An Assessment of EI and SA Reporting in SLID

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ABSTRACT

This study assesses two potential problems with respect to the reporting of Employment Insurance (EI) and Social Assistance (SA) benefits in the Survey of Labour and Income Dynamics (SLID): (a) under-reporting of the monthly number of beneficiaries; and (b) a tendency to incorrectly report receiving benefits throughout the year, while in fact benefits may have been received only in certain months, leading to artificial spikes in the January starts and December terminations of benefit spells (seam effect). The results of the analysis show the following:

(1) The rate of under-reporting of EI in SLID is about 15%. Although it varies by month (from 0% to 30%), it is fairly stable from year to year.

(2) There are significant spikes in the number of January starts and December terminations of EI benefit spells. However, the spikes in January starts appear to represent a real phenomenon, rather than a seam problem. They mirror closely the pattern of establishment of new EI claims (the latter increase significantly in January as a result of the decline in employment following the Christmas peak demand). There are no corresponding statistics for EI claim terminations to assess the nature of December spikes.

(3) The rate of under-reporting of SA in SLID is about 50%, significantly greater than for EI. The rate of under-reporting goes down to about 20% to 30%, if we assume that those who received SA, but did not report in which months they received benefits, received benefits throughout the year.

(4) There are large spikes in the number of January starts and December terminations. As in the case of EI, the SA could reflect a real phenomenon. After all, SA starts and terminations are affected by labour market conditions, in the same way EI starts and terminations are affected. However, the SA spikes are much larger than the EI spikes, which increases the probability that these, at least in part, are due to a seam effect.

1. Executive Summary

The objectives of this study are to assess two potential problems with respect to the reporting of Employment Insurance (EI) and Social Assistance (SA) in the Survey of Labour and Income Dynamics (SLID), and propose ways of overcoming them:

- under-reporting of the number of beneficiaries and amount of benefits received;
- a "seam effect" which arises from a tendency to incorrectly report receiving benefits throughout the year, while in fact benefits may have been received only in certain months, leading to artificial spikes in the January starts and December terminations of benefit spells (seam effect); this problem distorts the length of benefit spells and the sequencing of different types of benefits.

2. Findings

a) Employment Insurance

The results of the analysis show the following:

EI under-reporting:

- The rate of under-reporting of EI in SLID is about 15%. Although it varies by month (from 0% to 30%), it is fairly stable from year to year.
- The extent of under-reporting of EI benefits payments in SLID is about 10%, relative to the Human Resources Development Canada (HRDC) administrative data.

EI seam effect:

- There are significant spikes in the number of January starts and December terminations of EI benefit spells.
- However, the spikes in January starts appear to represent a real phenomenon, rather than a seam problem; they mirror closely the pattern of establishment of new EI claims, which increase significantly in January as a result of the decline in employment following the Christmas peak demand.
- The spikes in December terminations also most likely represent a real phenomenon. The spikes can be explained by the rise in seasonal jobs in December, resulting in EI beneficiaries finding employment and leaving EI. However, because we do not have administrative data to compare with, it is not clear to what extent the December spikes represent a real phenomenon and to what extent a seam problem.

b) Social Assistance

SA under-reporting:

- The rate of under-reporting of SA in SLID is about 50%, significantly greater than for EI. The rate improves to about 20% to 30% if we assume that, those who received SA, but did not report in which months they received benefits, received benefits throughout the year.
- The amount of SA benefits reported in SLID is about two-thirds of the amount of provincial administrative data.

SA seam effect:

- There are large spikes in the number of January starts and December terminations. As in the case of EI, the SA could reflect a real phenomenon. After all, SA starts and terminations are affected by labour market conditions, in the same way EI starts and terminations are affected.
- However, there are at least two reasons why it is reasonable to conclude that to a large extent the SA spikes represent a seam effect:
  a) the SA spikes are much larger than the EI spikes; and
  b) the monthly pattern appears suspicious: for example, the majority of respondents showing a December 1996 termination of SA benefits are reporting benefits in all the months in 1996 and no benefits in any of the months in 1997; the suspicion is that when individuals receive SA in most months of the year, they tend to say that they received SA in all the months; conversely, when they do not receive SA in most of the months of the year, they tend to report that they did not receive SA at all.

3. Recommendations

a) Employment Insurance

The quality of the EI data in SLID is fairly good: there is a high reporting rate, and no evidence of seam effect. Therefore, there are no overwhelming reasons for any drastic measures here. However, some of the changes recommended below with respect to SA would improve further the reporting of EI.

b) Social Assistance

By contrast, SA is subject to significant under-reporting of benefits and pronounced seam effects. A number of steps can be taken to improve the reporting of SA:

- Imputations: One way of addressing the under-reporting problem is by imputing months and benefit amounts based on the personal characteristics and labour force status of the respondent.
- Data linking: A more satisfactory approach is to expand the concept of data linking and link SLID not only to income tax data, but also to provincial SA records (the same suggestion applies to EI data, although the urgency is less than in the case of SA). Data linking would also deal with the seam problem.

- **Dependent interviewing**: At the present time, the dependent interviewing questions with respect to SA (and EI) are not truly dependent interview questions. They are simply used to put the respondent in the right frame of mind, but are not being used to improve the quality of the data:
  a) in the case of those who reported receiving benefits in December of the previous year, if the information is contradicted in the next year no action is taken; and
  b) in the case of those who did <u>not</u> report receiving benefits in December of the previous year, no dependent interviewing questions are asked at all.

  It will make sense to:

  a) apply dependent interviewing questions to <u>all respondents</u>—i.e. even if respondents reported in January 1997 that they did not receive any SA make sure they agree with the information they provided last year about benefits in December, regardless if they said they had received benefits in December or not;
  b) if there is a conflict in the dependent interviewing questions, an attempt should be made to <u>resolve the conflict</u>—both during the interview (through further probing) and through editing of the responses later on; this is particularly important if the conflict in questions can lead to a seam effect; for example:
     - suppose that respondents reported in January 1997 that they received SA benefits in December 1996;
     - then, suppose that in the next interview (on January 1998) they dispute that they received SA in December 1996 and also report no SA in 1997; in the absence of no resolution of the two conflicting answers, it would appear that an SA claim was terminated in December 1996 (causing a seam effect).
- *Income editing:* With respect to the adjustment and imputation, given that there is an under-reporting of EI (as well as SA and WC), it will make sense to be more generous. In particular, if there is evidence from any source (January interview, May interview, or tax return) that respondents received EI, then it should be assumed that they received EI. Consequently, the amount of benefits and monthly pattern of benefits if missing should be imputed.

1. Introduction

There is considerable policy interest on the impacts of the recent changes in Employment Insurance (EI) and social assistance (SA). The Survey of Labour and Income Dynamics (SLID) is particularly well suited for this type of analysis, because it follows a panel of individuals throughout the recent changes (1993-97).

The SLID data have not yet been used extensively to study topics related to EI and SA. There are two potential concerns about the quality of the SLID data:

- Under-reporting: Virtually all surveys that deal with government transfers (e.g. Survey of Consumer Finances) under-report government transfers. Particularly concerning is the possibility that the extent of under-reporting may change from year to year, or that it may relate systematically to respondent characteristics.
- "Seam" effect: A "seam effect" arises from a tendency to incorrectly report receiving benefits throughout the year, while in fact benefits may have been received only in certain months, leading to artificial spikes in the January starts and December terminations of benefit spells (seam effect); this problem distorts the length of benefit spells and the sequencing of different types of benefits. It is known from other longitudinal surveys that respondents may report receiving a government benefit throughout the year while, in fact, they may have received it only in certain months. A similar problem has been found in SLID with respect to job spells (see Cotton and Giles, 1998).

The above situation would lead to artificial spikes in the number of January starts and December terminations of benefit spells. At the same time, it would distort the length of benefit spells, as well as the sequencing of different types of benefits (EI and SA in this case). In order to correctly assess the interaction between EI and SA, it is important that we are confident that the length and sequence of benefit spells is being correctly recorded in SLID.

The objectives of this study are to assess the extent of underreporting and "seam" effect in the reporting of EI and SA in SLID and propose ways of overcoming them.

We begin by examining the accuracy of the reporting of EI in SLID, first the issue of under-reporting (Section 2) and then the issue of seam effect (Section 3). Then we examine the same two issues with respect to SA (Sections 4 and 5). The conclusions are summarized in the executive summary.

2. EI Under-Reporting

2.1 Analysis

In this section we analyzed each annual cross-sectional sample, and compared the monthly incidence of EI benefits and the annual amount of EI payments to administrative data. Our comparators were the following:[1]

- monthly number of EI beneficiaries (source HRDC's EI data, available through CANSIM);
- annual number of EI beneficiaries, i.e. number who received benefits in at least one month during the year (source Revenue Canada's T1 file, available through their web site);[2] and
- annual EI payments (source: HRDC and Revenue Canada; the Revenue Canada figures are lower than the HRDC figures because not all EI beneficiaries are tax filers).

2.2 Findings

(a) Monthly Number of EI Beneficiaries

Chart 1A compares SLID estimates of the monthly number of EI beneficiaries to the number of EI beneficiaries reported by HRDC (in the 10 provinces only), while *Chart 1B* shows the ratio of SLID estimates to HRDC figures. The comparison shows that:

- The monthly number of EI beneficiaries in SLID ranges from 75% to 100% of the number of beneficiaries reported in the HRDC administrative data.
- Both the HRDC data and SLID data have a seasonal pattern. However, the SLID pattern is flatter than the HRDC pattern. As a result, the low points of the HRDC pattern come close to the SLID pattern and, at those points, the monthly number of EI beneficiaries in SLID comes very close to the HRDC figures.

In summary, over the period 1993-97, the number of EI beneficiaries in SLID is under-reported by about 15%. The extent of under-reporting varies by month. However, on an annual basis, the extent of under-reporting is fairly stable over time.

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[1] Our analysis followed these steps:
  a) For each year, we selected respondents with positive cross-sectional weights for the corresponding year and complete labour market information (ILBWT26>0).
  b) Then we weighted responses using each years cross-sectional, complete labour information weights (ILBWT26).
  c) We used variables COMPTYPE and MTHRCV14 to identify in which months an individual received EI.
  d) Finally, we used variable UIBEN42 to determine the annual incidence of EI benefits and the amount of EI benefits received in a year.

[2] The Revenue Canada data tend to understate the incidence of EI since not every EI recipient files an income tax return. One could estimate the annual incidence of EI from the HRDC data; however, these data are not published regularly and will require analysis of the EI microdata (EI Status Vector file).

Chart 1A: Monthly Number of EI Beneficiaries
(SLID Estimates vs. HRDC Admin Data)
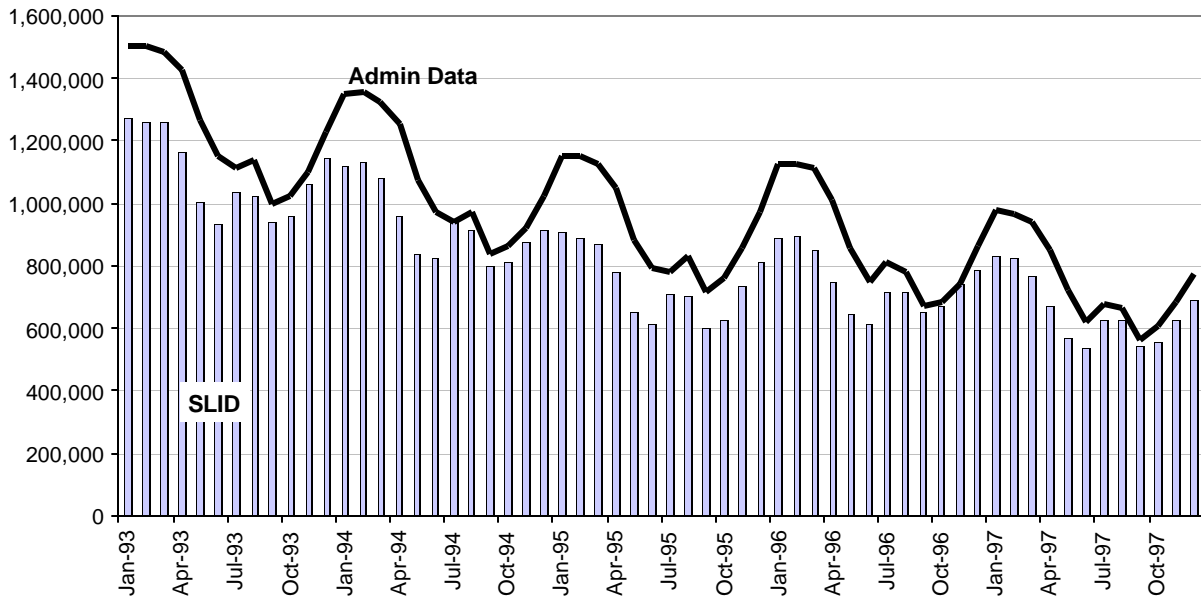
Admin Data

SLID

Chart 1B: Monthly Number of EI Beneficiaries
(SLID Estimates as a % of HRDC Admin Data)
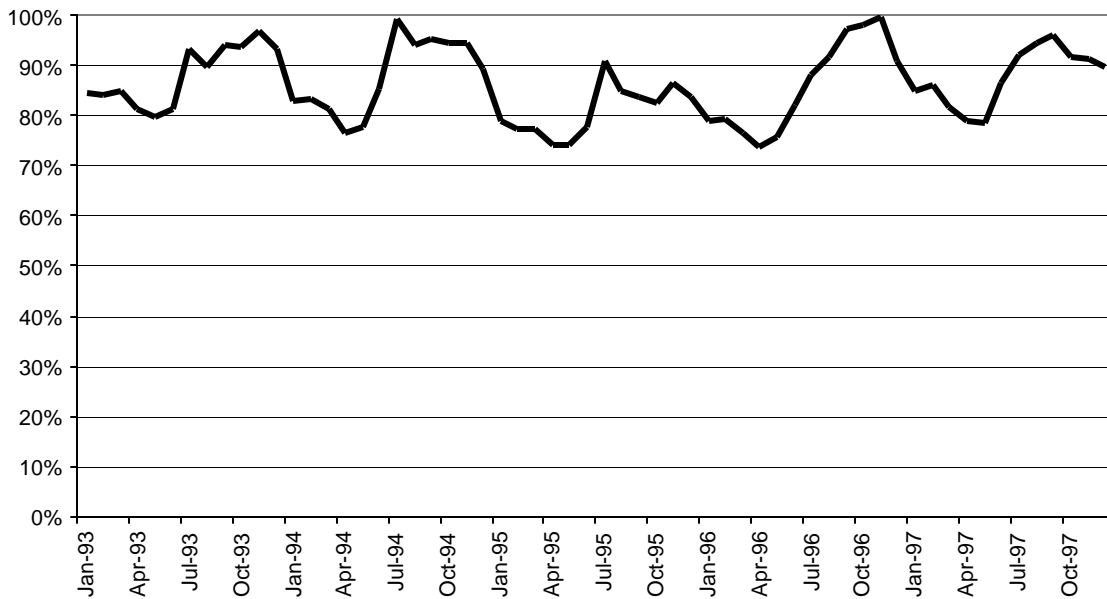
(b) Annual Number of EI Beneficiaries

Charts 2A and 2B compare SLID estimates of the annual number of EI beneficiaries to the number of income tax filers reporting EI income. According to this comparison, SLID fares very well. The reporting rate was 93% in 1993 and has risen to 105% by 1997.

The high reporting rate is largely explained by the fact that SLID respondents are asked for their permission to link their questionnaire to their tax file. The percentage of respondents who consent has increased over time, and it stands now at about 75%.

The fact that the reporting rate (vis-a-vis the tax data) has exceeded 100% may reflect the fact that some EI beneficiaries have not filed a tax return. Also, it may be related to the introduction of the second SLID panel in 1996.

(c) Annual EI Payments

Charts 3A and 3B show that the reporting of EI benefits relative to the income tax data is around 100%, similar to the reporting rate observed in the previous section with respect to the annual number of EI beneficiaries. Relative to HRDC's EI data, SLID's reporting rate of EI benefits is about 90%

Chart 2A: Annual Number of EI Beneficiaries
Income Tax Data vs. SLID Estimates
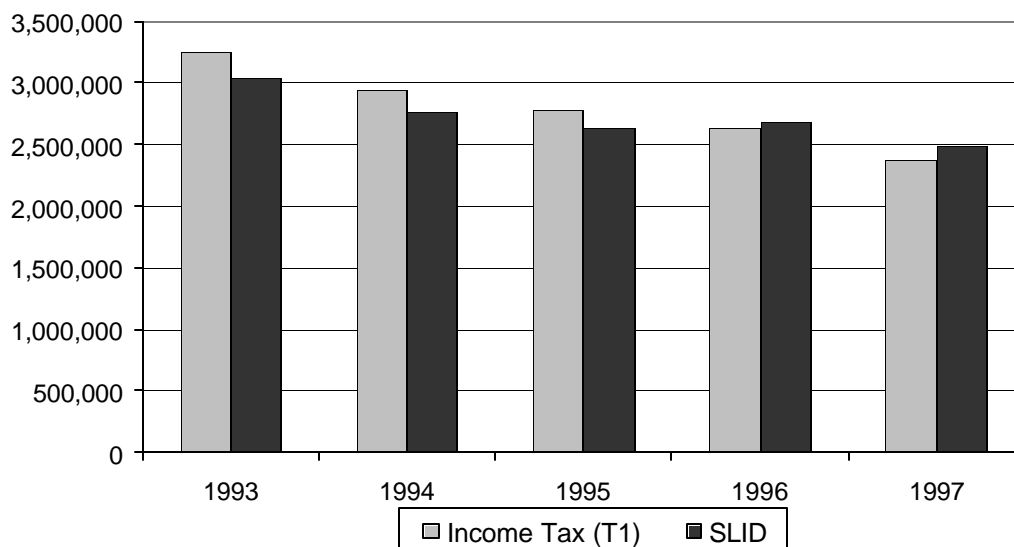
Note: The tax data include the territories.

Chart 2B: Annual Number of EI Beneficiaries
(SLID Estimates as a % of Income Tax Estimates)
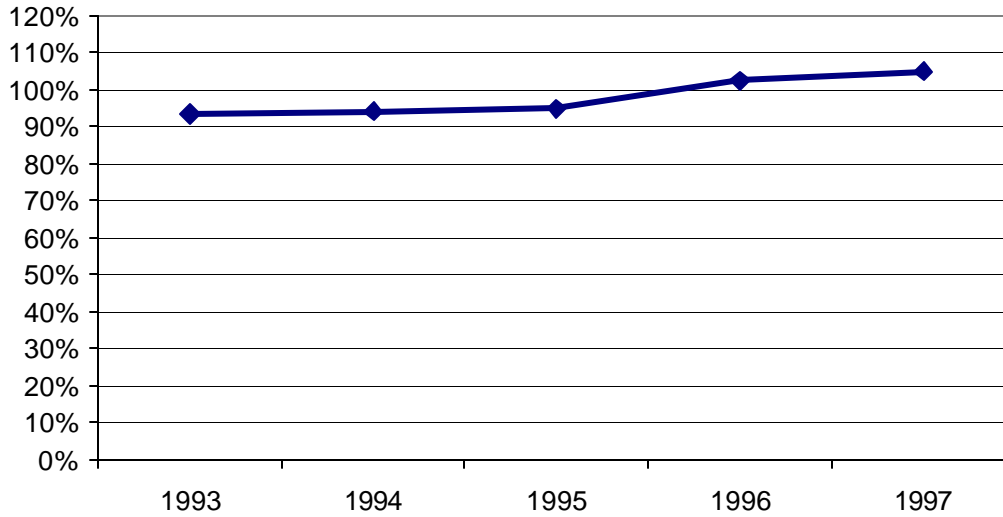
Note: The tax data include the territories.

Chart 3A: Annual EI Payments
SLID vs. HRDC Data vs. Income Tax Data
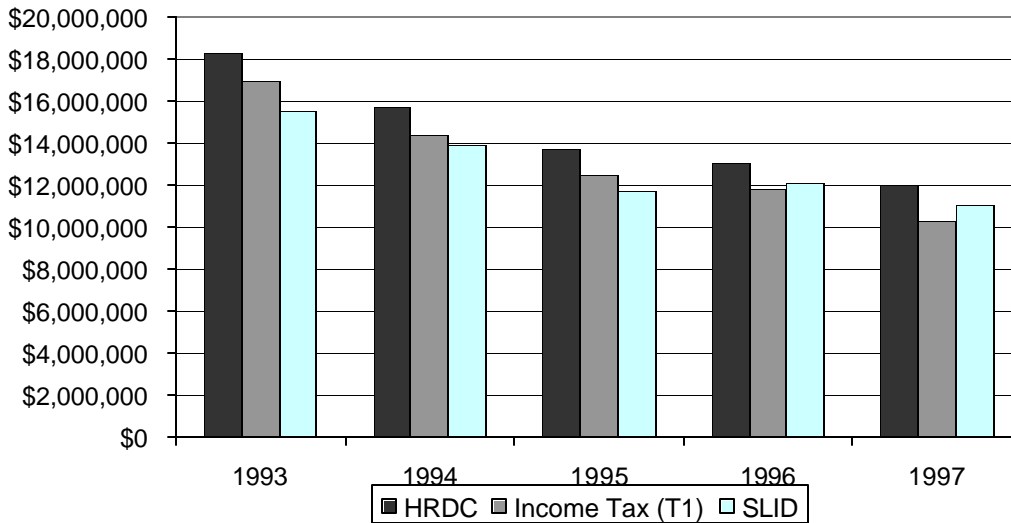
Note: The tax data include the territories; the HRDC data do not.

Chart 3B: Annual EI Payments
SLID as a % of HRDC Data & Income Tax Data
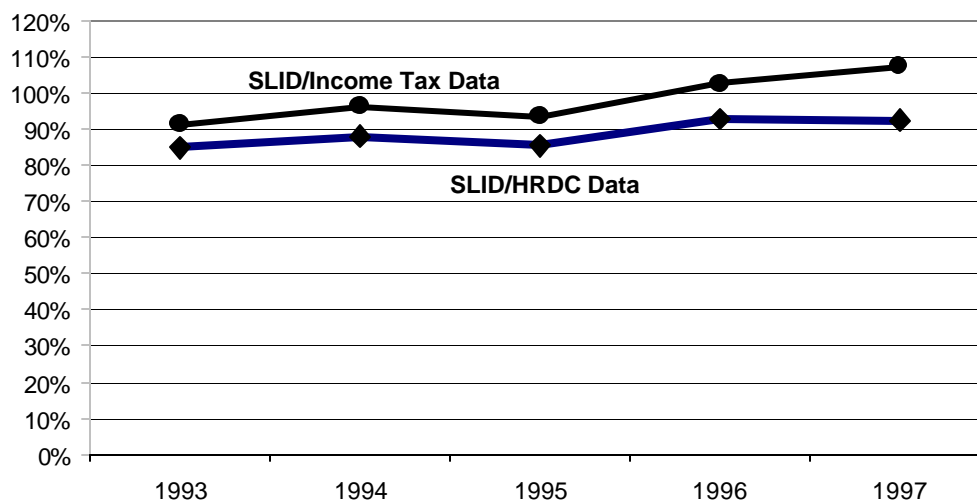
Note: The tax data include the territories; the HRDC data do not.

2.3 Conclusion

The annual incidence of EI and annual EI payments are fairly accurately represented in SLID. So it may not be necessary to make any adjustments here.

The monthly incidence presents a bit of a problem because the under-reporting is higher (about 15%) and it also varies by month. The probable causes of the problem are:
(a) respondents may report EI, but may not always be sure in which months they received EI; or
(b) information about the receipt of EI is obtained from the tax file of the respondent, but there is no corresponding information of the monthly pattern in the January interview.

Among the suggestions that could be considered:
- With respect to annual incidence of benefits and beneficiaries there is no serious problems and no action is required.
- With respect to the monthly incidence, the rate of under-reporting is rather low (about 15%) and has remained stable over time. We have two options:
  (a) The first option is not to take any action at all and use the SLID data as they are.
  (b) The second option is to impute a monthly pattern for those who received EI during the year, but for whom the monthly pattern of benefits is not known. If we take the extreme approach of assuming that these individuals received benefits in all months, then the monthly incidence will exceed substantially the actual monthly incidence (*Chart 4*). Therefore what is required is a more careful imputation to close the gap between the current SLID monthly pattern and the actual pattern.[3]

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[3] The imputation methodology can be fine-tuned by linking SLID records to EI records (using name, birthday and address information); the linking would be used strictly for methodological reasons (to test the imputation logic) and no permanent analytical file will be created.

Chart 4: EI Beneficiaries: SLID Estimates vs. Admin Data
(After Treating "Do not Know" Cases as Receiving EI)
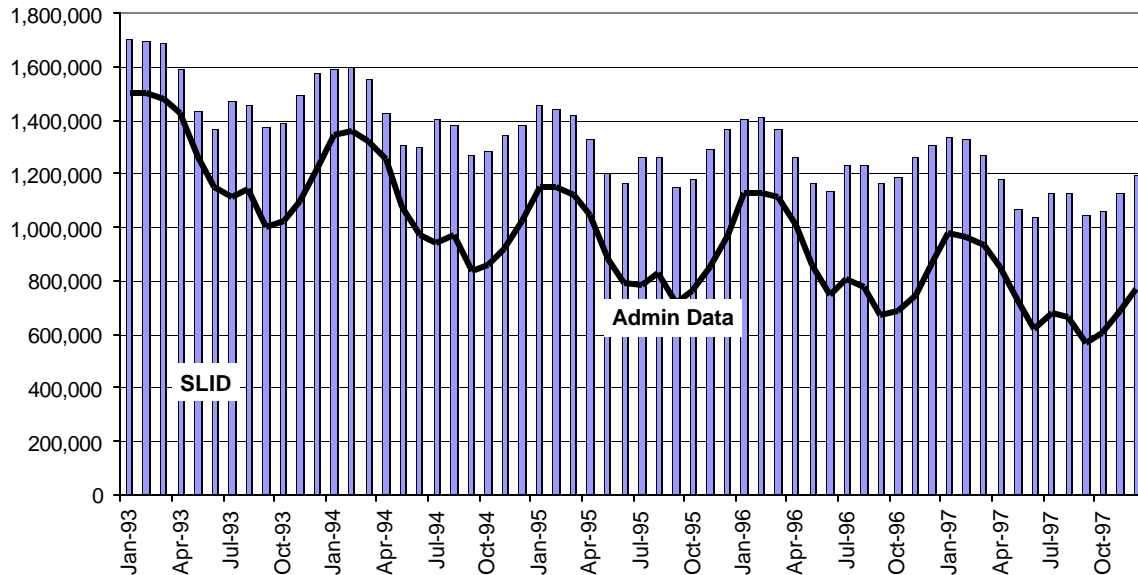
3. EI "Seam" Effect

3.1 Analysis

In this section we analyze the sample of longitudinal respondents, in order to examine the monthly pattern of starts and terminations of EI spells. We want to see whether in fact there is a tendency for a disproportionate number of EI spells to start in January or end in December, and how extensive is this problem.[4]

3.2 Findings

<u>Chart 5</u> shows that there are significant spikes in the number of January starts of EI spells. At the surface, this finding suggests the presence of a seam effect. However, examination of the establishment of new EI claims based on HRDC admin data also shows similar spikes.

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[4]   We followed the following steps:
   (a) We selected respondents with positive longitudinal weights (ILGWT26>0) and within scope (RESP99=1) in all five years (1993-97), who received EI or SA in any of the five years.
   (b) We weighted responses using the 1997 longitudinal weights.
   (c) We used variables COMPTYPE and MTHRCV14 to identify in which months an individual received EI.
   (d) We defined the start of an EI spell as a month with EI benefits preceded by a month with no EI benefits.
   (e) Similarly, we defined the end of an EI spell as a month with no EI benefits preceded by a month with EI benefits.

Based on this comparison, we conclude that the seam effect is not a serious issue with EI. In other words, the spike of EI starts in January reflect a real phenomenon: a jump in the number of EI claims following the increase in temporary employment related to the Christmas holiday.[5]

Chart 6 shows the monthly pattern of EI spell terminations. It shows that there is a spike in December (relative to the number of starts in the previous November of following January). This could be explained by the rise in seasonal jobs in December. However, because administrative numbers to compare with are not readily available, it is not clear to what extent the December spikes represent a seam effect or a real phenomenon.[6]

In any case, there are even bigger spikes in EI termination in other months. So in this sense, the December spikes are of relatively less concern. Whether all these spikes are real or not is difficult to judge unless one produces similar statistics from the EI administrative data.

Chart 5: EI Benefit Starts: SLID vs. HRDC Estimates

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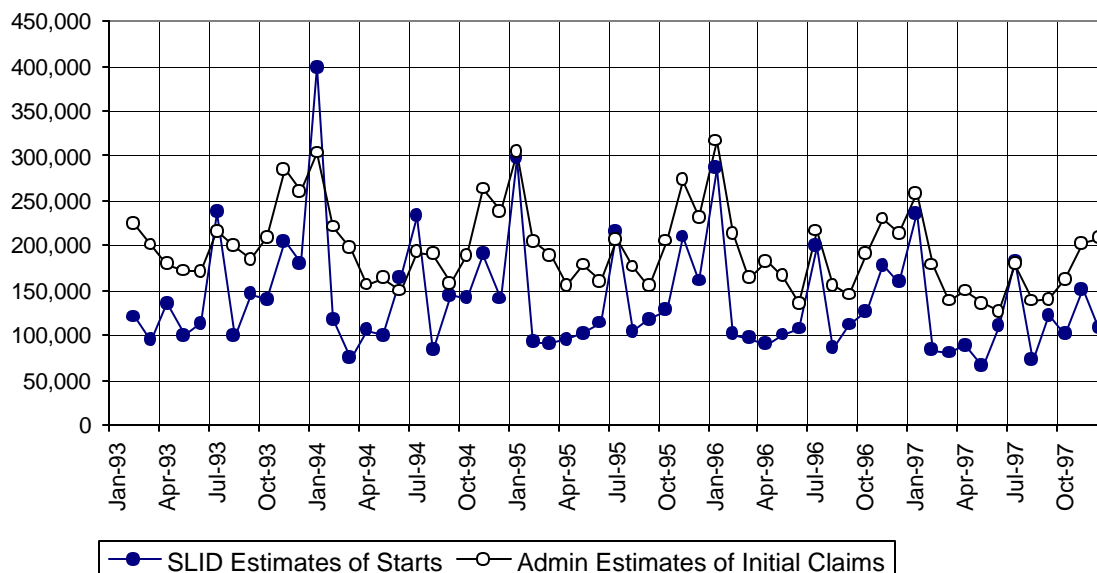
[5]   However, although the number of January starts in SLID is quite close to the actual number of starts, this is not the case for other months. In fact, for some months, SLID estimates are as low as half the number of starts of new claims according to HRDC records.

[6]   The required data can be produced from the HRDC administrative microdata.

Chart 6: EI Benefit Terminations: SLID Estimates
(No comparison to administrative data available)
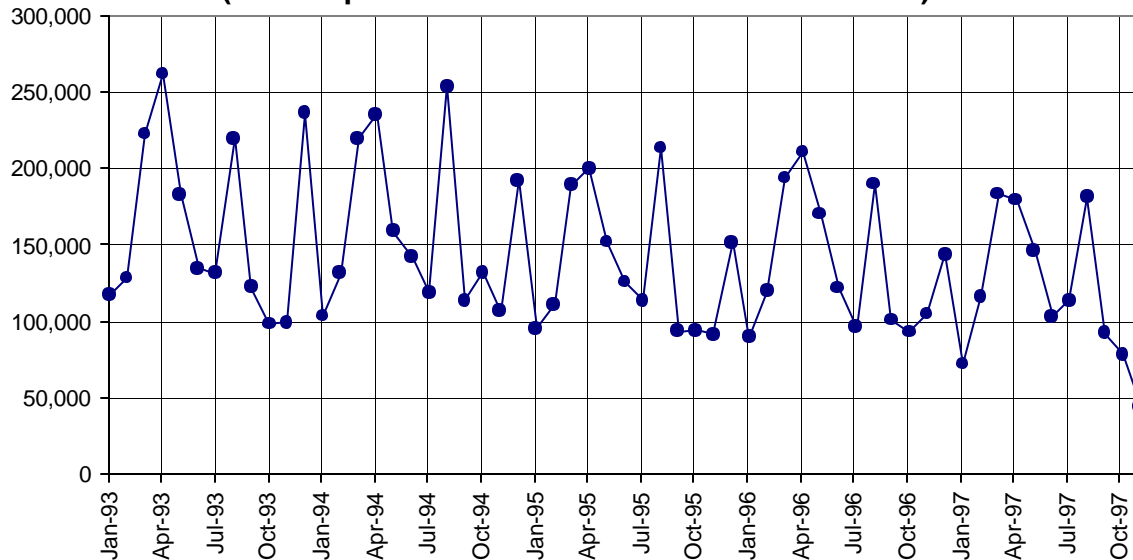
3.3 Conclusion

There does <u>not</u> seem to be a seam effect with respect to EI starts, and it is unclear whether there is one with respect to EI terminations.

Of more concern is the fact that the SLID pattern of EI starts deviates for certain months from the administrative counts. It is quite possible that the same problem exists with respect to EI terminations as well (but we do not have the necessary administrative data to test this).

Among the suggestions that could be considered:
- With respect to the seam effect, there is no evidence that it is present and, therefore, no action is required.
- Possibly, some imputations may be desirable to fine tune the overall monthly pattern (since it appears to deviate from the actual pattern in certain months).[7]

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[7] For example, in the case where an EI spell appears to start in January, if the individual is still working in the same job in January, the start of the EI spell could be pushed after the end of the job spell.

4. SA Under-Reporting

4.1 Analysis

In this section we conducted the same type of analysis as we did for EI in Section 2. We compare the SLID to administrative data as follows: [8]

- The number of individuals reporting SA was compared to the monthly provincial SA caseload. Typically one person within the family reported SA to SLID. So, in this sense, the two data sets are comparable.
- SLID estimates of total annual SA benefits were compared to provincial estimates of annual SA payments.

The analysis of the under-reporting issue (whether it refers to cases or benefit payments) is problematic because the concept of SA is not as clear as the concept of EI.

- In the case of EI, there is a single national program and there is no ambiguity whether a person receives EI or not.
- By contrast, SA programs differ among the provinces. Moreover, there is ambiguity with respect to the nature of some programs.[9]

4.2 Findings

In this section we compare the SLID estimate of SA applicants (i.e. individuals who reported SA—one individual per family unit) to the provincial caseload (one applicant per family unit).

The analysis shows that there is extensive under-reporting of the number of SA applicants in SLID (*Charts 7A and 7B*): The ratio of SLID estimates to the actual number of cases is just over 50%.

One factor behind the above result is that the SLID estimate excludes respondents who received SA during the year but did not report in which months they received SA. However, even after assuming that those who received SA in a year received it in all 12 months, the reporting rate is still significantly below 100% (it rises from 50% to 70%). In other words, even if we overstate the reporting of SA (by assuming that those who received SA in a year received it in all months of that year), we are still significantly below the number of cases reported in provincial administrative data.

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[8]   We used the following variables to identify receipt of SA:
     a) COMPTYPE and MTHRCV14 to identify in which months an individual received SA;
     b) SAPIS42 to determine the annual incidence of SA benefits and the amount of SA benefits received in a year; and
     c) Because SA is a family-based program (unlike EI which is strictly based on the individual's insurability) we also used variable FMSAV27 (from the DEC31FAM entity); this variable indicates in which months any member of the family received SA.

[9]   For example, Alberta's "Program for Independence" and "Children in Need" are income tested but it is not clear whether they should be considered as part of SA. Equally important, it is not clear how respondents think about such programs—i.e. whether they think of them as SA or not—which of course will affect their responses.

Similarly, with respect to annual payments there is considerable under-reporting of SA in SLID. *Chart 8* shows that the ratio of annual SA payments according to SLID, relative to the administrative data, is about two-thirds.

Chart 7A: Number of SA Applicants (one per family unit)
SLID Estimates vs. Provincial Caseload (one per family unit)
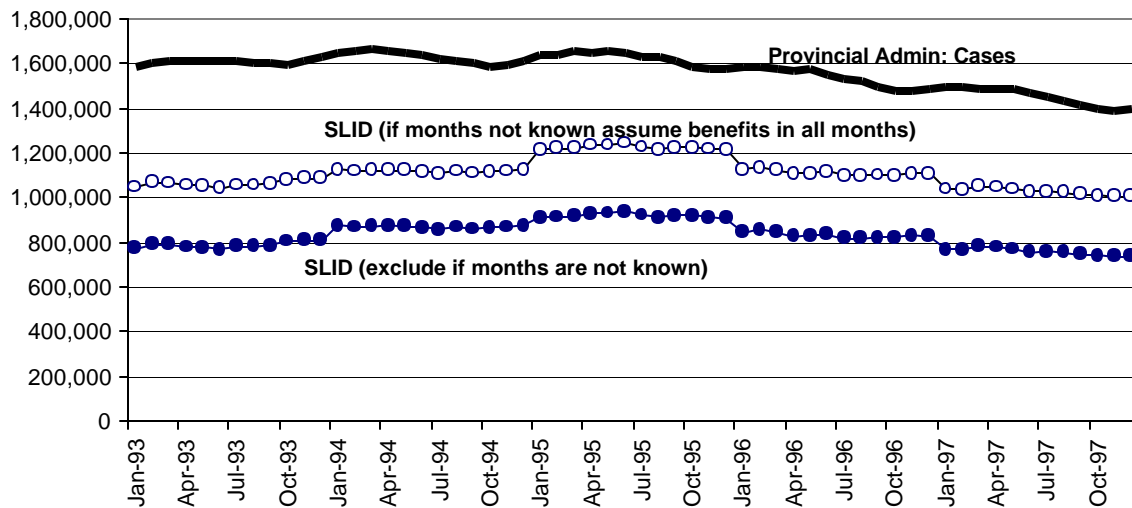
Chart 7B: Number of SA Applicants (one per family unit)
SLID Estimates/ Provincial Caseload
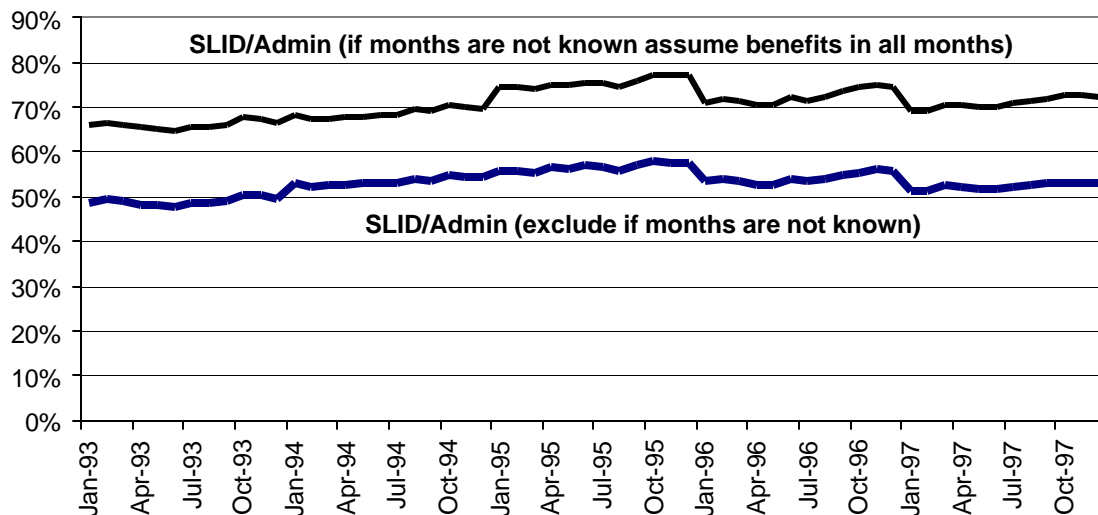
Chart 8: SA Benefit Payments
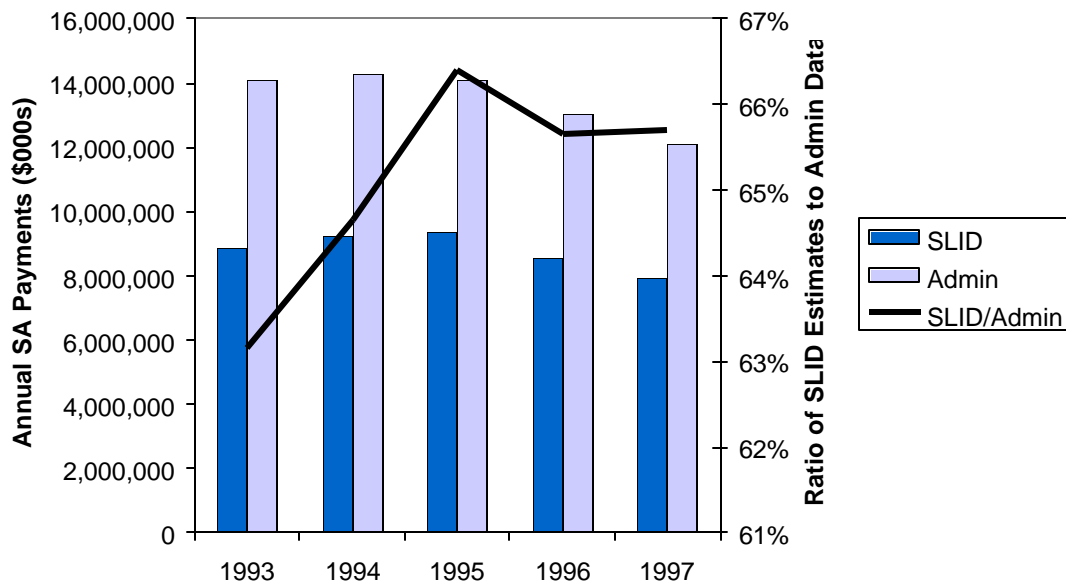
4.3 Conclusion

There is a significant under-reporting of SA in SLID (both in terms of monthly incidence and total amount of benefits received). The precise size of under-reporting depends on the comparator. The comparators used here were provincial data of caseload and benefit payments.

The above finding is not perhaps surprising since the same problem is common to the Survey of Consumer Finances. Part of the reason for the under-reporting of SA is the confusing nature of social assistance. Another reason may be that there is more stigma attached to receiving SA and greater reluctance to report it in a survey.

It should also be added that linking SLID to tax data is somewhat less effective in the case of SA than in the case of EI, since it is more common for SA recipients than EI recipients not to file an income tax return.

There is no easy solution to the above problem, short of linking SLID records to provincial SA records (in the same way SLID records are linked to tax records). Imputing a monthly pattern, for those who we know received SA but the months are not known, will improve the monthly incidence of SA. However, it will still remain significantly below administrative counts, since on an annual basis there is also considerable under-reporting.

5. SA Seam Effect

5.1 Analysis

In this section, we performed similar analysis to that performed for EI in Section 3, to see if January starts and December terminations of SA benefit spells are relatively more common.

5.2 Findings

The results of the analysis show that there are very significant spikes in December terminations and January starts of SA spells. The spikes were equally dramatic regardless if we looked at the number of SA applicants or the number of SA beneficiaries (i.e. individuals living in a family that one member is a SA applicant):

- *Chart 9A* shows that there are pronounced spikes in the number of January starts of SA applicant benefit spells.
- A small consolation is that the spikes have declined over time. In 1993 January starts were about eight times as common as other months in the year. By 1997, the ratio declined to about four.

Charts 10A and 10B show that there are similar spikes with respect to SA terminations. However, contrary to the case of starts, there is no reduction in the spikes over time.

Chart 9A: SA Applicant Starts
(Individual him/herself started receiving SA)
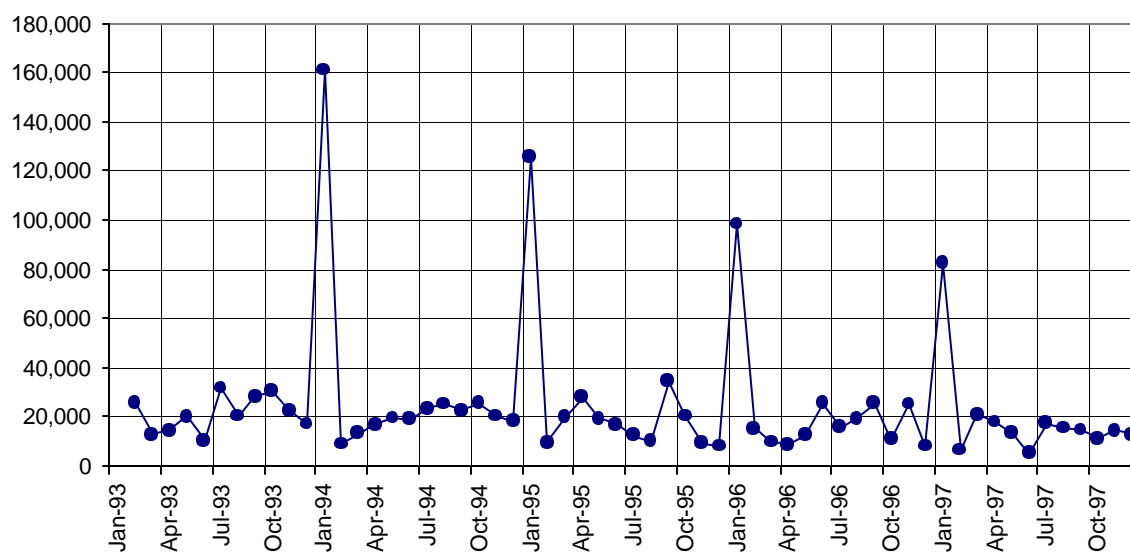
Chart 9B: SA Beneficiary Starts
(Someone within the family start receiving SA)
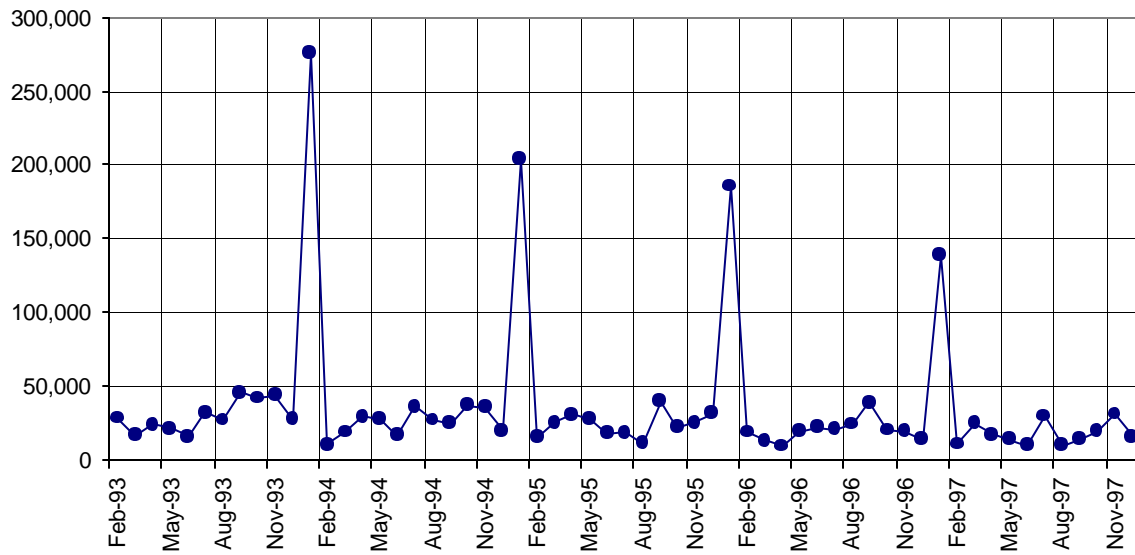
Chart 10A: SA Applicant Terminations
(Individual applicant stopped receiving SA)
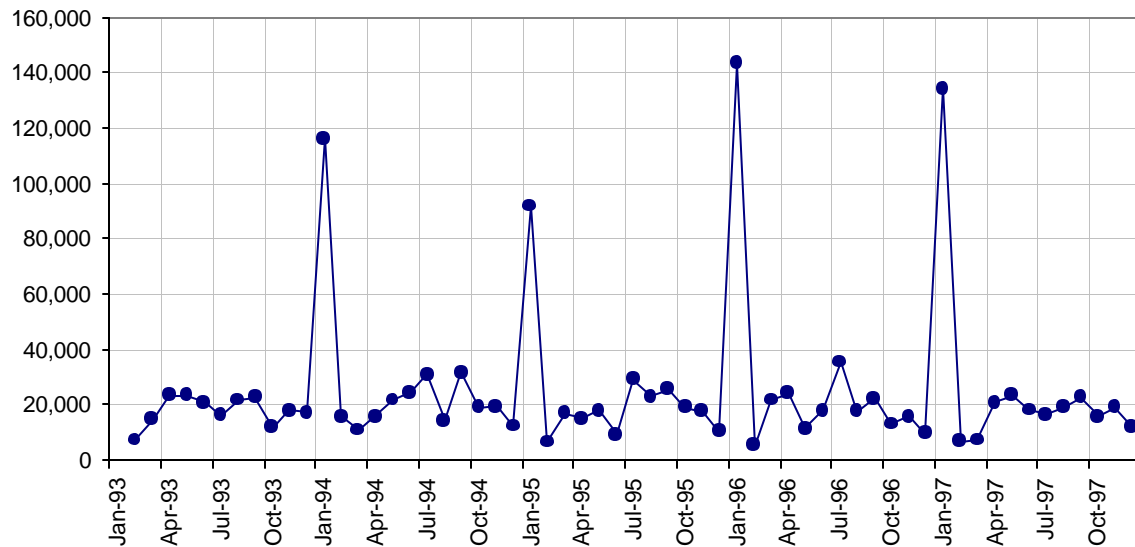
Chart 10B: SA Benefit Terminations
(Family Received Benefits)
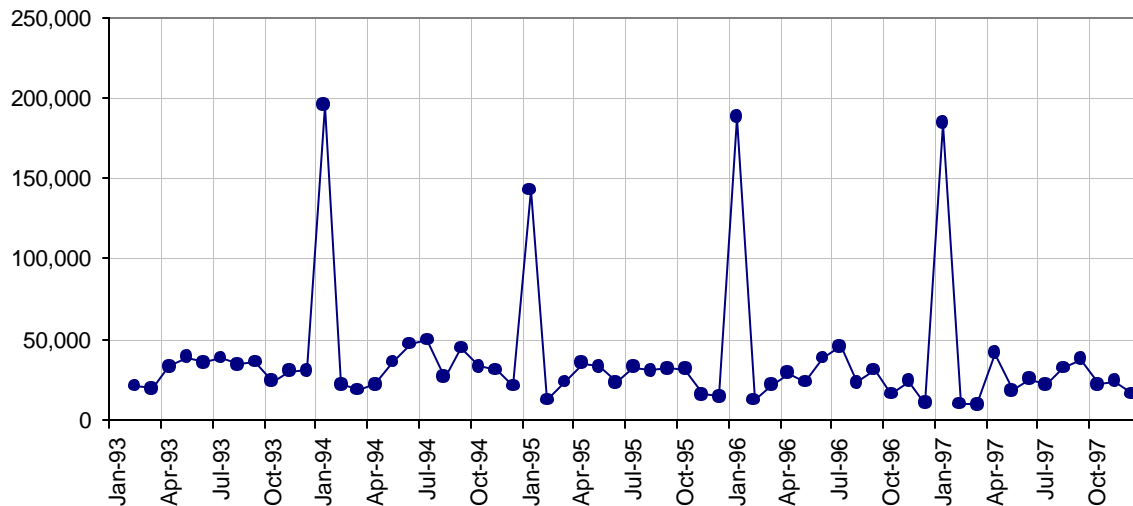
5.3 Nature of the SA Spikes

Are the above spikes proof of a seam effect with respect to starts and terminations of SA spells? It is impossible to be absolutely sure since we do not have administrative data of starts and terminations of SA spells.

In the case of EI, where administrative estimates of starts of new benefit spells were available, we found that the SLID spikes represent for the most part a real phenomenon, rather than a seam problem.

It is possible that this is also the case to some extent with SA spells. After all, SA starts and terminations are affected by labour market conditions, in the same way EI starts and terminations are affected.

However, a closer look at the raw data suggests that there is a problem with the way SA data are reported in SLID. For example (see *Chart 11*):
(a) In December 1996, there were 199 SA terminations—i.e. 199 individuals lived in families that received SA in December 1996, but not in January 1997 (117+15+56+11).
(b) In 117 of the above cases (59%), the individuals reported benefits in all the months in 1996, and no benefits in any month in 1997; these cases appear highly suspicious; one possibility is that many of these individuals received SA in most months in 1996, but not necessarily in December 1996.
(c) In 56 of the above cases (28%), the individuals reported benefits in some months in 1996, but no benefits in any month in 1997; these cases appear somewhat suspicious, although less suspicious than the 117 cases above; some of these individuals may have received benefits in January 1997; if there was no link to their tax file (or they did not report SA in their tax return) they would still appear as not having received SA in 1997.
(d) The remaining 26 cases (15+11) appear legitimate.

Based on the above analysis, 117 to 173 of the 199 terminations in SA in December 1996 (i.e. 59% to 87%) are highly or at least somewhat suspicious. We showed in Chart 10B that the rate of SA terminations in December 1996 was about 4 times as high as for the rest of the months. To attribute this spike entirely to incorrect reporting would require that 75% of the records be suspicious (which is within the range of 59% to 87% mentioned above).

Chart 11: Comparison of Monthly Receipt of SA: 1996 vs. 1997
(Unweighted sample of individuals living in families that reported SA in December 1996)

		No.	%
1996 received SA in all months	1997: received SA in all months	719	60.6%
	1997: SA in January, but not in all months	61	5.1%
	1997: did not receive SA in any month	117	9.9%
	1997: no SA in January; but SA in other months	15	1.3%
	1997: SA in the year; months not known	105	8.9%
1996 received SA in December but not in all months	1997: received SA in all months	48	4.0%
	1997: SA in January, but not in all months	39	3.3%
	1997: did not receive SA in any month	56	4.7%
	1997: no SA in January; but SA in other months	11	0.9%
	1997: SA in the year; months not known	15	1.3%
All individuals living in a family that received SA in December 1996		1,186	100.0%

5.4 Proxy Responses

More than half of the responses in SLID are obtained through proxy—i.e. the same member of the household provides information for all the members of the household. The individual that reports on behalf of the household is not necessarily the same person each year. If the individual reporting changes between two surveys and the two members of the household have a different understanding of what is SA, this would lead to a seam effect. For example, a household that was on social assistance for two years, may appear as exiting from SA at the end of the first year if the person reporting in the second year does not think the family is receiving SA. This problem is more likely to happen with SA than other programs since, as it was mentioned earlier, the definition of SA is not very clear.

5.5 Logit Regression Analysis

We also conducted logit regression analysis of the January starts and December terminations to see if the observed spikes are systematically correlated with personal characteristics.[10] The regression results are reported in the Appendix. They show that virtually none of the coefficients are significant at the 95% level. In other words, although the spikes are significant, they are not systematically correlated with any personal characteristics and, in that respect, are more random in nature.

5.6 Dependent Interviewing

a) Introduction

SLID uses dependent interviewing, a technique that consists in reminding respondents of information that they supplied in the previous interview, to assist them in recalling events that have occurred since.

A survey such as SLID can give rise to "seam problems," since respondents sometimes have trouble remembering the dates on which they started work, stopped work, experienced an interruption of work, etc. over a one-year period. These memory errors can result in an excessive proportion of periods beginning or ending on the "seam" of two consecutive reference periods.

Reminding the respondent of information collected in the previous interview can help reduce these seam problems. This technique is much easier to use in a Computer Assisted Interview (CAI) environment. CAI also serves to detect errors of logical consistency between the information collected in the labour interview and, a few months later, the information collected in the income interview.

b) Dependent Interviewing for EI, SA and Workers Compensation

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[10] The universe for the analysis of SA starts, for example, was individuals with a SA start in any month of the year; in this case, the dependent variable was one if individuals had a January start, and zero otherwise.

We concentrate here on EI, although the same points also apply to SA and Workers Compensation:

If in last year's survey (January 1997 interview) respondents reported that they received EI in December 1996, then in the January 1998 interview they are asked the following dependent interviewing questions:

- Based on our interview of a year ago, respondent received EI in December 1996. Is this correct?

If it is not correct, no retroactive change is made to the SLID information; basically the new information is ignored.

Regardless of what answer was given above, respondents are then asked:

- Did respondent receive any income from EI in 1997?
- (if answer is yes) in which months?

If in last years survey (January 1997 interview) respondents did NOT report that they received EI in December 1996, then no dependent interviewing takes place.

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c) Adjustments and Imputations

Again, the discussion about adjustments and imputations applies equally to EI, SA, and Workers Compensation.

SLID is linked to the tax return:
In the case that respondents have consented to the linking of their SLID record to their tax return, then:
- the amount of EI benefits in 1997 is obtained from their 1997 income tax return;
- if in the January 1998 interview they reported that they did not receive EI, then the 12 months are recoded from 2 (no) to 7 (received EI, but months not known).
- conversely, if in January 1998 they said that they received EI but no EI was reported in their tax return, the months are recoded from 1 (yes) to 2 (no EI benefits).

SLID is NOT linked to the tax return:
- the amount of EI is obtained during the May 1998 interview;
- if they did not report EI benefits in the May 1998 interview, but in the January 1998 interview they reported that they received EI in some months in 1997, then the EI amount for 1997 is imputed.
- conversely, if an amount is reported, but they said in the January 1998 interview that they did not receive EI in 1997, the month codes are changed from 2 (no) to 7 (received EI, but months not known).

---

d) Comments

With respect to the dependent interviewing questions, the above listed questions are not truly dependent interview questions. They are simply used to put the respondent in the right frame of mind, but are not being used to improve the quality of the data:

- in the case of those who reported receiving benefits in December of the previous year, if the information is contradicted in the next year no action is taken; and

- in the case of those who did <u>not</u> report receiving benefits in December of the previous year, no dependent interviewing questions are asked at all.

It will make sense to:

- apply dependent interviewing questions to <u>all respondents</u>—i.e. even if respondents reported in January 1997 that they did not receive any EI make sure they agree with the information they provided last year about benefits in December, regardless if they said they had received benefits in December or not;

- if there is a conflict in the dependent interviewing questions, an attempt should be made to <u>resolve the conflict</u>—both during the interview (through further probing) or through editing of the responses; this is particularly important if the conflict in questions can lead to a seam effect; for example, suppose respondents said last year that they received EI benefits in December 1996, but then next year they dispute this and also report no EI in January of 1997; in the absence of resolution of the conflicting answers, it would appear that an EI claim terminated in December 1996 (causing a seam effect).

With respect to the adjustment and imputation, given that there is an under-reporting of EI (as well as SA and WC), it will make sense to be more generous. In particular, if there is evidence from any source (January interview, May interview, or tax return) that respondents received EI, then it should be assumed that they received EI. Consequently, the amount of benefits and monthly pattern of benefits if missing should be imputed.

5.7 Conclusion

There are significant spikes in December terminations and January starts of SA spells in SLID. These spikes appear to reflect the presence of a significant seam effect. Among the possible solutions that should be considered are:

- In the shorter term, certain adjustments could be made to the data. For example, in the case where an EI spell appears to start in January, if the individual is still working in the same job in January, the start of the EI spell could be pushed after the end of the job spell.

- In the longer term, the dependent interviewing question could be strengthened, as suggested above. This suggestion would help reduce to size of the spikes in December termination and January starts.

- Given the magnitude of under-reporting of SA and the magnitude of seam effects, it will be desirable in the future to also consider the idea of extending the concept of data linking beyond income tax data, and include EI records and provincial SA records.

Appendix: Logit Regression Results

- We used two different samples: (a) benefit start in 1996; and (b) benefit termination in 1996.
- The dependent variable was the logit of January starts (1 if started in January; zero otherwise) or the logit of December terminations (1 if ended in December; zero otherwise).
- The independent variables are shown in Box A.
- The key statistic that is relevant to our analysis is the Sigma statistic. If sigma is less than 0.025, then the corresponding coefficient is significantly different than zero at the 95% level of confidence.
- The results show that virtually none of the coefficients are significant.

Box A: Explanation of Logit Independent Variables

AGE96 (0)   Age 16-24 (omitted)
AGE96(1)   Age 25-44
AGE96(2)   Age 45-64
AGE96(3)   Age 65+

DISABS96(1)   Had a disability in 1996

HHSZ96(0)   Household size 1 (omitted)
HHSZ96(1)   Household size 2
HHSZ96(2)   Household size 3
HHSZ96(3)   Household size 4+

HLEVEG96(0)   Education not known (omitted)
HLEVEG96(1)   Less than grade 9
HLEVEG96(2)   Grades 9-13
HLEVEG96(3)   High school graduation
HLEVEG96(4)   Some university
HLEVEG96(5)   Non-university diploma
HLEVEG96(6)   University degree

MARST96(0)   Married/common law (omitted)
MARST96(1)   Separated, divorced, widowed
MARST96(2)   Never married

PVRES96(0)   Atlantic (omitted)
PVRES96(1)   Quebec
PVRES96(2)   Ontario
PVRES96(3)   Prairie
PVRES96(4)   B.C.

SEX96(1)   Female

TTINC96(0)   Under 15,000
TTINC96(1)   15,000-24,999
TTINC96(2)   25,000-39,999
TTINC96(3)   40,000-59,999
TTINC96(4)   60,000-79,999
TTINC96(5)   80,000 plus

1. SA JANUARY STARTS: INDIVIDUAL RECEIVED BENEFITS

```
Total number of cases:    324 (Unweighted)
-2 Log Likelihood before  417.78547
-2 Log Likelihood after   379.701


---------------------- Variables in the Equation ------------------------


Variable          B      S.E.    Wald   df     Sig      R   Exp(B)


AGE96                            1.4820   3    .6864   .0000
 AGE96(1)      -.1619    .3356    .2328   1    .6295   .0000    .8505
 AGE96(2)       .2761    .4711    .3435   1    .5578   .0000   1.3180
 AGE96(3)      5.5663  13.5194    .1695   1    .6805   .0000 261.4675
DISABS96(1)    -.4249    .3191   1.7735   1    .1829   .0000    .6538
HHSZ96                           1.5075   3    .6805   .0000
 HHSZ96(1)     -.4276    .4048   1.1157   1    .2909   .0000    .6521
 HHSZ96(2)     -.0954    .4458    .0458   1    .8305   .0000    .9090
 HHSZ96(3)     -.1246    .4887    .0649   1    .7988   .0000    .8829
HLEVEG96                         9.8809   6    .1298   .0000
 HLEVEG96(1)   -.6086   1.4320    .1806   1    .6708   .0000    .5441
 HLEVEG96(2)  -1.4841   1.4062   1.1138   1    .2913   .0000    .2267
 HLEVEG96(3)  -1.4629   1.4364   1.0372   1    .3085   .0000    .2316
 HLEVEG96(4)  -1.2259   1.4209    .7443   1    .3883   .0000    .2935
 HLEVEG96(5)  -1.5735   1.4238   1.2213   1    .2691   .0000    .2073
 HLEVEG96(6)  -3.1072   1.7254   3.2432   1    .0717  -.0546    .0447
MARST96                          3.5751   2    .1674   .0000
 MARST96(1)    -.7047    .3766   3.5008   1    .0613  -.0599    .4943
 MARST96(2)    -.4675    .3905   1.4330   1    .2313   .0000    .6266
PVRES96                          5.8580   4    .2100   .0000
 PVRES96(1)    -.1062    .3744    .0805   1    .7767   .0000    .8992
 PVRES96(2)     .4439    .3692   1.4460   1    .2292   .0000   1.5588
 PVRES96(3)    -.4904    .4103   1.4289   1    .2319   .0000    .6124
 PVRES96(4)     .3205    .4747    .4559   1    .4995   .0000   1.3779
SEX96(1)        .3755    .2807   1.7895   1    .1810   .0000   1.4558
TTINC96                          5.0078   5    .4149   .0000
 TTINC96(1)     .1125    .3553    .1003   1    .7515   .0000   1.1191
 TTINC96(2)    -.6057    .4662   1.6884   1    .1938   .0000    .5457
 TTINC96(3)     .1480    .6081    .0592   1    .8077   .0000   1.1595
 TTINC96(4)   -1.2719    .9968   1.6281   1    .2020   .0000    .2803
 TTINC96(5)   -1.1993   1.1460   1.0952   1    .2953   .0000    .3014
Constant       1.4661   1.5104    .9422   1    .3317
```

2. SA JANUARY STARTS: ANY FAMILY MEMBER RECEIVED BENEFITS

```
Total number of cases:     499 (Unweighted)
-2 Log Likelihood before:  686.9415
-2 Log Likelihood after:   645.705

--------------------- Variables in the Equation -----------------------

Variable          B       S.E.     Wald    df     Sig       R    Exp(B)

AGE96                              3.3096    3    .3463    .0000
 AGE96(1)       .2702    .2640    1.0472    1    .3062    .0000   1.3102
 AGE96(2)       .5467    .3457    2.5012    1    .1138    .0270   1.7276
 AGE96(3)      -.8506   1.2835     .4392    1    .5075    .0000    .4272
DISABS96(1)    -.1702    .2468     .4757    1    .4904    .0000    .8435
HHSZ96                             .4428    3    .9313    .0000
 HHSZ96(1)     -.0712    .3581     .0395    1    .8424    .0000    .9313
 HHSZ96(2)      .0917    .3931     .0544    1    .8156    .0000   1.0960
 HHSZ96(3)     -.0606    .4163     .0212    1    .8843    .0000    .9412
HLEVEG96                          7.9068    6    .2450    .0000
 HLEVEG96(1)   -.8383    .9213     .8278    1    .3629    .0000    .4325
 HLEVEG96(2)  -1.0096    .8966    1.2679    1    .2602    .0000    .3643
 HLEVEG96(3)  -1.2579    .9142    1.8932    1    .1688    .0000    .2842
 HLEVEG96(4)  -1.4322    .9048    2.5054    1    .1135   -.0271    .2388
 HLEVEG96(5)  -1.2324    .8917    1.9104    1    .1669    .0000    .2916
 HLEVEG96(6)  -2.0237   1.0363    3.8136    1    .0508   -.0514    .1322
MARST96                           3.2390    2    .1980    .0000
 MARST96(1)    -.2972    .2693    1.2177    1    .2698    .0000    .7429
 MARST96(2)     .2384    .2863     .6932    1    .4051    .0000   1.2692
PVRES96                           9.2958    4    .0541    .0434
 PVRES96(1)    -.4066    .2772    2.1512    1    .1425   -.0148    .6659
 PVRES96(2)    -.3464    .2747    1.5899    1    .2073    .0000    .7072
 PVRES96(3)    -.8667    .3024    8.2147    1    .0042   -.0951    .4203
 PVRES96(4)    -.6998    .3709    3.5590    1    .0592   -.0476    .4967
SEX96(1)       -.0736    .2009     .1340    1    .7143    .0000    .9291
TTINC96                          13.1872    5    .0217    .0681
 TTINC96(1)     .5235    .2876    3.3124    1    .0688    .0437   1.6879
 TTINC96(2)     .2196    .3220     .4649    1    .4953    .0000   1.2455
 TTINC96(3)     .8710    .3993    4.7584    1    .0292    .0634   2.3892
 TTINC96(4)     .0510    .5010     .0104    1    .9189    .0000   1.0524
 TTINC96(5)    -.9309    .5725    2.6440    1    .1039   -.0306    .3942
Constant       1.0778   1.0165    1.1243    1    .2890
```

3. SA JANUARY TERMINATIONS: INDIVIDUAL RECEIVED BENEFITS

```
Total number of cases:     361 (Unweighted)
-2 Log Likelihood before:  478.28258
-2 Log Likelihood after:   428.261


--------------------- Variables in the Equation -----------------------

Variable            B        S.E.     Wald    df      Sig       R    Exp(B)

AGE96                                 9.2495   3     .0262   .0824
 AGE96(1)          .2058     .3243    .4027    1     .5257   .0000   1.2285
 AGE96(2)          .8457     .4185   4.0837    1     .0433   .0660   2.3295
 AGE96(3)        -1.8621    1.1213   2.7580    1     .0968  -.0398    .1553
DISABS96(1)       -.6185     .2946   4.4082    1     .0358  -.0710    .5387
HHSZ96                                3.6277   3     .3046   .0000
 HHSZ96(1)        -.2750     .4046    .4618    1     .4968   .0000    .7596
 HHSZ96(2)        -.1235     .4647    .0706    1     .7904   .0000    .8838
 HHSZ96(3)         .3513     .4510    .6069    1     .4360   .0000   1.4210
HLEVEG96                              8.4526   6     .2068   .0000
 HLEVEG96(1)       .0496    1.1489    .0019    1     .9656   .0000   1.0509
 HLEVEG96(2)      -.0607    1.1124    .0030    1     .9565   .0000    .9411
 HLEVEG96(3)      -.3133    1.1416    .0753    1     .7838   .0000    .7310
 HLEVEG96(4)      -.7169    1.1271    .4045    1     .5248   .0000    .4883
 HLEVEG96(5)      -.7583    1.1103    .4664    1     .4946   .0000    .4685
 HLEVEG96(6)     -1.2138    1.2902    .8851    1     .3468   .0000    .2971
MARST96                               3.9462   2     .1390   .0000
 MARST96(1)       -.6370     .3305   3.7145    1     .0539  -.0599    .5289
 MARST96(2)       -.4351     .3474   1.5679    1     .2105   .0000    .6472
PVRES96                               1.3385   4     .8548   .0000
 PVRES96(1)       -.0660     .3664    .0325    1     .8570   .0000    .9361
 PVRES96(2)       -.0382     .3505    .0119    1     .9132   .0000    .9625
 PVRES96(3)        .0221     .3951    .0031    1     .9554   .0000   1.0223
 PVRES96(4)       -.5178     .4986   1.0785    1     .2990   .0000    .5958
SEX96(1)           .4380     .2662   2.7080    1     .0998   .0385   1.5497
TTINC96                               5.1161   5     .4019   .0000
 TTINC96(1)       -.1719     .3356    .2624    1     .6085   .0000    .8420
 TTINC96(2)       -.4787     .3964   1.4583    1     .2272   .0000    .6196
 TTINC96(3)       -.8369     .5397   2.4044    1     .1210  -.0291    .4331
 TTINC96(4)       -.2245     .7193    .0974    1     .7549   .0000    .7989
 TTINC96(5)      -1.9688    1.1876   2.7483    1     .0974  -.0396    .1396
Constant           .4539    1.2667    .1284    1     .7201
```

4. SA JANUARY TERMINATIONS: ANY FAMILY MEMBER RECEIVED BENEFITS

```
 Total number of cases:     528 (Unweighted)
 -2 Log Likelihood before:  703.51768
 -2 Log Likelihood after:   669.977


--------------------- Variables in the Equation ------------------------
```

Variable	B	S.E.	Wald	df	Sig	R	Exp(B)
AGE96			6.7790	3	.0793	.0333	
AGE96(1)	.0582	.2744	.0450	1	.8321	.0000	1.0599
AGE96(2)	.6692	.3432	3.8025	1	.0512	.0506	1.9526
AGE96(3)	−.3399	.7517	.2045	1	.6511	.0000	.7118
DISABS96(1)	.2933	.2565	1.3077	1	.2528	.0000	1.3409
HHSZ96			11.1526	3	.0109	.0856	
HHSZ96(1)	−.8945	.3289	7.3976	1	.0065	−.0876	.4088
HHSZ96(2)	−.2883	.3617	.6353	1	.4254	.0000	.7495
HHSZ96(3)	−.8200	.3726	4.8422	1	.0278	−.0636	.4404
HLEVEG96			6.2545	6	.3953	.0000	
HLEVEG96(1)	−1.8305	1.2821	2.0384	1	.1534	−.0074	.1603
HLEVEG96(2)	−1.3950	1.2559	1.2338	1	.2667	.0000	.2478
HLEVEG96(3)	−1.2452	1.2671	.9658	1	.3257	.0000	.2879
HLEVEG96(4)	−1.8097	1.2632	2.0525	1	.1520	−.0086	.1637
HLEVEG96(5)	−1.4013	1.2565	1.2436	1	.2648	.0000	.2463
HLEVEG96(6)	−1.7158	1.4395	1.4206	1	.2333	.0000	.1798
MARST96			.7445	2	.6892	.0000	
MARST96(1)	−.2283	.2657	.7383	1	.3902	.0000	.7959
MARST96(2)	−.1238	.2919	.1799	1	.6715	.0000	.8835
PVRES96			5.0751	4	.2797	.0000	
PVRES96(1)	−.0602	.2870	.0440	1	.8338	.0000	.9416
PVRES96(2)	.4506	.2763	2.6599	1	.1029	.0306	1.5692
PVRES96(3)	−.0319	.3273	.0095	1	.9223	.0000	.9686
PVRES96(4)	.2811	.3602	.6088	1	.4352	.0000	1.3246
SEX96(1)	−.0051	.2013	.0006	1	.9798	.0000	.9949
TTINC96			1.5034	5	.9127	.0000	
TTINC96(1)	−.0640	.2847	.0505	1	.8222	.0000	.9380
TTINC96(2)	.1874	.3123	.3601	1	.5485	.0000	1.2061
TTINC96(3)	.2585	.3791	.4651	1	.4952	.0000	1.2950
TTINC96(4)	.1701	.5820	.0854	1	.7701	.0000	1.1854
TTINC96(5)	−.1181	.6048	.0381	1	.8452	.0000	.8886
Constant	1.0779	1.3418	.6453	1	.4218		

Related Statistics Canada publications

Questionnaires

General Aspects of the Survey of Labour Income Dynamics. Prepared by Mylène Lavigne (Social Survey Methods Division) and Sylvie Michaud (Social Survey Methods Division), March 1998 (Catalogue No. 98-05).

1998 Preliminary Interview Questionnaire. Prepared by Ruth Dibbs (Household Surveys Division), Debbie Lutz (Household Surveys Division), and Willo Wallace (Household Surveys Division), February 1998 (Catalogue No. 98-04).

SLID Labour Interview Questionnaire January 1998. Prepared by Ruth Dibbs (Household Surveys Division), Debbie Lutz (Household Surveys Division), and Willo Wallace (Household Surveys Division), February 1998 (Catalogue No. 98-03).

Questionnaire and Collection Procedures for SLID Income Data Collection - May 1998. Prepared by Judy Sauvé (Household Survey Division), Debbie Lutz (Household Survey Division), Anne Palmer (Household Surveys Division), and Willo Wallace (Household Surveys Division), April 1998 (Catalogue No. 98-06)

Analytical Reports

Measurement Issues in the Reporting of Unemployment Insurance. Prepared by Alison Hale (Household Surveys Division), Chantal Grondin (Social Survey Methods Division), Sylvie Michaud (Social Survey Methods Division), October 1994 (Catalogue No. 94-17).

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Impact of Edit and Imputation on Income Estimates: A Case Study. Prepared by Maryanne Webber (Income Statistics Division) and Cathy Cotton (Income Statistics Division), May 1998 (Catalogue No. 98-12).

The Seam Effect in the Survey of Labour and Income Dynamics. Prepared by Cathy Cotton (Income Statistics Division) and Phil Giles (Income Statistics Division), November 1998 (Catalogue No. 98-18).

Bridging Two Surveys: An Integrated Series of Income Data From SCF and SLID 1989-1997. Prepared by Cathy, Cotton, January 2000 (75F0002MIE - 00-02).