Welfare benefits and the duration of welfare spells: evidence from a natural experiment in Canada

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Abstract

This paper uses a major reform of the welfare program implemented in the Province of Quebec (Canada) in 1989 to analyze the impact of benefits on the length of welfare spells. An important feature of this reform was the abolishment of discrimination based on age that applied to benefits. As a result, the monthly benefits of single individuals aged less than 30 increased from $173 to $425 ($1986), an increase of over 145\%. To assess the impact of the reform, we estimate a series of semi-parametric hazard models analogous to the regression approach to difference-in-differences estimation. The estimated duration elasticities with respect to benefits for single men and women aged 22–29 are 0.25 and 0.28, respectively.

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1. Introduction

One of the most crucial issues concerning welfare reform is whether benefits induce ‘dependency’ in terms of frequency and duration of participation. Such dependency is likely to increase the costs of welfare programs. In a dynamic context, it may lead to a depreciation of human capital and to increasing difficulties in getting and holding jobs. For some claimants, welfare dependency may therefore have a perverse effect on the very poverty the program seeks to alleviate.

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In his paper, Moffitt (1992) surveyed a growing body of literature that links welfare benefits and welfare dependency in the US. While this literature has focused primarily on static welfare participation—that is, participation at a point in time—more recent research efforts have focused on dynamic welfare participation—that is, entry and exit into welfare. The latter studies have mostly been concerned with estimating the determinants of the exit rates (hazard rates) from AFDC rolls. Although they have generally found that the AFDC benefits levels have a negative impact on exit rates, the evidence is rather weak (e.g. O’Neil et al., 1987; Blank, 1989; Ruggles, 1989; Fitzgerald, 1991; Giannarelli, 1992; Hoynes and MaCurdy, 1994).

As suggested by Fitzgerald (1989), small (and often non-significant) estimated effects can partly be explained by data deficiencies. In most studies, the identification of the benefits effect rests essentially on cross-state variability in benefits levels and/or on within-state time variability in real benefits. One problem with cross-state variability is that differences in state benefits levels (and implicit tax rates) may be partly offset by differences in other program characteristics such as the severity in the administrative treatment of claims, the level of earnings exemption or the assets test. They may also reflect differences in state cost of living, in non-AFDC benefits or in other regional variables that are difficult to control for. Furthermore, average real monthly benefits across states have shown fairly smooth changes over time, with moderate increases between 1968 and 1974, gradual declines between 1974 and 1982 and flattening after 1982 (see Hoynes and MaCurdy, 1994). Therefore, studies that rely on a relatively short time frame may lack sufficient within state variability in the benefits data, which may account for the small and imprecise estimated effects. Even when the time frame is sufficiently long, changes in real benefits are likely to be correlated with other covariates that affect spell duration but that are hard to adequately measure.

In Canada, only four papers have analyzed the exit rates from social assistance (Bailey, 1994; Barrett and Cragg, 1998; Barrett, 2000 and Dooley and Stewart, 1999). The first two papers are based on data from the British Columbia Income Assistance Program. Neither of these study the impact of the basic real assistance level on social assistance spell duration. The papers by Barrett (2000) and Dooley and Stewart (1999) introduce covariates in a proportional hazard model of welfare duration using administrative data for New-Brunswick and Ontario, respectively. However, they do not exploit the presence of a ‘natural experiment’ relating to an important reform in a social assistance program.

In this paper, we analyze the impact of benefits on the length of welfare spells using information from a natural experiment that took place in the Province of Quebec in August 1989. A major reform was implemented which provided for the abolishment of discrimination based on age that applied to the benefits single individuals and childless couples below the age of 30 were entitled to. Thus, prior to the reform, the basic benefits able-bodied singles below the age of 30 (and childless couples) were entitled to amounted approximately to 40% of those of individuals over 30 years of age. With the reform,

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1 For instance, Ruggles (1989), Fitzgerald (1991) and Giannarelli (1992) use the first panel of SIPP, which contains 32 months of data.

2 O’Neil et al. (1987) use the NLS Young Women’s panel over an 11-year period.

3 The reform also introduced a parental contribution test. This feature will be discussed in the next section.
monthly benefits were equalized and thus rose from $173 to $425 (1986 $) for those below 30 years of age, an increase of over 145%.\textsuperscript{4}

Fig. 1 depicts the schedule of monthly benefits singles of both age groups were entitled to between January 1975 and December 1993. The step-wise shape arises because the benefits are generally adjusted against inflation once a year whereas we use the monthly CPI (1986 = 100) to deflate them. The schedule shows that the real benefits of both age groups have remained relatively constant throughout the whole period, except for the dramatic jump observed for the younger group in August 1989. At that time, the monthly benefits are equalized at $453. The real benefits eventually decline to about $425 and remain stable (on a yearly basis) thereafter.

Our general estimation strategy consists in exploiting the time of entry into welfare to identify the impact of benefits on duration, and the fact that only individuals below 30 years of age were subjected to such a dramatic change. As such, they constitute our basic ‘treatment group’, whereas individuals over 30 years of age constitute our ‘control group’.

In the statistical analysis, we first compare the pre- and post-reform Kaplan–Meier survival estimates as well as smoothed hazard functions of leaving welfare for men and women, and for each age group. We next exploit the ‘natural experiment’ dimension of the reform by estimating a series of proportional hazard models that are equivalent to the familiar difference-in-differences (henceforth DD) estimators that are widely used in the linear regression approach (e.g. Meyer et al., 1995). This allows the removal of any bias.

\textsuperscript{4} In fact, in the case of single individuals, the 1989 reform introduced four basic benefits schedules depending on a claimant’s situation vis-à-vis employability programs: participant, non-participant (i.e. who refuses to participate), available or not-available (due to a temporary illness, etc). Since the benefits levels vary little across these categories, and since only 10% of claimants on average participate in these programs, we did not directly take these differences into account in our analysis.
due to a post-reform shift in exit rates due to changes in factors that are common to the
treatment and control groups. Introducing additional covariates in a stepwise fashion
provides a series of DD estimates that are corrected for observable factors other than the
reform that may affect the difference in the average spell duration of the control and
treatment groups over time. In each step of our analysis, DD estimators are contrasted to
those obtained from a traditional Before–After (henceforth BA) estimator.

To our knowledge, this paper provides the first attempt to estimate the impact of
benefits levels on welfare duration of single individuals. Indeed, most papers in the
literature focus on US programs which are limited exclusively to families. The empirical
analysis uses over 11 years of monthly data from the case records of the Quebec Social
Assistance administrative files. We proceed by estimating a flexible specification for the
duration distribution based on the well known Prentice–Gloeckler–Meyer semi-paramet-
ric proportional hazard model with time-varying covariates. The paper is organized as
follows. Section 2 discusses the main features of the 1989 welfare reform and provides a
description of the data used in the empirical analysis. Section 3 shows how the semi-
parametric proportional hazard model can be made equivalent to a DD estimator and
discusses the empirical variables used in the model. Main findings are reported in Section
4. Finally, we conclude the paper in Section 5.

2. Data and descriptive statistics

The data used in the analysis are drawn from the administrative files of the Ministère de
la Solidarité sociale which is responsible for the administration of the program in the
Province of Quebec. Because the program is highly centralized, the data covers every
region of the province and is usually considered quite reliable. The data at our disposal
span many years prior to, and many years following, the 1989 reform. Prior to discussing
the data, it is best to describe the main features of the program and the context in which the
reform was implemented.

2.1. Institutional features

The Quebec welfare program provides households a means tested monthly benefit. The
implicit tax rate on earned income is set at 100% (above a small earnings disregard). To be
eligible for benefits, individuals must satisfy an assets test. Net assets above a certain basic
level are converted into monthly income flows which are subtracted from benefits. All
those in our sample necessarily satisfy the assets test.

As stated earlier, the main trust behind the implementation of the 1989 reform was the
elimination of discrimination based on age that applied to the benefits single individuals
and childless couples below the age of 30 were entitled to. Such discrimination was

5 Fortin et al. (2002a) provide a simple analysis of the 1989 reform in Quebec by comparing the exit rates
from welfare for a sample of singles under 30 years of age and a sample of singles aged 30 years and over.
However, they do not use a ‘natural experiment’ approach to assess the impact of the reform.
deemed inconsistent with the Quebec Charter of Rights and Liberties which precludes
discrimination based on age. It is usually acknowledged that changes in the relative labour
market opportunities of younger individuals before 1989 had little to do with the
implementation of the reform. This adds to the ‘exogenous’ nature of the change in
benefits.

The 1989 welfare reform had two major components. First, the maximum monthly
benefits for single able-bodied individuals under 30 years of age were increased by about
145%. Second, it introduced a ‘parental contribution’ test. Under this latter provision, a
financial contribution is required from parents whose resources are deemed adequate to
support their children. The contribution is based on the parents’ net income and is
subtracted from the child’s benefits. In some cases, the parental contribution test may
render an adult child ineligible for assistance. However, there are many instances when
the parental contribution test does not apply. The parental contribution test is applicable
for a maximum of 3 years. The features of the test are such that it applies mostly to very
young adults. As of November 1998, 21.8% of single welfare recipients aged 20 or below
were subjected to the parental contribution. Only 4.6% of those in the 21–24 group, and as
little as 0.5% of those in the 25–29 cohort, were affected by the test (see Fortin and
Santarossa, 2000).

Unfortunately, the information concerning the application of the parental contribution
test is not available at the individual level in our sample. It is thus not possible to explicitly
introduce this provision into our statistical model. However, the features of the test and the
evidence provided below strongly suggest breaking the treatment group into a number of
age sub-groups when analyzing the impact of the 1989 reform.

2.2. The sample

The sample we use is drawn from the monthly case records of the social assistance
program in the Province of Quebec. The administrative files contain detailed information
on individual characteristics. It is thus possible to identify those who are permanently
handicapped or seriously ill. These individuals are excluded from the master files in order
to avoid sampling individuals who are very unlikely to exit welfare. The drawing was
performed on the basis of the social insurance number and was thus not related to the
frequency or the length of individual spells.

A welfare spell is defined as a sequence of consecutive months of welfare receipt. An
exit occurs when an individual is not in receipt of welfare benefits for one month. Right
censoring occurs if a spell was ongoing in December 1993. Since single individuals are the
focus of our analysis, spells during which the ‘single’ status changed were removed from

6 The monthly parental contribution amounts to 40% of the difference between the parents’ annual income
and various regulatory exemptions, divided by 12.

7 The test does not apply when one of the following conditions is met: the child has been self-sufficient and
has lived outside his or her parents’ residence for a period of at least 2 years; the child has held a full-time job for
at least 2 years or has received unemployment benefits; the child is, or has been, married; the child has a common
law spouse or has cohabited at some time with that person for a period of at least 1 year; the child has dependent
children; the child holds a bachelor’s degree; the child is pregnant; the child’s parents claim welfare benefits.
As well, age (in months) at the start of the spell determines the age group to which the individual belongs.

### 2.3. Descriptive statistics

Table 1 presents descriptive statistics for the period 1983–1993. Spells that were ongoing in January 1983 have been removed from the sample. The choice of this period is dictated by empirical considerations. Indeed, the period should be short enough to minimize the impact of potential cohort effects. On the other hand, the period should be long enough so that we may obtain precise estimates of the impact of the reform. The sample of women includes 20,381 observations while the sample of men, which is much

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**A. Variables that are constant over spell:**

- Years of education
  - 10.607 (2.115) 11.331 (2.256) 11.551 (3.244) 11.214 (3.456) 10.004 (2.035) 10.552 (2.574) 10.868 (2.909) 10.650 (3.377)
- Born in Canada (%)
  - 94.983 (2.115) 92.409 (2.256) 89.165 (3.244) 88.101 (3.456) 94.631 (2.035) 90.740 (2.574) 87.188 (2.909) 84.716 (3.377)
- City of Montreal (%)

**B. Variables which vary over spell:**

- Age at start of spell
  - 19.777 (0.970) 22.880 (1.080) 26.758 (1.406) 35.912 (4.251) 19.804 (0.980) 22.968 (0.812) 26.857 (1.409) 35.576 (4.123)
- Monthly welfare benefits
  - 263.959 (124.800) 446.957 (15.430) 263.959 (124.800) 446.957 (15.430)
- Unemployment rate
  - 10.574 (1.858) 1.578 (1.858)
- Minimum wage
  - 4.292 (0.135) 4.292 (0.135)

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8 We also treated these spells as censored and included them in the analysis. The econometric results were robust to their inclusion.

9 This treatment may be the source of a left-censoring bias. However, in analyses not reported here we also included spells that were ongoing in January 1983 by following them back as early as February 1975. Our econometric results were quite robust to the inclusion of these spells.

10 In the econometric section we investigate the robustness of our results with respect to the choice of the sample period.
larger, includes 43,326 observations. The 18–24 age group accounts for more than 45% of all spells.

Panel A reports descriptive statistics on variables that are constant throughout the spells. The individuals in our sample are generally poorly educated. The average schooling is somewhat less than high-school (12 years in Quebec). This is true of all age groups. Women are on average slightly better educated than men, which is also true of the total population in general. The panel also indicates the proportion of individuals living in the City of Montreal (excluding its suburbs). The city represents approximately 15% of the total provincial population. Welfare claimants are under-represented in Montreal. This probably reflects the fact that Montreal has a higher per capita income than elsewhere in the province of Quebec.

The main characteristics of our sample have remained relatively constant until 1989. However, the average level of education of male recipients under 30 years of age decreased by nearly 1 year in the post-1989 period while that of women remained remarkably constant. Also, the proportion of individuals born outside Canada rose from 1% to nearly 10% during the same period.

The last panel concerns variables which vary over the spells. The monthly welfare benefits are computed from program parameters. The large standard error for individuals under 30 years of age is primarily due to the reform of 1989. Note that, in real terms, maximum benefits of individuals over 30 years of age vary little. The monthly

![Fig. 2. (a) Smoothed monthly hasard rates, Women 18–21 (b) Kaplan–Meier monthly survival rates, Women 18–21.](image-url)
unemployment rate of adult men (25–64 years of age) best approximates the labour market tightness of strongly attached individuals. As shown in the table, the unemployment rate was considerably high during the period of analysis. Finally, much of the variation in the real minimum wage is due to the fact that it remained constant in nominal terms for nearly 5 years at a time when inflation varied considerably.

2.4. The impact of the 1989 reform: preliminary evidence

*Prima facie* evidence on the impact of the 1989 reform is provided in Figs. 2–9. For each age sub-group, and for both males and females, we plot kernel smoothed monthly hazard rates and Kaplan–Meier monthly survival rates separately.\textsuperscript{11} To reduce potential biases that may arise due to cohort effects or to changes in exogenous variables, only spells that occurred between 1983 and 1993 were used in drawing these graphs. In each graph we plot pre-1989 and post-1989 curves.\textsuperscript{12}

Figs. 2a and 3a clearly show that the exit rates of women in the 18–21 and 22–24 age groups declined significantly after the 1989 reform. Those in the 25–29 and 30+ groups (Figs. 4a and 5a) seem to have the same exit behaviour both before and after August 1989.

\textsuperscript{11} We use the Epanechnikov Kernel with tail correction. See Klein and Moeschberger (1997) for details.

\textsuperscript{12} Spells that started prior to August 1989 and that were still ongoing at that time were truncated and considered censored.
Figs. 6a–8a, on the other hand, clearly show that men in the treatment groups witnessed a significant decline in their exit rates following the implantation of the welfare reform. The exit rates of the male control group (Fig. 9a) show a much weaker decline, if any.

The survivor functions give a complete accounting of the survival experience of each age sub-groups. These are plotted along with the mean duration of welfare spells before and after the reform. We also report Log-rank and Wilcoxon statistics that test the null assumption that the survivor functions are the same. Both are chi-square statistics with a single degree of freedom. They differ only insofar as the Wilcoxon statistics weights early times more heavily than later times.

The survivor functions in Figs. 2b and 3b shift upwards following the reform. Consequently, the mean spell duration of women between 18 and 21 years of age increases from 9.45 months to 18.43 months, whereas the mean duration of women in the 22–24 age group increases from 14.25 to 19.75. The test statistics reject the null assumption that the survivor functions are identical in the pre-1989 and post-1989 periods in both cases. The survivor functions in Figs. 4b and 5b are remarkably stable. The test statistics can not reject the null assumption in either case. Consequently, the means spell duration of women in the 25–29 age group and those in the control group has not increased despite the fact that the former were affected by the reform while the latter were not.

Fig. 4. (a) Smoothed monthly hasard rates, Women 25–29 (b) Kaplan–Meier monthly survival rates, Women 25–29.

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13 The means are computed on the basis of the Kaplan–Meier survival rates.
Fig. 5. (a) Smoothed monthly hasard rates, Women 30+ (b) Kaplan–Meier monthly survival rates, Women 30+.

Fig. 6. (a) Smoothed monthly hasard rates, Men 18–21 (b) Kaplan–Meier monthly survival rates, Men 18–21.
Fig. 7. (a) Smoothed monthly hazard rates, Men 22–24 (b) Kaplan–Meier monthly survival rates, Men 22–24.

Fig. 8. (a) Smoothed monthly hazard rates, Men 25–29 (b) Kaplan–Meier monthly survival rates, Women 25–29.
Figs. 6b–9b plot the survivor functions for men. In each case, the survivor function shifts upwards in the post-1989 period. The test statistics reject the null assumption in all four figures. However, a glance at the figures shows that the shifts are much more important for the three treatment groups than for the control group. The mean spell duration of men between 18 and 21 years increases by about 6.7 months, that of men in the 22–24 and 25–29 groups by about 5 months, whereas that of the control group (30+) increases by less than 2 months.

These preliminary results are consistent with the hypothesis that the reform had a strong positive impact on the length of welfare spells of both men and women below 30 (except in the case of women in the 25–29 age group). However, one must be cautious to draw any firm conclusion from this analysis since the large shifts in the survivor functions of the treatment groups after 1989 cannot be attributed to the reform alone. The deteriorating labour market that accompanied the economic downturn of 1990, which was observed not only in Quebec but also in the rest of Canada, hurt younger individuals more severely. Demographic factors and technical changes may also impact long run labour market opportunities of men and women of various age groups differently, and thus their relative exit rates.

While the empirical evidence suggests that the 1989 reform may have played an important role in increasing the average welfare spell duration, the contribution of other variables must be factored out to ascertain its real impact. In order to do this, we will rely on a semi-parametric proportional hazard model (see Meyer, 1990) that is cast within a difference-in-differences framework.
3. The statistical model

The population of single welfare claimants is divided into two groups with two potentially different hazard functions: Group C (control group) includes claimants aged 30 and over and Group T (treatment group) includes those that are under 30. We will temporarily assume there is a single treatment group to simplify the notation. The (log) hazard of leaving welfare at \( t \) for a claimant \( i \), conditional upon participating up to time \( t \), is assumed to take the following form:

\[
\lambda_i^T(t) = \lambda_0^T(t) + (\alpha_P + \alpha_R)D_R + z_i(t)\alpha + \epsilon_i^T
\]

if he or she belongs to group \( T \), and

\[
\lambda_i^C(t) = \lambda_0^C(t) + (\beta_P + \beta_R)D_R + z_i(t)\beta + \epsilon_i^C
\]

if he or she belongs to group \( C \).

In Eqs. (1) and (2), \( D_R \) is a dummy variable equal to zero in the pre-reform period and to one in the post-reform period. The parameters \( \alpha_R \) and \( \beta_R \) measure the impact (in percentage) of the reform on the hazard rate of the treatment and control groups, respectively. The parameters \( \alpha_P \) and \( \beta_P \) allow for additional time-specific effects that may shift the hazard rates permanently after the reform. The row-vector \( z_i(t) \) includes explanatory variables (both at the individual and aggregate levels).\(^{14}\) These covariates are allowed to have different effects on the exit rates of each group. The variable \( \lambda_0^J(t) \) is the baseline (log) hazard for group \( J \) (with \( J = T, C \)). It represents the common exit rate of all individuals from group \( J \) at period \( t \) in the pre-reform period (i.e. when \( D_R \) is zero) and for \( z_i(t) = 0 \). For simplicity, it is assumed that the reform does not distort the baseline hazard. The \( \epsilon_i^J \)'s are i.i.d. random variables with zero mean reflecting individual unobserved permanent heterogeneity. The \( \alpha \)'s and the \( \beta \)'s are parameters to be estimated.

Without imposing any restrictions the parameters of interest \( \alpha_R \) and \( \beta_R \) are not identified separately, only \( (\alpha_P + \alpha_R) \) and \( (\beta_P + \beta_R) \) are. The two estimators we use in the empirical analysis are particular cases of this general framework that are obtained by imposing specific identifying restrictions. First, the analog of a traditional BA estimator is obtained by assuming that there are no time-specific effects that have a permanent impact on the exit rates of the treatment group in the post-reform period \( (\alpha_P = 0) \), that the control group is unaffected (directly or indirectly) by the reform \( (\beta_R = 0) \), and that the covariates are orthogonal to the random terms. A basic drawback of the BA estimator is that it is biased in the presence of unobservable variables that permanently affect the intercept of the treatment group’s hazard after the reform.

An alternative approach uses the analog of a DD estimator.\(^{15}\) It addresses one shortcoming of the BA estimator by allowing for a post-reform shift in the hazard rates that is common to both treatment and control groups, while still imposing that the reform

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\(^{14}\) For notational convenience, we ignore the distinction between calendar time and time on welfare.

\(^{15}\) See Heckman et al. (1999), pp. 1894–1896, for a discussion of the properties of the DD estimator.
has no effect on the control group. One simple way to compare this DD estimator with the BA estimator is to combine Eqs. (1) and (2) into one equation, while imposing $\beta_R = 0$:

$$
\lambda_i(t) = \lambda^C_0(t) + D_{IT} \Delta^T_0 + \beta_P D_R + (z_P + \alpha_R - \beta_P) D_{IT} D_R + z_i(t) \beta \\
+ D_{IT} z_i(t) \tilde{\alpha} + \eta_i,
$$

where $D_{IT}$ is a dummy variable equal to one when the claimant belongs to the treatment group, $\Delta^T_0(t) = \lambda^{T}_0(t) - \lambda^{C}_0(t)$, $\tilde{\alpha} = \alpha - \beta$ and $\eta_i = e^C_i + D_{IT} (e^T_\iota - e^C_\iota)$. In Eq. (3), the DD estimator is obtained by further imposing $z_P = \beta_P$. Therefore, the impact of the reform on the hazard of the treatment group, which is given by $\alpha_R$, is given by the parameter associated with the interaction term between the post-reform and the treatment group dummy variables, $D_{IT} D_R$. This parameter can be interpreted as a shift in the post-reform hazard rates of the treatment group beyond any post-reform shift (given by $\beta_P$) that is common to both the control and treatment groups (and controlling for the explanatory variables included in $z_i(t)$). On the other hand, the BA estimator is obtained by imposing $z_P = 0$ in Eq. (3), rather than $z_P = \beta_P$. Therefore, it is given by the sum of the coefficient of $D_R$ and that of $D_{IT} D_R$ (since $\beta_P + (\alpha_R - \beta_P) = \alpha_R$). In the next section, we will present BA and DD estimators for various specifications of Eq. (3).

Based on standard economic theory, one should expect the exits from welfare to decline with an increase in benefits. Therefore, one the parameter $\alpha_R$ in Eq. (3) should be negative (probably closer to zero or even positive for the 18–21-year-old group, due to the effect of the parental contribution). However, to obtain a consistent estimate of this parameter, it is important to take into account covariates included in the vector $z_i(t)$ and especially those that are correlated with the reform.

As mentioned earlier, demographic factors and technical changes may also impact the long run labour market opportunities of men and women of various age groups differently. A simple way to take these cohort effects into account is to add yearly dummy variables in interaction with age groups in the model. However, one problem with this approach is that it makes the impact of the reform poorly identified since only two quarters (summer and fall quarters of 1989) can be used to identify the parameter of the post 89 × 18–29 variable. Nevertheless, in some specifications, we do present results using these interactive yearly dummies as a way of eliminating as much as possible the presence of unobserved cohort effects that could potentially be correlated with the impact of the reform.

Many important sectors of activity in Quebec (e.g. construction, tourism, agriculture, fisheries) are characterized by seasonal cycles that may exert a significant impact on the exit rates from welfare. This can accounted for by including a series of seasonal dummy variables. Likewise, the economic downturn of 1991 has hurt younger individuals more severely. This can be partly accounted for by including a variable that captures the cyclical activity in interaction with age group. A natural candidate is the adult male unemployment rate. Also, to take into account some policy variables, we have included the real minimum wage as an additional covariate in the model.16

16 In some specifications, we have introduced parameters of the unemployment insurance program. However, these variables were never significant when yearly dummy variables were introduced into the model. We therefore decided to ignore these variables in the empirical section.
Finally, other covariates reflecting observable individual characteristics may be important in explaining the exit rates. It is usually thought that the behavioural response to social assistance parameters varies with gender. Therefore, Eq. (3) will be estimated separately for single men and women. Moreover, the impact of the reform (and of other covariates) is likely to vary with age even within the treatment group. In particular, as discussed earlier, the parental contribution test introduced with the reform is likely to have a stronger (positive) effect on the exit rates of the youngest age sub-groups. Therefore, in a number of specifications, some parameters of the model will be allowed to vary across various age sub-groups. Finally, variables such as schooling, region of residence (which may reflect local labour markets) and the status of immigrant may affect the duration of spells on welfare and will be introduced in some specifications of the model.

In estimating Eq. (3) we use a non parametric baseline hazard. Indeed, the smoothed hazard rates in Figs. 2a–9a are non-monotone and probably not well approximated by most common parametric specifications. Also, there are some theoretical arguments and empirical evidence indicating that estimated coefficients for covariates are robust to misspecification of unobserved heterogeneity if the baseline hazard is reasonably flexible (Ridder, 1987; Portugal and Addison, 2000). As in many studies, we have incorporated unobserved heterogeneity by assuming that $\epsilon_i^J$ (and hence $\eta_i$) was distributed as a gamma variable with mean normalized to one and variance equal to $\sigma^2$. However this specification seldom converged and when it did, the parameter estimates of the covariates were almost identical to those obtained from specifications that omitted unobserved heterogeneity. These results are in line with Barrett (2000) who also found that the coefficient estimates for covariates are little affected by the form of heterogeneity distribution. Consequently, we will only report results based on specifications that omit unobserved heterogeneity.\footnote{We also experimented with the hazard model developed by Ham and Rea (1987) that is based on a logistic function with heterogeneity modeled using a non-parametric discrete distribution [see Heckman and Singer (1984)]. The results were very close to those presented in this paper.}

4. Results

In discussing the results, we adopt the following strategy. We first present the DD parameter estimates as outlined in Eq. (3). We start with a single treatment group and no covariates. Following the discussion of Section 2.4, we next allow for three different treatment groups, but leave out covariates. We then gradually incorporate sets of covariates to assess the robustness of the reform effect. In the most complete specification, many key covariates are interacted with age groups. We also assess the robustness of the results to the choice of the sample period. For each specification, we also provide results based on the traditional BA estimation approach. Finally, given the change in mean benefits between the pre- and post-1989 periods, duration elasticities are computed for the most simple specification (no covariates) and for the most complete specification with many covariates interacted with age group dummies.

In order to implement our econometric strategy, ongoing lengthy spells must be censored. After some experimentations, we have censored all observations lasting more
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<td>–0.114 (0.018)</td>
<td>–0.114 (0.018)</td>
<td>–0.059 (0.020)</td>
<td>–0.069 (0.020)</td>
<td>–0.108 (0.038)</td>
<td>–0.075 (0.039)</td>
<td>–0.106 (0.043)</td>
<td>–0.084 (0.041)</td>
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<tr>
<td>18–29</td>
<td>0.454 (0.016)</td>
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<tr>
<td>18–21</td>
<td></td>
<td>0.606 (0.023)</td>
<td>0.088 (0.068)</td>
<td>–0.835 (0.615)</td>
<td>–0.819 (0.617)</td>
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<td>22–24</td>
<td></td>
<td>0.421 (0.018)</td>
<td>1.324 (0.520)</td>
<td>0.243 (0.525)</td>
<td>0.297 (0.529)</td>
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<tr>
<td>25–29</td>
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<td>0.385 (0.019)</td>
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<td>–0.316 (0.567)</td>
<td>–0.287 (0.568)</td>
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<td></td>
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<tr>
<td>Post 89 × 18–21</td>
<td></td>
<td>–0.417 (0.040)</td>
<td>–0.233 (0.044)</td>
<td>–0.141 (0.045)</td>
<td>–0.159 (0.046)</td>
<td>–0.176 (0.036)</td>
<td>–0.013 (0.043)</td>
<td>–0.091 (0.039)</td>
<td>–0.074 (0.072)</td>
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<tr>
<td>Post 89 × 22–24</td>
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<td>–0.309 (0.029)</td>
<td>–0.221 (0.032)</td>
<td>–0.142 (0.032)</td>
<td>–0.154 (0.033)</td>
<td>–0.186 (0.037)</td>
<td>–0.253 (0.070)</td>
<td>–0.253 (0.064)</td>
<td>–0.475 (0.107)</td>
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<tr>
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<td></td>
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<td>–0.207 (0.033)</td>
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<tr>
<td>Schooling, age, migrant + region dum</td>
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<td>Baseline × age Gr.</td>
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<tr>
<td>Year dum × age Gr.</td>
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<td>Log – likelihood</td>
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Notes: (a) The sample is drawn from the monthly administrative files of the social assistance program in the Province of Quebec (Canada). It is limited to single able-bodied recipients. (b) Standard errors in parentheses. (c) All specifications use a proportional semi-parametric hazard model. In specifications (3)–(9), seasonal dummies (3), Adult male unemployment rate, and Real minimum wage are introduced in interaction with age groups. In specifications (4)–(9), dummy variables for being born in Canada and for the region of residence (12) are introduced. These are: Gaspésie, Bas St-Laurent, Saguenay, Québec, Mauricie, Estrie, Montérégie, Laval, Laurentide, Outaouais, Abitibi and Côte-Nord. City of Montreal is omitted. (d) The baseline hazard comprises over 21 parameters. Specifications (6)–(9) interact the baseline hazard with each age group.
<table>
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<td>–0.004</td>
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<td>–0.425</td>
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<td>18–21</td>
<td>(-0.626)</td>
<td>(-0.517)</td>
<td>(-0.402)</td>
<td>(-0.417)</td>
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<td>(-0.198)</td>
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<td>YES</td>
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<td>Year dum×age Gr.</td>
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<td>NO</td>
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<td>(-52 323)</td>
<td>(-52 119)</td>
<td>(-72 586)</td>
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</table>

Notes: (a) The sample is drawn from the monthly administrative files of the social assistance program in the Province of Quebec (Canada). It is limited to single able-bodied recipients. (b) Standard errors in parentheses. (c) All specifications use a proportional semi-parametric hazard model. In specifications (3)–(9), seasonal dummies (3), Adult male unemployment rate, and Real minimum wage are introduced in interaction with age groups. In specifications (4)–(9), dummy variables for being born in Canada and for the region of residence (12) are introduced. These are: Gaspé, Bas St-Laurent, Saguenay, Québec, Mauricie, Estrie, Montérégie, Laval, Laurentide, Outaouais, Abitibi and Côte-Nord. City of Montreal is omitted. (d) The baseline hazard comprises over 21 parameters. Specifications (6)–(9) interact the baseline hazard with each age group.
than 40 months and the spell length has been split into 21 intervals. The monthly intervals are: 1, 2, 3, 4, 5, 6, 7, 8, 9, 10, 11, 12, 13, 14, 15–16, 17–18, 19–21, 22–25, 26–31, 32–39, 40+. The time-varying variables are computed for each interval following the month of entry into the social assistance program. For intervals that last more than one month, the variables are averaged out over the interval.

4.1. Results for the samples of men

Tables 2 and 3 report the results of the semi-parametric proportional hazard models yielding DD and BA estimates of the impact of the reform for men and women, respectively. Except for the last two columns, the sample period used for estimation is 1983–1993. Column 1 presents estimates for the simplest model with only one treatment group (the 18–29 age group), one control group (the 30–45 age group) and no additional covariates. Columns 2–7 report results from the specifications that include increasing numbers of covariates. Finally, Columns 8 and 9 report estimates for other sample periods.

Based on the DD approach, Column 1 of Table 2 indicates that the common impact of the post-reform period on both the treatment and control groups, which is given by the estimated coefficient of the Post 89 dummy variable, is to reduce their exit rates from welfare by 11.4%. However, the estimated coefficient of the Post 89×18–29 interactive variable indicates that the reform itself has decreased the exit rate of men in the 18–29 age group by 33.4%. This estimate is significant at the 1% level. On the other hand, based on a simple BA estimator, which assumes that no factors other than the reform influence the exit rates of the treatment group, the impact of the reform would be more substantial, inducing a reduction of the exit rate of this group by 44.8% (≈ 0.334 – 0.114). Moreover, the equality between the BA and the DD estimators is statistically rejected, since their difference, which is equal to the coefficient of the Post 89 variable, is significantly different from zero at the 1% level. Since it is likely that part of the decrease in the exit rates observed after 1989 is due to common factors affecting both the control and the treatment groups, the BA estimator probably overestimates the impact of the reform. In any case, based on either the DD or the BA estimator, our results provide strong evidence that higher benefits raise the duration of welfare spells of single male claimants under 30. In discussing results on men, we will focus mainly on the DD estimators since for most specifications the difference between the BA and the DD estimators is positive and significant.

Column 2 shows results when the effect of the reform is allowed to vary across three age sub-groups: 18–21, 22–24 and 25–29. According to the parameter estimates, the reform has significantly reduced the exit rates of each of these sub-groups. Moreover, the estimated impact decreases with age. The hypothesis that these three sub-groups have similar exit behaviour is strongly rejected by a likelihood ratio test (χ² = 122.2 vs. a critical value of χ²0.05(4) = 9.49).18 Therefore, all other specifications are based on three treatment groups.

18 We also performed tests with a larger number of age sub-groups but we could not reject the hypothesis that they could be aggregated into the three sub-groups we use in the analysis.
In Column 3, covariates for seasonal fluctuations (three seasonal dummies), cyclical activity (adult male unemployment rate) and labour market policy (minimum wage in 1986 $Can), all in interaction with age groups, are added to the model. All the parameters estimates are statistically significant. The results are striking in that they show that introducing these covariates strongly reduces the estimated effect of the reform on exit rates. Its impact decreases (in absolute value) from $-41.7$ to $-23.3\%$ for the 18–21 age group, from $-30.9$ to $-22.1\%$ for the 22–24 age group, and from $-27.5$ to $-23.6\%$ for the 25–29 age group. These results confirm the importance of controlling for covariates that are likely to affect the relative exit rates of treatment and control groups over the pre- and post-reform periods.

Column 4 introduces individual observable covariates into the model (schooling, age, indicator for being born in Canada and region of residence). Experimentation showed that schooling variables should preferably be interacted with age groups. A likelihood-ratio test with a statistic of 860 (with a critical value of $\chi^2_{0.05}(19) = 30.1$) rejects the hypothesis that the joint effect of these additional covariates is zero on the hazard rates. Adding these individual covariates reduces the effect of the reform on all treatment groups. The impact, as measured by the DD estimators, is now about $-14.\%$ on the exit rates of the 18–21 and 22–24 age groups and $-20.7\%$ on that of the 25–29 age group.

Ten yearly dummies are introduced in Column 5 to take into account unobservable factors such as technical and demographic changes that vary over time and which are assumed common to all age groups. Although the null assumption that these year dummies are jointly zero is strongly rejected by a likelihood-ratio test ($\chi^2 = 52.6$ vs. a critical value of $\chi^2_{0.05}(10) = 18.3$), inclusion of these variables has little effect on other parameter estimates, and in particular on those associated with the impact of the reform. In Column 6, the baseline hazard is allowed to vary across age groups. Four baseline hazard are therefore estimated along with the other parameters of the model. Again, although the hypothesis that the baseline hazard is identical for all age groups is strongly rejected (likelihood statistic of 536.8), the DD parameter estimates associated with the reform do not vary much. It increases by at most 3.2 percentage units (for men in the 22–24 year group).

In Column 7, the year dummies are interacted with age groups. This allows the time-specific effects to vary across the four groups. A consequence of this change is to make the impact of the reform non-significant for men in the 18–21 age group. There are two possible explanations for this. First, as mentioned earlier the impact of the reform may be poorly identified with this specification, since it relies solely on two quarters (summer and fall quarters of 1989). Second, it may be that the positive impact of the parental contribution test on their exit rates has offset the negative impact of higher maximum benefits. Indeed, the reform still has a negative effect on the exit rates of men in the 22–24 and 25–29 groups. As mentioned earlier, the latter groups are much less likely to have been affected by the parental contribution test. According to the estimates, the reform reduced the hazard rates of these groups by 25.3\% (asymptotic $t$-statistic of 3.6). Thus gradually increasing the number of covariates does not remove the impact of the reform on

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19 More detailed tables and a more in-depth analysis of the effects of covariates on the exit rates can be found in Fortin et al. (2002b).
the exit rates of these groups. Note that in this specification the BA estimator is still significant for all treatment groups. In particular, it indicates that the reform has reduced the exit rates of the 18–21 age group by 12% and those of the two other treatment groups by 36%.

The next two columns analyze the sensitivity of the results to the choice of sample period using the same specification as in Column 7. Estimates for 1979–1993 with four additional year dummies are reported in Column 8. According to the results based on the DD estimators, the effects of the reform on the 22–24 and 25–29 age groups are \(- 25.3\) and \(- 22.5\)%, respectively. These numbers compare well with those of Column 7. On the other hand, using this sample period, the reform is estimated to reduce the exit rates of men in the youngest age group by 9.1%. This suggests that increasing the number of observations improves the precision of the estimates. However, it is not clear that the estimates obtained from a longer sample period are more accurate than those obtained from the shorter one. Some cohort effects which are not taken into account by time-specific effects are likely to intervene when a longer sample period is used.

As expected, the effect of the reform on the 18–21 age group is, once again, not significant when the sample period used is limited to 1986–1992 (Column 9). On the other hand, the reform is now predicted to have a much stronger negative effect on the exit rate of the two other groups \((- 47.5\)% for the 22–24 age group and \(- 42.3\)% for the 25–29 age group, using the DD estimators). One problem with using this sample period is that the coefficients associated with the minimum wage variables (not shown in the table) are no longer identified, due to a lack of variability over the period.

The estimated exit rates can be converted into expected durations (see Katz and Meyer, 1990 for details). Elasticities of expected duration with respect to benefits can be estimated by computing the ratio of the percent change in expected duration associated with the reform and the percent change in maximum benefits \((= 147\%)\), for each treatment group. Table 4 reports such elasticities along with their standard error for each group and gender, for two sample periods and using the DD estimators. Elasticities are also calculated for two hazard models. The simplest specification is similar to the DD regression with three treatment groups but no covariates (see Column 2 of Tables 2 and 3). The other specification includes the full set of covariates (see Column 7 of Tables 2 and 3). In specifications which use the sample period 1983–1993, duration elasticities for men are smaller when covariates are included in the model. This is particularly the case for men in the 18–21 age group for which the elasticity is reduced from 0.405 (significant) to 0.007 (non-significant) in the model with covariates. The elasticities of the two other groups also decrease but to a much lesser extent. Those of the 22–24 age group are reduced from 0.325 to 0.243, while those of the 25–29 age group decrease only from 0.278 to 0.264. These results can be used to predict the impact of the reform on spell duration for men in the 22–24 and 25–29 age groups, given that their predicted mean duration in the absence of the reform is 10.4 and 9.95, respectively. Based on the model with the full set of covariates, the reform is estimated to have increased their mean duration by 3.7 and 3.9 months, respectively.

\[20\text{ The standard errors are computed numerically using the Delta method.}\]
4.2. Results for the samples of women

Results for women are reported in Table 3. The set-up of the table is identical to that of Table 2. Qualitatively, the results are very similar to those for men. In particular, a likelihood test based on comparison between Columns 1 and 2 strongly rejects the hypothesis that the impact of the reform does not vary across age sub-groups.

Focusing on Column 7 and based on DD estimators, our results show that the reform has had a negative impact on the exit rates of all women under 30. Women in the 18–21 age group have been less affected than those in the 22–24 age group. This is consistent with the hypothesis that the incidence of the parental contribution test was higher among the youngest age group. One must be cautious however to draw any firm conclusion on this issue given that the estimated effect of the reform is likely to be poorly identified and that its measure is not very robust to the choice of the sampling period [see columns (8) and (9)]. On the other hand, contrary to men, the BA estimators of the impact of the reform on women are not statistically different from the corresponding DD estimators, for all specifications covering the period 1983–1993. Indeed, no Post 89 estimates are significant at the 5% level.

Table 4
Elasticities of expected durationa

<table>
<thead>
<tr>
<th></th>
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<th></th>
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</thead>
<tbody>
<tr>
<td></td>
<td>Specification (1)b</td>
<td>Specification (2)c</td>
</tr>
<tr>
<td>Men</td>
<td></td>
<td></td>
</tr>
<tr>
<td>18–21</td>
<td>0.405</td>
<td>0.007</td>
</tr>
<tr>
<td></td>
<td>(0.047)</td>
<td>(0.020)</td>
</tr>
<tr>
<td>22–24</td>
<td>0.305</td>
<td>0.243</td>
</tr>
<tr>
<td></td>
<td>(0.031)</td>
<td>(0.078)</td>
</tr>
<tr>
<td>25–29</td>
<td>0.278</td>
<td>0.264</td>
</tr>
<tr>
<td></td>
<td>(0.031)</td>
<td>(0.085)</td>
</tr>
<tr>
<td>Women</td>
<td></td>
<td></td>
</tr>
<tr>
<td>18–21</td>
<td>0.697</td>
<td>0.112</td>
</tr>
<tr>
<td></td>
<td>(0.076)</td>
<td>(0.027)</td>
</tr>
<tr>
<td>22–24</td>
<td>0.385</td>
<td>0.357</td>
</tr>
<tr>
<td></td>
<td>(0.046)</td>
<td>(0.089)</td>
</tr>
<tr>
<td>25–29</td>
<td>0.035</td>
<td>0.200</td>
</tr>
</tbody>
</table>

Notes: Standard errors in parentheses. Katz and Meyer (1990)’s approach has been used to convert estimated exit rates into expected durations. The percent change in benefits used to compute the denominator of the elasticities is 147%. This corresponds to the impact of the reform.

b Specification (1) distinguishes three treatment age sub-groups but does not include any controlling covariates (see column 2 of Tables 2 and 3).

c Specification (2) distinguishes three treatment age sub-groups but also includes the whole set of covariates (see column 7 of Tables 2 and 3).


e Specification (4) corresponds to specification (2), but for the period 1986–1992 (see column 9 of Tables 2 and 3).
Table 4 shows that, based on results for the period 1983–1993, elasticities of duration with respect to benefits are quite similar to those of men, except for the 18–21 age group. Based on the specification using the full set of covariates and using DD estimators, the elasticity estimates are all statistically significant and are equal to 0.112 for the youngest group, 0.357 for the 22–24 group and 0.2 for the 25–29 age group. Using these results and given predicted mean durations of 10.1, 14.2 and 14.6, respectively, the reform of 1989 is estimated to have increased mean welfare spells by 1.66, 7.45 and 4.29 months, respectively. Also, the estimated mean elasticity (weighted by the relative number of observations) for the 22–29 age group is 0.28 and the reform is estimated to have increased the mean welfare spells of this group by 5.9 months.

All in all, our results suggest that the 1989 reform has increased the financial burden of the welfare system dedicated to young able-bodied single individuals by as much as 170.5%. This is a compounded effect that results from a 145% increase in benefits and the resulting 25.5% increase in the average spell duration.

Our results are consistent with those reported by Barrett (2000) who uses administrative data from the Canadian province of New Brunswick for the period 1986–1993. Even though no major reform was implemented in that province over that period, changes in real benefits were sufficiently large to allow estimation of the impact of benefits on welfare duration. His analysis does not distinguish between single and married claimants and results are not reported by age groups. Yet, the elasticities of duration with respect to benefits can be calculated as being approximately equal to 0.22 and 0.32 for men and women, respectively.

5. Conclusion

This paper provides a first attempt to estimate the impact of benefits levels on welfare duration of various groups of single claimants. To this end, we exploit information on a major reform of the social assistance program enacted in the Province of Quebec in 1989. One important characteristic of this reform is that it more than doubled maximum benefits for single individuals under 30 years of age. In contrast, the reform left the level of benefits for single individuals of 30 years of age and over virtually unchanged. The latter therefore constitute a reasonable control group in the analysis.

The statistical framework adopted in our analysis is flexible in the sense that it does not impose any particular constraint on the baseline hazard rate. Estimations from an approach analogous to difference-in-differences regression are presented and compared with before–after estimations. We first present the simplest uncorrected model and then add complexity to evaluate the impact of additional covariates.

Our estimates suggest that the reform has increased the average spell duration of men in the 22–29 age group by about 3.8 months (an increase of 38%). Overall, the estimated impact on the number of months on welfare is stronger for women. In their case, it is 5.9 months (an increase of 41.1%) for the same age group and varies across age sub–groups, ranging from 4.3 months for the 25–29 age group to 7.45 months for the 22–24 age group.
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