circumstances.

For each supplementary survey a regressor was constructed which consisted of 0's where the supplementary survey was not run and non-zero values where the supplementary survey was run. The number of rotation groups asked the supplementary survey was used to obtain the non-zero regressor values. For example, if 4 rotation groups were asked the supplementary survey questions, the effect on the data would be only half as big as would be the case if all 8 rotation groups were asked the supplementary survey questions. Therefore, if all 8 groups were asked the supplementary questions the regressor value is 1, if 7 rotation groups were asked the regressor variable takes on the value of 7/8, if 6 rotation groups were asked the regressor variable is 6/8, etc.

The supplementary survey regressors constructed as described above are suitable for the RegARIMA regressions, however some of the supplementary surveys have been run in the same month for several years, or in the same month every second year. This results in leakage of the supplementary survey effect into the seasonal factors when using the D13 method. To allow for the leakage, the supplementary survey regressors were modified in the following way.

1. Produce a indicator series for the supplementary survey, as you would for the Reg-ARIMA method. An example series is (0, 0, 0, 0, 0, 0, 0, 1, 1, 1, 1, 1, 1, 1). (For space purposes I have produced a yearly series, with only the effected month included).

2. Produce a seasonal series based on this indicator series, by applying the SMA to the indicator series. The example seasonal series would then become (0, 0, 1/27, 1/9, 2/9, 3/9, 4/9, 5/9, 6/9, 7/9, 8/9, 26/27, 1, 1). This vector gives the ammount of the effect, if present, that has "leaked" into the seasonal factor.

3. Produce the regressor series by subtracting the seasonal series away from the original series. The example regressor series is (0, 0, -1/27, -1/9, -2/9, -3/9, -4/9, 4/9, 3/9, 2/9, 1/9, 1/27, 0, 0)

This method was not applied across all supplementary surveys, only those which were conducted consistently. For example, if a survey was run in 02/93, 02/97, 02/01, the D13 regressor was the same as the RegARIMA regressor. The exception was for the weakly earnings of employees survey, which was not run in August 1991, and therefore the regressor (for the month of August) was (...., 0, 0, 0, 0, -1, 0, 0, 0, ...), with the -1 in the August 1991 position.

LFS survey questionnaire effects
The questions asked in the LFS were altered starting in April 2001, and a slightly different interview regime was introduced whereby in one month in three (Feb, May, Aug, Nov) a more extended set of questions is asked than in the remaining months.

Two types of possible impact from the introduction of the new survey were considered. The first type of impact to be tested for was a level shift effect, whereby the underlying level of particular LF series may shift up or down abruptly at the point in time where the new survey was introduced. This could happen if for example survey respondents interpreted questions slightly differently in the old and new surveys due to a change in wording between the two surveys.

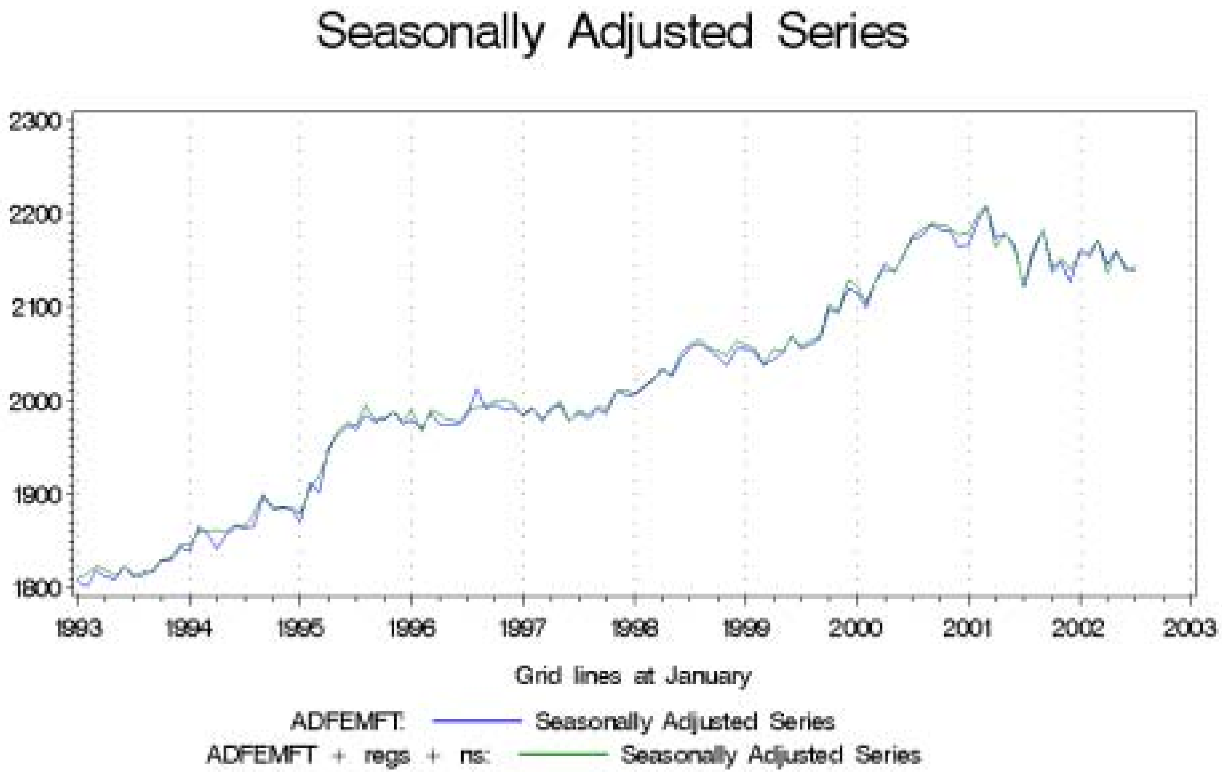
The other type of impact that was investigated was the possibility that the new survey may have a particular pattern of seasonal variation in the collected data that was not present in data from the old survey. The reason for this is that the use of different sets of questions in different months could have an impact on the responses obtained.

The seasonal adjustment factors used to adjust the LF series are mainly representative of the seasonality of the old survey rather than of the new survey. If the new survey does have a different seasonal pattern then the seasonal adjustment factors may well fail to remove all of the seasonal pattern of the new survey data. The remaining seasonality can manifest itself as an apparent increase in the volatility of the adjusted series. Under the long form/short form hypothesis, the seasonal level of the long form months would be either raised or lowered relative to the seasonal level of the short form months. In other words the long form/short form effect (if present) should impact on the seasonal pattern in a specific way that can be tested for.

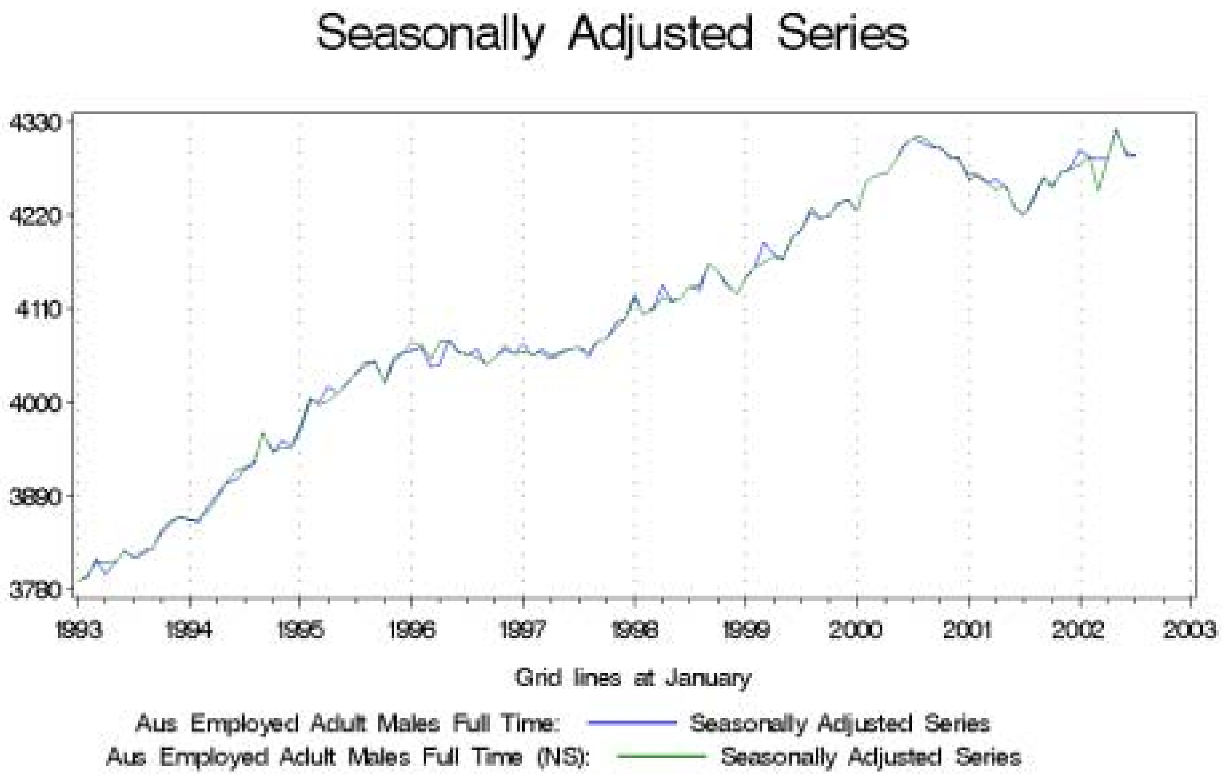
The level shift regressor was constructed as a vector containing all 0's from the start of the series to March 2001 and all 1's from April 2001 to the present. The short form/long form seasonal effect regressor was constructed as a vector containing all 0's from the start of the series to March 2001, followed by the repeating sequence 1, -2, 1, etc. In other words the regressor variable is 1 for the months when the short survey is run and -2 for the months when the long survey is run. Since this regressor vector has mean zero no level shift effect will leak into the seasonal effect regression coefficients.

Appendix 2: Seasonally adjusted series before and after correction for calendar related effects, supplementary survey effects and LFS redesign effects.

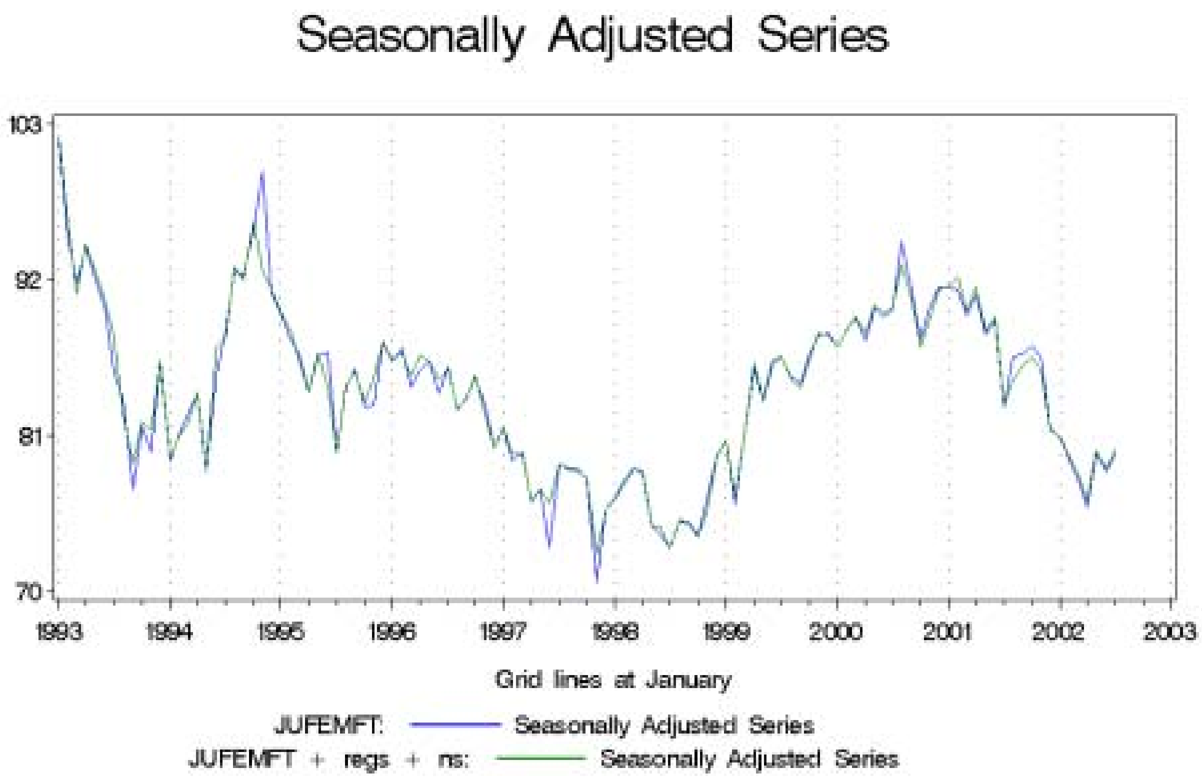
Adult Females FT



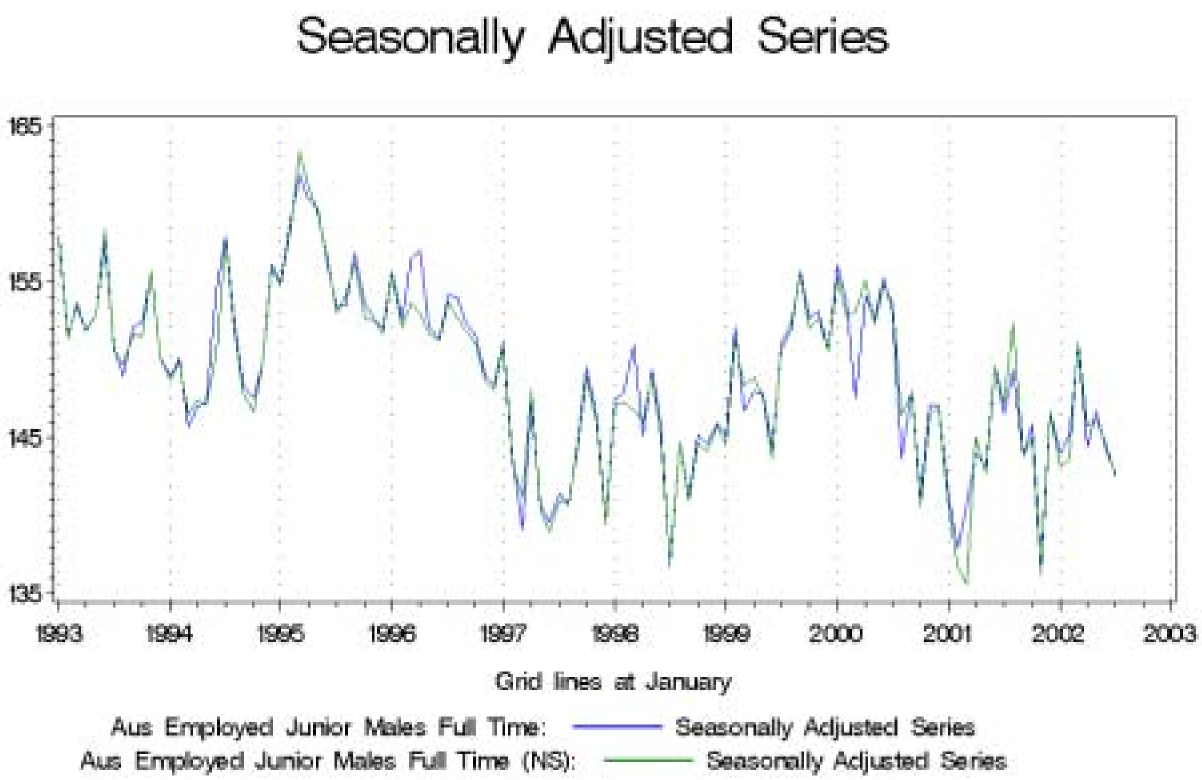
Adult Males FT



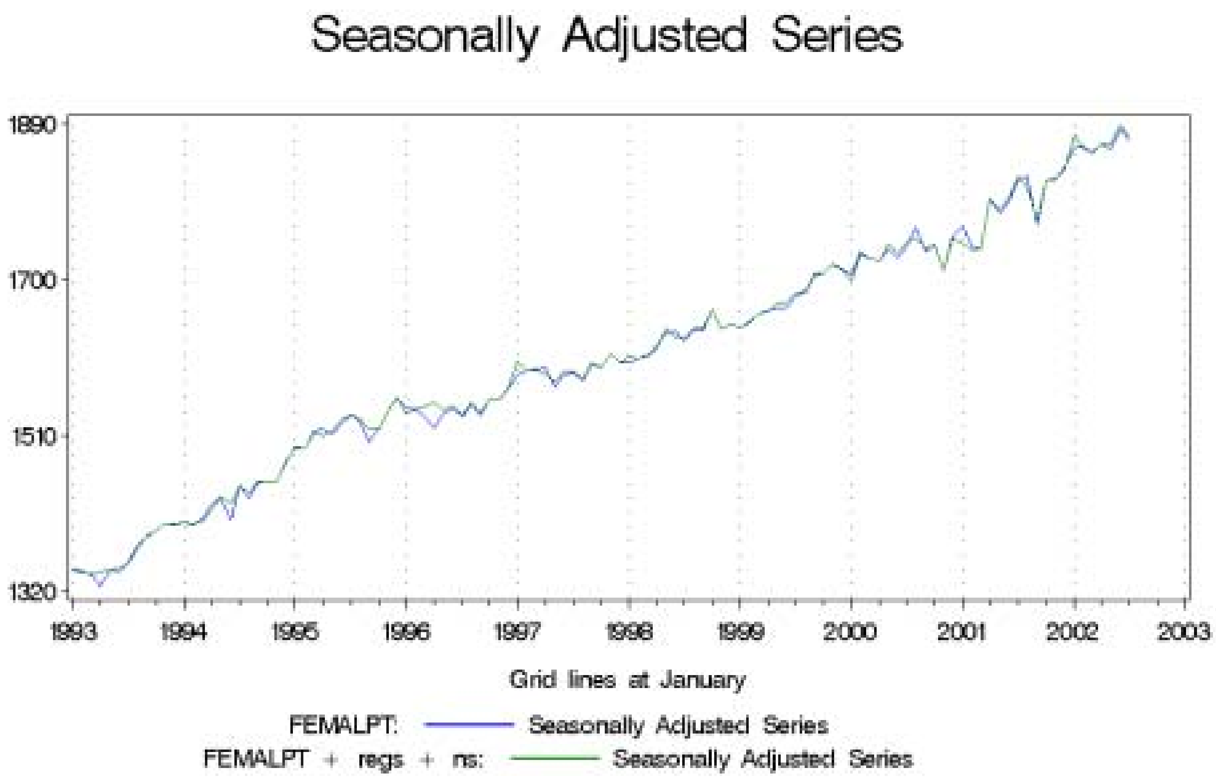
Junior Females FT



Junior Males FT



Females PT



Males PT