Abstract: The relative age effect is an established phenomenon in the literature, but estimates of its strength and duration vary. In order to study the economic magnitude of the effect, this article obtains a full-count register of all the 3,143,754 residents who paid taxes in Norway in 2008 and map the incomes of monthly cohorts of people born between 1942 and 1991. If the relative age effect is durable and prevalent, it would be expected to generate a different income curve for January-born taxpayers than December-born taxpayers. It does not. There is, however, evidence of time-limited effects in some activities.

JEL Classification: C12, D03, I20, J30

Key Words: birth date effect, life income, relative age

Corresponding Author and Affiliation: Erling Røed Larsen is an Associate Professor, Dept. of Economics, BI Norwegian Business School, N-0442 Oslo. Email: erling.roed.larsen@bi.no. Phone: +47 48 09 22 63. Fax: +47 23 26 47 88

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1. Introduction

When fresh cohorts of Norwegian children begin their first year of school in August every year, some pupils may be as much as 17 percent older than the youngest. This is the result of the school entry cutoff date, which in Norway is January 1st. A common and much-aired concern among parents is the fear that the relative age disadvantage might seriously affect their children’s experience of school. This concern is neither misplaced nor unwarranted given that economists, sociologists, and psychologists have demonstrated the existence of such a birth date, or relative age, effect. However, both the strength and duration of the effect have remained elusive due to data limitations, despite calls from policy-makers, economists, and parents to determine them. What they want to know is whether the school cutoff date matters to the full employment of ability, optimal allocation of talent, and complete utilization of economic capacity. In order to estimate the strength and duration of the relative age effect, this article takes the observed income in a complete register of all 3,143,754 residents who paid taxes in Norway in 2008 to map the life profile of income of all monthly cohorts of people born between 1942 and 1991. This article reasons that if the effect is strong, long-lasting, and affects a significant part of the spectrum of activities, we would expect it to leave an economic imprint on pay over the life-time. Thus, this article poses a very simple question: Given that schools practice cutoff dates, do different monthly cohorts display different profiles of life income?
The short answer is no, they don’t. After examining a complete-count register of Norwegians who paid taxes in 2008 I found no significant difference in the income profile of monthly cohorts measured from 16 to 66 years of age. These data and this answer may help settle the debate on the strength and duration of the birth date effect, which has raged since Allen and Barnsley’s (1993) seminal article. Almost two decades ago, they reported that 72 percent of 16–20-year-old Canadian ice hockey players who played at the highest level were born in the first half of the calendar year. Many researchers have since hunted for evidence for the prevalence of the relative age effect. For example, Bedard and Dhuey (2006) found childhood maturity effects to be persistent and substantial. In Germany, where the cutoff date is in the middle of the year, Mühlenweg and Puhani (2009) discovered a substantial disadvantage to being born in June rather than July. McEwan and Shapiro (2008) demonstrate how delaying enrollment in school by one year is advantageous for pupils in Chile since it decreases the probability of having to repeat the first grade by 2 percent. Cascio and Lewis (2006) utilize differences in educational attainment between pupils on either side of school entry cutoff dates in the United States to estimate the effect of additional schooling. Solli (2011) finds that Norwegian pupils born late in the year lag behind their elders in achieving a high school degree, are less likely to enroll in college, and have lower earnings at 30 years of age. But even if interest in the subject matter is old and many studies demonstrate the disadvantages of being a younger pupil, the issues are far from settled. Dong (2010) detects relatively short-lived effects. Elder and Lubotsky (2009) find that age-related differences in early school performance largely are driven by skill aggregation before kindergarten and disappear as the children grow older. Cascio and Schanzenbach (2007) find no evidence of relative age impacts on achievement in the population at large. Buckles and Hungerman (2008) even discover systematic differences between mothers who give birth in the winter and mothers
who give birth in the summer; a difference they claim can explain up to half of the association between season of birth and adult outcomes. In addition to this, Black, Devereux, and Salvanes (2011) find a possibility of a reversed effect since they are able to use Norwegian data on school start and IQ measured at 18, in which they detect a small positive effect on IQ of starting school younger.

In fact, because of the richness of the Norwegian data, they also employ income data from Norway. But they use data on people of age 24 to 35 only, and thus their results cannot answer this article’s question of the income differences of across all ages from 16 to 66. To see the importance of including all years, we need only reason that it is entirely possible that if being older is advantageous when people are young, being younger may be advantageous when people are old. The effects may cancel out or not; we cannot know without access to analysis of all ages. Moreover, the results of Black, Devereux and Salvanes open up the question of whether absence of evidence truly is evidence of absence. The absence of a relative age effect in Norway may or may not be universal. This article proposes that it would be more interesting to find that an existing short-run relative age effect seems to disappear in the long-run than to find that there is no long-run effect when there is no short-run effect.

Thus, the study by Black, Devereux, and Salvanes needs to be augmented in two directions; the coverage of age-span and a thorough search for a relative age effect in some activities during some years to establish the existence of a relative age effect at all in Norway. In short, the disagreement among experts and divergence in estimates of the strength, causality, and duration of the birth data effect are obviously highly unsettling given the importance of the matter.
In reporting the results of regressions on a comprehensive data set, this article seeks to move the issue forward. This data set includes observations drawn from a register of incomes and month of birth of every tax-paying resident of Norway in the year 2008 who was born between 1942 and 1991; 3,143,754 taxpayers in all. Econometrically, I test and reject the hypothesis that two separate regressions of income onto a quadratic polynomial of age, one for January cohorts and one for December cohorts, will yield different results than a pooled regression of both. In fact, the life-income profiles are almost indistinguishable from each other. Moreover, I test and reject the hypothesis that one overall regression of income onto a quadratic polynomial of age and the within-year number of months between January and birth month would yield a statistically significant coefficient estimate for the latter variable. It does not.

However, despite very similar average life-time earnings in monthly cohorts, the income paths of different monthly cohorts through life are not exactly the same. There appears to be a slight shift of the income curves measured both in terms of cohort year and absolute age. First, for the youngest 20 vintages, the incomes of the December-born taxpayers tend to be lower than those of January-born taxpayers. People born in January are older, and maturity pays off long into the 30s. Interestingly, the effect is reversed for taxpayers in their late 50s and early 60s. By then, people born in December are younger, and youth pays off in old age – as shown by the data. Second, people born in December, or any month late in the year, reach a given income at a younger age. To see how we can say that, keep in mind that for each yearly cohort the January-born taxpayers will be older than the December-born taxpayers by 11 months, so that comparisons within yearly cohorts are likely to yield differences. Thus, when we control for age, using the above-mentioned regression, we can compare estimated incomes of January-born and December-born, November-born, and October-born taxpayers at identical
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ages. By disentangling yearly cohort from absolute age we find that taxpayers born late in the year actually tend to reach a given income level when they are younger than January-born taxpayers, most likely since they are the youngest in any year-cohort, and thus reach work experience thresholds when they are younger than their January-born co-graduates and colleagues.

There is, though, strong evidence against a large and long-lasting effect of month of birth on overall income profile over the life span. One might reasonably object, however, that this finding could have been caused by sample selection bias. But our dataset is not a sample; it is a full-count universe of all Norwegian taxpayers. Alternatively, as mentioned above, Norway could be an aberration; results from Norway would therefore be irrelevant for the rest of the world. Indeed, this could be due to Norway’s well-documented population homogeneity and income equality. If so, the result would not indicate an absence of a lifetime effect of birth date in the wider world, only that is simply does not exist in Norway. In order to test the strength and duration of birth date effect it must first be shown to exist; an endeavor Black, Devereux, and Salvanes does not undertake. This article therefore goes to some length to establish and corroborate the presence of a birth date effect in Norway. I do this by examining data from several different sources, in order to span the spectrum across dimensions of activity, age, selectiveness, and talent.

The procedure runs as follows. This article gathers data on the 953 Norwegian-born elite soccer players who played in the top division, Norway’s premier league (in Norway known as the “Tippeliga”), in the period 2000–2010. I do this since the literature points to sports as an activity in which the relative age effect is often present. In addition, I acquire historical data on every unique observation of 4,906 top politicians, i.e. former or current members or deputy
members of Parliament, over a period spanning more than six decades, 1945–2011. The article does this because it views politics as something very different from sports; thus, including two different activities allows us to span out a spectrum. Moreover, the article identifies month of birth of the 537 members of the prestigious National Association of Writers, an association to which a writer is only eligible after publishing works of recognized literary merit. Finally, data are constructed on 487 business leaders selected from the list of Top 500 Companies, ranked by revenue. Using these data, we observe a clear birth-date effect in sports, an absence of an effect in politics, and a reversed effect in business.

A third objection would cite non-compliance to explain the absence of a long-lasting relative age effect. While early and late school starts would reduce the relative age effect, it would not mean it did not exist, only that it was partly reversible and, moreover, hard to detect. After all, if the idea is to let the data mimic a randomized controlled trial (RCT) by randomly assigning a treatment (being born early in the year) to some participants and no treatment to others (being born later in the year), the assumption of the experiment architecture is violated if some members of the experiment group join the control group without the knowledge of the experimenter. I take this objection seriously and therefore explore two ways to control for it. Although Bedard and Dhuey cite Norway as a useful country for data since there is little or no evidence of early/late starting or grade retention, both Solli and Black, Devereux, and Salvanes found non-compliance, especially among pupils born in January and December. This article inspects the possible presence of non-compliance by investigating how income profiles of a January–December comparison, where non-compliance is at its maximum, fare against income profiles of a March–October comparison, where non-compliance is much less frequent. All results are intact.
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The Norwegian data do in fact prove to be useful since the coverage is comprehensive (100 percent participation); variables are not self-reported (but come from an income register compiled by a tax authority); and contain births over a substantial period (1942–1991). These data can therefore function as an ex post laboratory to study economic ramifications of the birth date effect. Thus, I view my contribution as purely empirical.

The article proceeds as follows. Section two describes our data and method of data acquisition and section three presents a simple, interpretative theoretical framework that yields a null hypothesis, i.e. that the birth date effect leads to a long-lasting effect on life-income. It also includes a brief presentation of the statistical apparatus. The fourth sets out our empirical results. The subsequent section discusses merits of and weaknesses in the methodology. The final section concludes and offers brief comments on policy implications.

2. Data

a. Data on income and birth date

Our main data source is a complete-count register of every single taxpayer in Norway for the year 2008, acquired by Statistics Norway from the Norwegian IRS. These data provide information on income defined as “gross income including tax free transfers” and date of birth. I utilize observations on the full registry of the 3,143,754 tax-paying residents of Norway in 2008 born in the period 1942–1991 (out of a total population of 4.8 million). As our data set is limited for legal reasons of confidentiality to first and second order moments of income for monthly cohorts in the time period 1942 – 1991, the data set consists of three 50-by-12 matrices of 600 entries of average income, its variance, and number of agents. This article fixes the age of taxpayers to the age of taxpayers in January 2008 as the point of reference. Since the data set is a complete count of every taxpayer it includes legal immigrants who paid...
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taxes in Norway. In order to pay taxes in Norway every immigrant needs to have a social
security number, and this involves a registration of personal information, including date of
birth. It is reported, however, that the accuracy of date of birth for immigrants varies with the
country of birth, and for some taxpayers the precision may not be of Norwegian quality;
where the time of birth is precise to within minutes.

b. Data on elite soccer players, politicians, writers, and business leaders

Information provided by the website¹ altomfotball.no allowed us to establish month of birth
of the 953 Norwegian-born leading soccer players (i.e. members of teams featured in the
Tippeligaen, Norway’s premier league), during the 11-year period 2000–2010. For politicians
I used information available on the Norwegian Parliament’s own webpage, stortinget.no, and
constructed code to identify unique representatives and deputies from 1945 to 2011.
Algorithms ensure avoidance of double-counts. The end result consists of month of birth for
4,906 unique observations of former or current members of parliament (MP) or deputy
members (“vara-representant”). Artists comprise our third data set. I obtained the month of
birth of all 537 members of the prestigious National Association of Writers, an association
whose membership is restricted to writers of works of particular literary merit, from
publically available websites. Our online search also yielded data on prize winning actors
(“Amanda Prize”), musicians (“Spellemannsprisen”), writers (“Brageprisen” and
“Riksmålsforbundets litteraturpris”), and journalists (“Gullpennen”).

I identified Norway’s leading 500 companies by revenue, utilizing the website
norgesstorstebedrifter.no. I then located the names and months of birth of the directors from

¹ With permission.
the website “Proff forvalt”. As I was unable to trace nine companies and thirteen directors, our dataset comprises the months of birth of 487 business leaders.

3. Interpretative Framework and Empirical Techniques

a. A Framework with Birth Date Effects

There is a substantial literature on the theory of formation of ability. For example, Todd and Wolpin (2003) propose a production function of cognitive achievement, where factors encompass endowments, family inputs, and school inputs. Heckman (2000) emphasizes the dynamic nature of skill formation and theorizes that skill acquisition leads to further skill acquisition. Elder and Lubotsky (2009) use a model of human capital accumulation in which it acts as a function of parental investment, schooling, and the historical human capital. In this model, the return from schooling in early grades may be larger for children who are older at entry. Bedard and Dhuey (2006) posit a relationship between student outcomes and age, while adding a number of controls such as number of siblings, birth date, parental education, access to books, and rural status. Falch and Sandgren Massih (2011) explain IQ scores at 20 years of age by employing factors such as IQ scores at 10 years of age, early birth, schooling, father’s education, GPA in third grade, teacher rating, and month of birth. Our strategy in this article is first to synthesize these contributions and construct a simple theoretical framework to understand the possible long-lasting effects of relative age at school entry on performance, and thus pay, and then use our data to test whether the relative age effect lasts a lifetime.

To that end, let $P_{it}$ be the qualitative performance, or productivity, of agent $i$ at time $t$ and let $w_{it}$ be his hourly wage. I assume the former is reflected in the latter, with stochastic noise as given in equation (1):
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(1) \[ w_i' = \omega(P_i') + \epsilon_i', \text{s.t. } \frac{\partial w}{\partial P} > 0, \]

where the mapping function \( \omega \) is unknown and the disturbance \( \epsilon \) is assumed classically distributed with zero mean and constant variance. I assume that \( \omega \) is monotone and nicely behaved so wage is a proxy for performance. I also assume that the number of working hours is randomly distributed across month and age cohorts, and allows the use of annual income as a proxy for annual performance.

Assume further that performance is a function \( f \) of unobservable genetic ability, age (biological maturity), and training and assume that it can be monitored and evaluated. Without loss of generality, let us use a simple, linearly additive approximation of the latent function \( f(.) \). In equation (2), let us write performance \( P_i' \) of agent \( i \) at time \( t \), which can be measured by its proxy wage \( w_i' \), as a linear combination of three factors:

(2) \[ P_i' = f(\xi_i', A_i', TR_i') = \beta + \gamma \xi_i' + \alpha \min(18, A_i' / 18) + \theta TR_i' + \delta_i', \]

where \( \xi \) represents unobservable genetic ability, \( A \) denotes age, and \( TR \) denotes accumulated training in excess of depreciation and memory loss. Again, the error term contains omitted variables that are assumed to be orthogonal to the determinants and is modeled as a nicely behaved random variable with zero-mean and constant variance. The age term is set to avoid performance-enhancing effects from age for individuals above the maturity level of 18 years of age. (It could in fact be reversed for age at higher thresholds; see Discussion.)
Imagine that both the agent himself and external observers are able to judge the quality of his performance relative to others. If so, there exists an early-detection procedure of talent that continuously tests and evaluates the performance of young individuals (below 18) at time t. Assume further the existence of internal and external selection algorithms, SA, that combine to rank agents. Internally, agents observe their own performance, but even though many activities are in themselves rewarding we may assume that agents extract negative utility from not performing as well as others. Poor performers self-select away from activities in which they do relatively badly. Moreover, externally, coaches and teachers also assess performance and select some good performers to additional practice. This process entails a self-reinforcing effect, as in Heckman (2000), and cumulative learning, as in Elder and Lebotsky (2009).

These selection mechanisms also comprise a process in which high-performers are given, or give themselves, additional practice. In equation (3), we may simplify this complex process to the following: performance, internally or externally assessed at time T by selection algorithms SA, above a quality or quantity threshold performance requirement $\pi^T$ that depends on the age cohort at that time, $a + bC^T$, qualifies for additional training. Poorer performance does not.

$$SA^T = \begin{cases} 
q, P^T \geq \pi^T = a + bC^T \\
fail, P^T < \pi^T = a + bC^T^*
\end{cases}$$

Two individuals under 18 of identical genetic ability and same amount of accumulated training, i.e. with identical $\xi$ and TR, within the same age cohort $C$ will be expected to score differently if their ages are not exactly the same. The difference in performance $DP$ at time $T$ is given by equation (4):

$$DP^T = \alpha(A_j^T - A_n^T) + \epsilon_j - \epsilon_n,$$
where the difference $A_j^T - A_n^T$ is 11 months if individual $j$ belongs to the January cohort and individual $n$ belongs to the December cohort, and where the expected value of the error term component is zero (and so tends to vanish when differences are computed for large groups of observations). In fact, the difference in performance $DP$ is relatively larger the younger the age cohort. The detectable difference in performance, $DP$, thus stands out even from a rough inspection of 6-year-olds but is almost invisible for 17-year-olds, all else being equal. Let qualified individuals, i.e. those who score above $q$, be exposed to an additional training dosage of $d_1$ and not-qualified individuals a training dosage $d_0$ per time unit. The difference in performance of two identically gifted individuals, amounts to the accumulated effect of being subjected to different training regimes:

\[
DP^{+\tau} = \theta \tau (d_1 - d_0) + \varepsilon_j - \varepsilon_n,
\]

where the dosage length $\tau$ denotes the number of years of training after the former individual $j$ qualified and individual $n$ did not. At this stage, this difference is real, of course, and may also be readily observable. The interpretation of the literature within this framework is that a strong and persistent birth date effect tends to occur more frequently, last longer, and be of a greater magnitude in activities where selection algorithms are implemented early and extra training dosages are substantial. Sports like ice hockey, soccer, American football, and cross-country skiing may be good candidates, and at least equations (1) – (5) are consistent with the asymmetrical birth dates found by Allen and Barnsley (1993) among Canadian ice hockey players.
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If the birth date effect also affects lifelong cognitive abilities, work-skills acquisition, and vocational choices, it would, I hypothesize, imply a difference in the life income profile between January cohorts and December cohorts.

b. Statistical Tests and Inferences

Equations (1) – (5) constitute the theoretical framework with which one can understand a lasting birth date effect, and let us now turn to statistical tests of its validity. I compare incomes of January and December cohorts for all birth years in the period 1942–1991 as reported in the year 2008. First, in order to investigate whether there are differences at all, I compare the observed mean income of all taxpayers belonging to the January cohort of vintage $v$ (year of birth), $X_1^v$, with the mean income of all taxpayers belonging to the December cohort of vintage $v$, $X_{12}^v$. I test the hypothesis that these two statistics could have been derived from a space with the same normal distribution of income, characterized by mean $\mu$ and variance $\sigma^2$. The alternative hypothesis is that the two statistics are derived from separate, distinct spaces of normal distributions resulting from different economic processes.

However, such initial tests of differences between same-year monthly cohorts test relative age effects on stages of life, but not over a life span. Also, it tests differences of cohorts of a given year of birth, which means that the two monthly cohorts are not of identical age. The January cohort is always 11 months older. An observed difference in average income could therefore be the result of absolute age, not necessarily additional training brought on by an early relative age effect. In order to control for absolute age, given that January cohorts per construction are always 11 months older than same-year December cohorts, we need to estimate mean income reached at comparable ages. To this end, this article uses a regression
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technique in which it regresses the mean income of a monthly cohort \( j \) of vintage \( v \) onto a
space spanned by a quadratic polynomial of absolute age, as given in equation (6):

\[
\bar{X}_v^j = c_j + \frac{d_{1j}A_v^j}{v} + \frac{d_{2j}(A_v^j)^2}{v} + u_v^j, \quad j = 1,12; v \in \{1,...,50\},
\]

where January-born of the youngest vintage, taxpayers born in 1991, are re-normalized to
have age 1 year and December-born taxpayers are renormalized to be of age 0.08 year. The
disturbance is assumed to be classical with mean-zero and constant variance. However,
employing data on January-born and December-born taxpayers limits our utilization of the
data set to 1/6 of it. In order to examine the full set and partially to control for non-compliance
effects prevalent in January and December, I augment the regression model in equation (6) by
including a variable \( \text{NMJB} \) that measures the number of months from January to the birth
month, as given in equation (7):

\[
\bar{X}_v = c_F + \frac{d_{1F}A_v}{v} + \frac{d_{2F}(A_v)^2}{v} + d_{3F}\text{NMJB}_v + u_v, \quad v \in \{1,...,50\},
\]

where the subscript \( F \) refers to full data set utilization. From the regressions in equations (6)
and (7) we obtain life profiles of incomes at different absolute ages as reported in 2008. Even
though the constructed theory presented in equations (1) – (5) explains the mechanisms of the
relative age effect, I take, for logical reasons, as the null hypothesis the proposition that the
income profile across all ages of January-born taxpayers is identical to that of December-born
taxpayers, i.e. that \((c_1, d_{1,1}, d_{2,1}) = (c_{12}, d_{1,12}, d_{2,12})\), and that the number of months from
January to the birth month is zero, i.e. \(d_{3F} = 0\). The alternative hypothesis is that the income
profile across all ages of January-born taxpayers differs from that of December cohorts and
that \(d_{3F}\) is non-zero. Put differently, according to the null hypothesis, there is no long-lasting
relative age effect; alternatively, there is such an effect and it lasts across all ages between 16 and 66. The null of equation (6) implies equality of the vector of parameters and the alternative that linear restrictions of equality do not hold, as described by Greene (1993, p. 206). For this purpose, we employ an F-test. The null of equation (7) implies that the coefficient $d_{3F} = 0$, where statistical inference is done by a simple t-test of coefficient $d_{3F}$.

In order to explore whether there exists a birth date effect in Norway in professional and vocational sub-groups, I test the statistical properties of selected activities in sports, politics, arts, and business. To that end, I broaden the perspective from months to quarters of a year and test a second type null hypothesis, given in equation (8):

$$H_0^2 : \{f_1, f_2, f_3, f_4\} - \{\phi_1, \phi_2, \phi_3, \phi_4\} = 0,$$

where $f_i$ is the $i$’th quarter’s frequency of births in the given sample of a professional sub-group and $\Phi_i$ is the $i$’th quarter frequency of births in the population register. Under the alternative hypothesis the equality does not hold and a difference in frequency will obtain between the distribution of birth dates in the given profession and in the population. Two tests are available, asymptotically equivalent under the null: Pearson’s chi-square and the likelihood ratio test. I use the former. It is important to note that the birth frequency is not uniformly distributed over the year. Observing the distribution of number of births from the tax data set, we find that 26.7 percent of all births take place in the second quarter, and only 23.1 percent in the fourth quarter. When computing the Pearson’s chi-square I control for this birth frequency difference.

4. Empirical Evidence
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**a. Income**

Let us start the inspection of empirical evidence by a close comparison of January-born and December-born taxpayers, before we turn to all months. In Figure 1, I plot the observed difference between mean income of January-born taxpayers and December-born taxpayers for vintages 1942–1991. We observe that the January-born taxpayers have substantially higher income for the about the youngest 20 vintages, i.e. workers aged 16–17 to 36–37, but that December-born taxpayers have the highest mean for at least the oldest 10 vintages, i.e. workers from 55–56 to 65–66. In Table 1 I report statistical tests of some selected annual income differences; here we see that e.g. for the vintage of taxpayers born in 1987 the January-born taxpayers made on average NOK 174,573 p.a. and the December-born taxpayers had a mean income of NOK 165,278 p.a. This difference is statistically significant with a t-distributed test statistic of 3.92.

**Figure 1. Income Differences (NOK) between January and December Cohorts Born in the Same Year. 1942–1991. All Tax-Paying Residents. Norway. 2008**

Source: Statistics Norway.
(Income in NOK)

<table>
<thead>
<tr>
<th>Cohort</th>
<th>Cohort Income</th>
<th>No. of Taxpayers</th>
<th>Standard Deviation</th>
<th>Test-statistic of Income Diff.</th>
</tr>
</thead>
<tbody>
<tr>
<td>January 1988</td>
<td>145,591</td>
<td>5,089</td>
<td>99,931</td>
<td></td>
</tr>
<tr>
<td>December 1988</td>
<td>140,345</td>
<td>4,859</td>
<td>197,024</td>
<td>1.69</td>
</tr>
<tr>
<td>January 1987</td>
<td>174,573</td>
<td>4,681</td>
<td>116,895</td>
<td></td>
</tr>
<tr>
<td>December 1987</td>
<td>165,278</td>
<td>4,677</td>
<td>112,229</td>
<td>3.92</td>
</tr>
<tr>
<td>January 1978</td>
<td>364,730</td>
<td>4,877</td>
<td>235,730</td>
<td></td>
</tr>
<tr>
<td>December 1978</td>
<td>344,766</td>
<td>4,810</td>
<td>201,124</td>
<td>4.48</td>
</tr>
<tr>
<td>January 1977</td>
<td>371,849</td>
<td>5,055</td>
<td>479,165</td>
<td></td>
</tr>
<tr>
<td>December 1977</td>
<td>357,788</td>
<td>4,579</td>
<td>205,342</td>
<td>1.84</td>
</tr>
<tr>
<td>January 1968</td>
<td>439,430</td>
<td>5,994</td>
<td>444,670</td>
<td></td>
</tr>
<tr>
<td>December 1968</td>
<td>447,268</td>
<td>5,966</td>
<td>426,357</td>
<td>-0.98</td>
</tr>
<tr>
<td>January 1967</td>
<td>451,465</td>
<td>5,802</td>
<td>436,252</td>
<td></td>
</tr>
<tr>
<td>December 1967</td>
<td>451,086</td>
<td>5,890</td>
<td>381,871</td>
<td>0.05</td>
</tr>
</tbody>
</table>

Note: The test statistic is described in the section on theory.

However, in January 2008 the December cohort of 1987 was 20.08 years old, and the January cohort of the same year 21 years old. These 11 months imply the possibility that the difference in pay was due to absolute age and not relative age. Ideally, we would include the income of the 1987 December cohort in the test when taxpayers in that cohort had reached the age of 21, not 20.08, i.e. by timing the observation 11 months later. Such data do not exist. However, in the theory section I describe a way to control for absolute age using regression
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techniques. By regressing the mean incomes of December cohorts onto a space spanned by a second order polynomial of age, where age observations for December-born taxpayers are re-normalized absolute ages such as 0.08, 1.08, 2.08, …, and regressing the mean incomes of January-born taxpayers, where age observations are re-normalized absolute ages such as 1.0, 2.0, 3.0…, we have a means to control for absolute age. Table 2 reports two such regressions, each containing 50 observations (one for each of the 50 vintages) and the pooled regressions of 100 observations. Here, the estimated coefficients are somewhat different. In fact, the December-born taxpayers reach a given income at a younger age than the January-born taxpayers, potentially because the work they perform is largely the same as their 11-month older friends. Why? One plausible interpretation is that school and work activities are defined by calendar years through the cutoff dates, so if a December-born worker graduates from school in the same class as a January-born worker, and they both start working in similar jobs, the December-born worker will reach their common income level 11 months earlier than the January-born worker. However, this cannot be the only effect since the January-cohorts have higher average incomes; thus we need to disentangle relative and absolute age effects.

From the results reported in Table 2, we make three observations about the separate regressions. First, the estimated coefficients are highly statistically significant. Second, there is curvature since the estimated coefficient for the squared age variable is negative, large in absolute value and comes with a substantial t-value. This means that the income curve is an inverse U and that tax-payers reach their maximum pay when they are in their mid-40s. Third, there is a noticeable difference in the estimated constant. This indicates that December-born taxpayers reach a given income at a younger absolute age than January-born taxpayers; the income profile is slightly shifted.
The Effect of Relative Age on Life Income

Table 2. Estimated Coefficients (t-values) of Life-Cycle Regression Curve for January

<table>
<thead>
<tr>
<th>Variable</th>
<th>January-born</th>
<th>December-born</th>
<th>Both months</th>
</tr>
</thead>
<tbody>
<tr>
<td>Intercept</td>
<td>41,632 (9.0)</td>
<td>55,280 (11.7)</td>
<td>48,963 (14.6)</td>
</tr>
<tr>
<td>Age</td>
<td>27,935 (66.6)</td>
<td>27,091 (61.1)</td>
<td>27,477 (89.1)</td>
</tr>
<tr>
<td>Age squared</td>
<td>-449.0 (-56.3)</td>
<td>-436.0 (-50.0)</td>
<td>-442.0 (-74.2)</td>
</tr>
</tbody>
</table>

No. of Taxpayers 257,499 245,060 492,196

Residual sum of squares 5178134057 6194793136 11996682332

Note: Normalized age is defined as real age above 16 years, so a person who is born in December 1991 is 0.08 years in January 2008.

More importantly, we now turn to testing whether or not the income profile of January-born taxpayers differs from that of December-born taxpayers. I compute the F-statistic with J = 3 degrees of freedom (numerator) and n – K = 94 degrees of freedom (denominator) for two unrestricted regressions versus one restricted regression with three linear restrictions equalizing the three parameters. An F-statistic of 1.72 is small, and statistical tables show that $F_{0.95}[3,94] = 2.70$ and $F_{0.90}[3,90] = 2.14$. Our test statistic is much smaller than the thresholds for statistical significance at the 5 percent and even the 10 percent level, thus we cannot reject the hypothesis that the two life income profiles are identical. Put differently, the income profile of January-born taxpayers is similar to the income profile of December-born taxpayers. In other words, the birth date effect does not appear to have made a different impact on incomes of different monthly cohorts for 50 different years of birth.
The Effect of Relative Age on Life Income

We visualize this by plotting the estimated income profiles given in Figure 2a. The inverse U-shape is clear, indicating increasing incomes in early working years and decreasing incomes as retirement nears. Close visual scrutiny finds little difference between the two estimated income profiles. In Figures 2b and 2c I illustrate the point mentioned above. In Figure 2b, I plot the observed average cohort income of vintages, i.e. incomes of cohorts by year of birth. In this figure, at any and every year, the January-born are 11 months older than December-born, and we observe that for the first 20 years after 16 years of age, the January-born taxpayers have higher incomes. In Figure 2c, however, I plot the observed average cohort incomes against their absolute ages. Thus, since December-born taxpayers are 11 months younger than January-born taxpayers in a given birth year, I plot the income of a December cohort 11/12 of a year to the left of the January cohort. It is now completely clear that December-born taxpayers actually tend to reach a given income level at a younger age than January-born, even if this occurs at a later point in time.

**Figure 2a. Regression-estimated Income Profile for January and December Cohorts.**

Taxpayers in Norway. 2008
The Effect of Relative Age on Life Income

Figure 2b. Average Observed Cohort Income of Vintages. Taxpayers in Norway. 2008

![Average Cohort Income of 1942-1991 Vintages](image)

Note: I have reversed the order of vintages plotted on the horizontal axis in order to make the youngest workers appear at the extreme left, so as to make the income profile easier to compare with the absolute age figure.

Figure 2c. Average Observed Cohort Income at Absolute Age. Taxpayers in Norway. 2008

![Average Cohort Income at Absolute Age](image)
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There is the possibility, of course, that some December-born children are held back and start school one year late and some January-born children are pushed ahead and start school one year early. This is discussed in detail in the Appendix, where one remedy is suggested and applied.

To control for the confounding factor of non-compliance with school starts; early starts or late starts; it would be useful to inspect the data for the months spanning February – November as well. In Table 3, I report the results from an augmented regression in which I include a variable NMJB that measures “The number of months from January to the birth month”. This measures for within-year age-difference. Thus, a taxpayer born in April lags January-born Taxpayers by 3 months and a taxpayer born in October lags January-born taxpayers by 9 months. By this inclusion we obtain a double-dividend. First, we can partly control for non-compliance since non-compliance is by far most prevalent in December and January; see Solli (2011) and Black, Devereux, and Salvanes (2011). Second, we can utilize the variation in the whole data set and obtain estimates with smaller variance than if we limit ourselves to 1/6 of the data. Table 3 reveals at least two important results. First, the estimate of the coefficient for within-year difference is not statistically significant, thus we cannot reject the null of identical distributions of income among different monthly cohorts and thus no relative age effect over the life span. Second, if anything the economic content of the positive sign of the estimate points us in the direction mentioned above, namely that observing two taxpayers when they are, say, 43 years old, you would expect the one with the latest month of birth to have the higher income. Put differently, the later in the year a birth month is the sooner the taxpayer reaches a given income level.
The Effect of Relative Age on Life Income


<table>
<thead>
<tr>
<th>Variable</th>
<th>All Months</th>
</tr>
</thead>
<tbody>
<tr>
<td>Intercept</td>
<td>47,245 (27.1)</td>
</tr>
<tr>
<td>Age</td>
<td>27,697 (196.1)</td>
</tr>
<tr>
<td>Age squared</td>
<td>-446.43 (-163.5)</td>
</tr>
<tr>
<td>No. of Months from January to Birth Month</td>
<td>190.38 (1.3)</td>
</tr>
</tbody>
</table>

Note: Normalized age is defined as real age above 16 years, so a person who is born in December 1991 is 0.08 years in January 2008.

b. Sports

Now the absence of a relative age effect on the income profile over 50 monthly cohorts could possibly be explained by particular circumstances in Norway and thus not be necessarily applicable to other counties. Absence of evidence is not evidence of absence. Black, Devereux, and Salvanes (2011) do not address this issue. To account for the possibility that there is no long-term relative birth date effect because there is no short-term relative birth effect, let us turn to an examination of the possible presence of a short-term or limited-in-scope relative age effect on a wide variety of activities, ranging from early-life vocations as sports to all-life vocations as politics and business.

Soccer is one of the most popular sports in Norway, and talent identification, recruitment and organized training start when players are as young as 6. At 7, children take part in matches,
leagues, tournaments, and competitions. Competitions introduce evaluation and selection. Eligibility for membership of a given group is defined by calendar year, making young January-born players older and on average stronger and faster than young December-born players. Several different tests, sampling mechanisms, and selection methods are used to identify and select the best players. These individuals are not only rewarded with diplomas and being selected for the best teams – which in itself could create a self-reinforcing psychological reward system – but the best players are also given the opportunity to train more often and take part in specialized training schemes. In Table 4, I report the observed number of births in the four quarters of the year for players who played in the premier league during the 11-year period 2000–2010.

### Table 4. Elite Soccer Players by Quarter of Birth. Unique Players. 2000-2010. Norway

<table>
<thead>
<tr>
<th>Quarters</th>
<th>Q1</th>
<th>Q2</th>
<th>Q3</th>
<th>Q4</th>
</tr>
</thead>
<tbody>
<tr>
<td>Observed number</td>
<td>297</td>
<td>254</td>
<td>230</td>
<td>172</td>
</tr>
<tr>
<td>Expected number, uniform distribution</td>
<td>238.25</td>
<td>238.25</td>
<td>238.25</td>
<td>238.25</td>
</tr>
<tr>
<td>Population Frequency(^1)</td>
<td>0.2524</td>
<td>0.2673</td>
<td>0.2491</td>
<td>0.2312</td>
</tr>
<tr>
<td>Expected number, multinomial distribution</td>
<td>240.54</td>
<td>254.74</td>
<td>237.39</td>
<td>220.33</td>
</tr>
<tr>
<td>Difference Sq. on Expected</td>
<td>13.25</td>
<td>0.00213</td>
<td>0.230</td>
<td>10.60</td>
</tr>
<tr>
<td>Chi-square statistic (X^2)</td>
<td>24.09</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No. players born in December</td>
<td>52</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No. players born in January</td>
<td>105</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Note: \(^1\)Estimated from the income register of taxpayers born between 1942 and 1991.
The Effect of Relative Age on Life Income

Table 4 reports a Pearson’s chi-square of 24.09. With three degrees of freedom, statistical tables report that at a 10 percent significance level $X^2_{.90} = 6.25$ and at the 5 percent level $X^2_{.95} = 7.81$, letting us reject the null at any meaningful level of statistical significance. Moreover, the number of elite players born in January is more than double the number of players born in December. In other words, the relative age effect appears to be indisputable in soccer performed the highest level. Notice that I compute the expected number of players in a quarter using the *actual* birth frequencies found in the tax-payer sample in order to control for the differences in frequency. This is important because one reason why there are more elite players born early in the year is that there simply are more people born early in the year. Thus, this effect must be controlled for. I use the proper expected number when I derive the chi-square, and we see from the table that the expected number of players born in the second quarter, given that the frequency of births of elite soccer players is similar to that one of tax players, is 255. Had births been evenly distributed across the year we would only have expected 238 players in the second quarter.

**b. Politics**

In Table 5, I tabulate the frequencies of leading Norwegian politicians of the last six decades, i.e. current and former members of parliament (unique observations) since WWII. With a Pearson’s chi-square statistic of 2.57 we cannot reject the null of a common multinomial distribution. Thus, we find no traces of the birth date effect in politics.
**Table 5. Members of Parliament\(^1\) By Quarter of Birth. 1945–2010. Norway**

<table>
<thead>
<tr>
<th>Quarters</th>
<th>Q1</th>
<th>Q2</th>
<th>Q3</th>
<th>Q4</th>
</tr>
</thead>
<tbody>
<tr>
<td>Observed number</td>
<td>1195</td>
<td>1343</td>
<td>1240</td>
<td>1128</td>
</tr>
<tr>
<td>Expected number, uniform distribution</td>
<td>1226.5</td>
<td>1226.5</td>
<td>1226.5</td>
<td>1226.5</td>
</tr>
<tr>
<td>Population Frequency(^1)</td>
<td>0.2524</td>
<td>0.2673</td>
<td>0.2491</td>
<td>0.2312</td>
</tr>
<tr>
<td>Expected number, multinomial distribution</td>
<td>1238.3</td>
<td>1311.4</td>
<td>1222.1</td>
<td>1134.3</td>
</tr>
<tr>
<td>Difference Sq. on Expected</td>
<td>1.51</td>
<td>0.76</td>
<td>0.26</td>
<td>0.035</td>
</tr>
<tr>
<td>Chi-square statistic (X^2)</td>
<td>2.57</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

| No. members born in December | 377    |
| No. members born in January  | 405    |

Note: \(^1\)Also included deputy members, i.e. substitutes for absent members.

c. **Artistic activities**

In Tables 6 and 7 I tabulate the frequencies of members of the national association of writers and winners of prestigious art prizes. Low chi-square statistics do not allow us to reject the null given that \(X^2_{.90}\) is 6.25 and \(X^2_{.95}\) is 7.81. We do notice, however, the high number of award-winners born in December in Table 7; 24 versus the 15 born in January, but I do not want to over-emphasize the finding.
## Table 6. Members of the National Association of Authors.¹ By Quarter of Birth. 2010.

**Norway**

<table>
<thead>
<tr>
<th>Quarters</th>
<th>Q1</th>
<th>Q2</th>
<th>Q3</th>
<th>Q4</th>
</tr>
</thead>
<tbody>
<tr>
<td>Observed number</td>
<td>140</td>
<td>152</td>
<td>121</td>
<td>124</td>
</tr>
<tr>
<td>Expected number, uniform distribution</td>
<td>134.25</td>
<td>134.25</td>
<td>134.25</td>
<td>134.25</td>
</tr>
<tr>
<td>Population Frequency¹</td>
<td>0.2524</td>
<td>0.2673</td>
<td>0.2491</td>
<td>0.2312</td>
</tr>
<tr>
<td>Expected number, multinomial distribution</td>
<td>135.54</td>
<td>143.54</td>
<td>133.77</td>
<td>124.15</td>
</tr>
<tr>
<td>Difference Sq. on Expected</td>
<td>0.147</td>
<td>0.499</td>
<td>1.219</td>
<td>0.00019</td>
</tr>
<tr>
<td>Chi-square statistic X²</td>
<td>1.86</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

| No. members born in December | 37 |
| No. members born in January | 50 |

¹Note: Members of “Forfatterforeningen”, a highly exclusive and prestigious association, to which membership is granted only to published authors of works of literary merit.
**Table 7. Elite Artists\(^1\). Writing, Acting, and Music by Quarter of Birth. 2010. Norway**

<table>
<thead>
<tr>
<th>Quarters</th>
<th>Q1</th>
<th>Q2</th>
<th>Q3</th>
<th>Q4</th>
</tr>
</thead>
<tbody>
<tr>
<td>Observed number</td>
<td>44</td>
<td>53</td>
<td>49</td>
<td>50</td>
</tr>
<tr>
<td>Expected number, uniform distribution</td>
<td>49</td>
<td>49</td>
<td>49</td>
<td>49</td>
</tr>
<tr>
<td>Estimated probability(^1)</td>
<td>0.2524</td>
<td>0.2673</td>
<td>0.2491</td>
<td>0.2312</td>
</tr>
<tr>
<td>Expected number, multinomial distribution</td>
<td>49.47</td>
<td>52.39</td>
<td>48.82</td>
<td>45.32</td>
</tr>
<tr>
<td>Difference Sq. on Expected</td>
<td>0.605</td>
<td>0.007</td>
<td>0.0006</td>
<td>0.484</td>
</tr>
<tr>
<td>Chi-square statistic X(^2)</td>
<td>1.097</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

- No. winners born in December: 24
- No. winners born in January: 15

Note: \(^1\)Winners of the Amanda Prize (actors), Spellemannsprisen (musicians), Brage Prize (writers), Gullpennen (journalists), Riksmålsforbundets litteraturpris (writers).

**d. Business**

Table 8 tabulates frequencies for leaders of the 500 leading Norwegian firms. A Pearson’s chi-square statistic of 14.66 rejects the null of a multinomial distribution given by population frequencies. We do observe the high number of leaders born in the second half of the year. While 36 leaders were born in January, 44 were born in December. This evidence is consistent with a reversed birth date effect, but let us not over-emphasize the finding. One could speculate, however, whether January-born individuals are groomed to acquire skills that allow early detection and early selection, in fields such as athletics for instance; their younger friends may then hone other talents, including social skills and leadership.
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Table 8. Leaders of Norway’s Top 500 Firms.¹ By Quarter of Birth. 2010. Norway

<table>
<thead>
<tr>
<th>Quarters</th>
<th>Q1</th>
<th>Q2</th>
<th>Q3</th>
<th>Q4</th>
</tr>
</thead>
<tbody>
<tr>
<td>Observed number</td>
<td>96</td>
<td>116</td>
<td>143</td>
<td>132</td>
</tr>
<tr>
<td>Expected number, uniform distribution</td>
<td>121.75</td>
<td>121.75</td>
<td>121.75</td>
<td>121.75</td>
</tr>
<tr>
<td>Population Frequency¹</td>
<td>0.2524</td>
<td>0.2673</td>
<td>0.2491</td>
<td>0.2312</td>
</tr>
<tr>
<td>Expected number, multinomial distribution</td>
<td>122.92</td>
<td>130.18</td>
<td>121.13</td>
<td>112.59</td>
</tr>
<tr>
<td>Difference Sq. on Expected</td>
<td>5.90</td>
<td>1.54</td>
<td>3.88</td>
<td>3.34</td>
</tr>
<tr>
<td>Chi-square statistic X²</td>
<td>14.66</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

No. leaders born in December | 44
No. leaders born in January | 36

Note: ¹A list was generated using a search algorithm online. 13 dates were missing among the top 500 firms.

5. Discussion

In empirical work, the issue of macro-to-micro inferences arises. We cannot, without maintained assumptions, infer from data on aggregates of individuals to individuals. In essence, the relative age hypothesis implies that if the conception of an agent born in December had been one month later, the individual would have had the same genetic make-up and family environment, but experienced different challenges in school and relative ages of classmates. The relative age effect implies that being born one month later would have been visible in life outcomes. Such a counter-factual can never be tested directly because data on a counter-factual do not exist and data from controlled experiments where only one factor is changed cannot be constructed outside a laboratory. However, given a maintained hypothesis of a certain level of homogeneity among economic agents, we are in a position to infer from
aggregate data to effects on individuals. If, contrary to what is suggested by Buckles and Hungerman (2008) for the United States, there are no systematic differences between March and April conceptions in Norway, the biological basis for births in December and January will be identical and we would expect no systematic differences in inherent talents. This invites the interpretation of this ex post data set as an outcome of a “natural” randomized controlled trial (RCT).

Why? The relative age hypothesis implies that being born in January involves a certain treatment and not being born in January (but in December) involves no treatment. Thus, running a double-blind randomized controlled trial (RCT) would involve assigning the January treatment to some agents in an experimental group and no treatment to agents in a control group (and potentially a placebo in a third group). Agents and observers would not know which agents received the treatment and which did not. If the randomization was successful, we could isolate and measure the treatment effect by evaluating how the experimental group performed compared to the control group. In fact, if the month of conception is random, our data mimics that part of an RCT. Conversely, if the month of conception is not random, our data cannot be interpreted as an RCT. Again, Buckles and Hungerman find in the United States that women who become mothers in certain seasons systematically vary from those in other seasons and if that result is valid for Norwegian mothers as well, there exists a potential for bias in the data.

However, any such bias would contribute to the existence of an effect, not the absence of one. Since we find no effect, or a negligible one, the only possible explanation for its non-existence would be that the selection effect of mothers had the opposite sign from, but exactly the same absolute value as, the relative age effect. If so, mothers capable of counteracting the
relative age effect would therefore tend to become pregnant in March and give birth in December, and mothers incapable of counteracting the relative age effect would tend to become pregnant in April and give birth in January. Thus, the pregnancy selection mechanism would nullify the relative age effect. Not only would the bias here constitute the opposite of what Buckles and Hungerman found, it would also be highly counter-intuitive. Why would capable mothers, i.e. ones with the ability to alleviate a disadvantage to their children, deliberately choose a conception date that would disadvantage their children – even if they could counter-balance this disadvantage? Why would women who control conception and are aware of possible disadvantages of December-births not wait one month? After all, Buckles and Hungerman’s detection is that, if there is a relative age effect, it is partly because capable mothers do the opposite, namely choose advantageous dates for births; in Norway, April conceptions. In a nutshell then, given the finding of no relative age effect it would not be entirely implausible to, this article suggests, interpret the outcome as stemming from a natural RCT.

However, our RCT is not a *double-blind* RCT. Both participants in the experiment, i.e. the children, and the observers of the experiment, i.e. parents, teachers, and coaches, know which child received the January treatment and which child did not. It is possible that this knowledge could lead to a desire among observers to counter the relative age effect by treating December-born children in especially advantageous ways. If so, parents, teachers, and coaches, in addition to the children themselves, would take into account the relative age in their interpretation of the child’s performance. However, if true, such a counter-effect would not be consistent with our findings of highly asymmetrical distributions of participants in sports, where the relative age effect appears to be at work, since recruitment to elite soccer seems to involve a disproportionately high frequency of January-born players. That being so,
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an observer-led treatment effect would counter-balance the relative age effect. We cannot informatively evaluate the magnitude of such an effect, but it would probably be small, given how much of the literature on the relative age effect results from teachers and coaches interpreting relative age-induced lead in skills as an inherent talent-induced lead in skills.

Another issue in empirical work is whether to use cross-sectional analysis, longitudinal studies, or even panels for observing and measuring. Observing different cohorts at given points in time is different from observing given cohorts at different points in time. Potentially, a January-cohort of vintage V could have followed a different income trajectory over time than many January-cohorts of different vintages observed at one time, i.e. as we do in the year 2008. Put differently, a life profile of yearly incomes for one individual at different stages of his life is not the same as the profile of yearly incomes for many individuals at different stages of their lives. In this article, we use the latter to infer something about the former. Essentially, we say that since the profile of incomes for December cohorts is sufficiently similar to the profile of incomes for January cohorts, when we examine cohorts born between 1942 and 1991, it appears unlikely that, for a given year of birth, the income life profiles of individuals born in December would be different from those born in January.

But they could be. For example, if individuals born in January 1960 made much more money over their life cycle than individuals born in December 1960, except for the year 2008 when we observe them, our inference would be flawed and we would be wrong about the 1960 vintage. Similarly, we could be wrong about several vintages. However, we are unlikely to be wrong about all 50 vintages at the same time, since it would require incomes for all vintages in the year 2008 to be an aberration and in the same direction. If we would be wrong in different directions we would observe much more scattered data points when we graphed
vintages against average income. The smoothness of the life income profiles indicates consistency.

Nevertheless, we can detect some differences. The estimated profile of incomes for the 50 January and December vintages did differ somewhat. Our estimated income curve implies that December-born reach a given income threshold at a younger age than January-born. In Table 3, we did observe a positive sign on the within-year relative age coefficient. This article thinks these two findings are due to the fact that taxpayers who are born late in the year in effect are the youngest members of a class of a given year. This effect could change over time because the average age of starting a career also changes over time, and the year-class effect versus the month of birth effect would change relative magnitudes over time. If so, the vintages of the 1940s and 1950s could be found to have different January-vs.-December income ratios when they were in their 20s in the 1960s and 1970s than the vintages of the 1970s and 1980s when they were in their 20s in the 1990s and 2000s. With only one year of data, we cannot test this or other possibilities, and must leave it to future research to settle the issue. Of course, data covering 50 years of 3 million observations per year are preferable to data from one year with 3 million observations. I do, however, appeal to some level of pragmatism since the dispute over the strength of the relative age effect is essentially about lack of full-count register data across all ages. Thus, one year of such hard-to-find data does move us part of the way forward, even if the conclusive answers require more data.

More importantly, there is a source of noise in non-compliance with cutoff dates. Some children are held back one year and some children start one year early. According to Solli (2011), as many as 1.54 percent of a male vintage (1970) enroll early and 4.79 percent (in 1969) enroll late, and non-compliance is definitely more likely among December and January
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cohorts. According to Black, Devereux, and Salvanes (2011), while the compliance rates are 90 and 85 percent for January and December, they are 97 and 96 for March and October. If the non-compliance phenomenon were constant over time, it would not necessarily disallow comparisons between vintages, even if it would disturb the estimated life profile difference between January cohorts and December cohorts. Solli, however, reports a slight change in the effect over time. According to her work, the frequency of non-compliance for early-starting boys was as high as 1.53 percent in 1969 and as low as 0.09 percent in 1991. For late starters, the numbers were 4.79 percent and 1.55 percent, respectively. Moreover, another source of disturbance is the non-uniformity across gender of non-compliance with enrollment regulations, with females tending to be early starters, rather than late starters.

Non-compliance could potentially mask or diminish the relative age effect. If many December-born pupils are held back and many January-born pupils start early, this would eliminate some of the relative age effect in our data; it would not mean the effect did not exist, however, only that it was overcome and counteracted by other measures. Since Solli finds the December and January cohorts involved in most cases of non-compliance one possible way to control for this confounder is running a regression with within-year relative age differences on the whole data set, i.e. the regression from which I presented results in Table 3.

However, there does exist a more direct way that also demonstrate the differences in an transparent way. Since non-compliance is much less prevalent for October-born and March-born pupils, a non-compliance-caused reduction of the relative age effect would have to be weaker when comparing October-born taxpayers with March-born taxpayers than when comparing December-born taxpayers with January-born. This comparison would control for most of the non-compliance effect, and still have some relative age effect at work since
March-born pupils are 7 months older than October-born pupils. Granted, a 7-month difference is less than an 11-month difference, but if the relative age effect was present and a potential non-compliance effect absent, we should be able to capture the former. In fact, if non-compliance presence contribute towards weakening of the relative age effect when the difference is 11-months and absence of non-compliance does not weaken a relative age effect when the difference is 7-months, the two relative age effects can potentially come out with the same magnitude, or even with a larger magnitude in the case of a 7-month difference.

In Table 9 I thus replicate Table 2 regressions on March and October cohorts. We observe results completely consistent with regressions on January and December cohorts. In fact, the differences detected in estimated parameters between January and December income profiles are almost exactly proportionately smaller between March and October compared to January and October. A 7-month difference is 36 percent smaller than the original 11-month difference. And the March–October difference in the intercept estimate is 39 percent smaller than the January–December difference in the intercept. Thus, the shift in profiles when cohorts are 7 months apart is smaller than the shift in profiles when cohorts are 11 months apart by almost exactly the expected amount, indicating little sensitivity to a systematic non-compliance disturbance.

<table>
<thead>
<tr>
<th>Variable</th>
<th>March-born</th>
<th>October-born</th>
<th>Both months</th>
</tr>
</thead>
<tbody>
<tr>
<td>Intercept</td>
<td>45,249 (9.5)</td>
<td>53,522 (9.8)</td>
<td>49,632 (13.7)</td>
</tr>
<tr>
<td>Age</td>
<td>27,793 (64.0)</td>
<td>27,110 (53.3)</td>
<td>27,430 (82.0)</td>
</tr>
<tr>
<td>Age squared</td>
<td>-444.0 (-53.5)</td>
<td>-435.6 (-43.8)</td>
<td>-439.7 (-68.0)</td>
</tr>
<tr>
<td>No. of Taxpayers</td>
<td>288,777</td>
<td>250,501</td>
<td>539,278</td>
</tr>
<tr>
<td>Residual sum of squares</td>
<td>5626101689</td>
<td>8065115271</td>
<td>14083298957</td>
</tr>
</tbody>
</table>

Note: Normalized age is defined as real age above 16 years, so a person who is born in December 1991 is 0.08 years in January 2008.

Another theoretical possibility is that, in general, this paper’s finding of no relative age effect could be due to two opposing effects that neutralize each other. For example, it is fathomable even if highly implausible that the income of working tax-payers in Norway is higher for a monthly cohort at the same time as the income of non-workers in the same monthly cohort is lower by the same amount. If so, averages could even out and mask two opposing effects. If, say, the average income of working January cohorts is higher than the average income of working December cohorts while the average income of non-working January cohorts is lower than the non-working December cohorts, and the differences in averages are equal in absolute terms but with different signs, the outcome would be a finding of no effect even a more nuanced finding is two opposing effects that neutralize each other. Moreover, it is also possible along similar lines that the average income of a monthly cohort is higher for men and
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lower for women in a way that the two averages exactly cancel each other. If so, there would be two effects, not zero.

In this article’s view, the former possibility is much less likely than the second. That January cohorts would have higher frequencies of non-workers than December-cohorts, in a manner such that the difference in their average income matches, with opposite sign, the difference between the averages of workers, appears unlikely. It is counter to exactly what this article has detected and documented: that the relative age effect is small, limited in time, and limited to some vocations. Granted, it is somewhat more likely that the relative age effect affects males in a different way than females. But that these two effects exactly cancel each other out and is reversed in another monthly cohort must be highly unlikely. Hypothetically, there are numerous ways an average of a monthly cohort could contain high numbers for some members of the cohort and low numbers for other members in a way that is exactly reversed in another monthly cohort. It must be left for future research to find out.

In fact, there are many other (and more pressing) questions left for future research. In the literature, the overwhelming majority of work has been on the disadvantage of being relatively younger during childhood. There has been little work on the possible advantage of being relatively younger during childhood, even if Black, Devereux, and Salvanes (2011) find some intriguing evidence – they find a small positive effect of starting school younger on IQ scores measured at age 18 – for saying that December-born children may benefit from being exposed to more mature class-mates. In our data, we do find that business leaders are more likely to be born late in the year. And there is a higher frequency of December-born prize-winning writers and artists than December-born individuals in the population at large. To the best of our knowledge, no research has been done on this possibility and it may be a topic
worth pursuing. For this article, however, it suffices to say that whatever the relative age effect on certain professions and vocations, in aggregate the relative age effect does not seem to noticeably affect the whole population across vintages.

What about the advantages of being relatively younger in older age, the other side of a relative age effect? In our data, December cohorts in vintages born before 1950 tend to have higher earnings that do January cohorts. This is noteworthy, and may perhaps be interpreted by straightforward and intuitive augmentation of the model presented by equations (1) – (5). For many activities the age advantage in younger years reverses in older years. An 11-year-old has an age advantage over a 10.08-year old since he is older, stronger, and faster, but a 60.08-year old has an age advantage over a 61-year old since he is (somewhat) younger, stronger, and faster. Thus, if we wanted rigorously to examine the hypothesis of a reversed age effect for older people, we could look at a different performance function, similar to the one given as an example in equation (9), for people past their physical and mental prime, i.e. over 50 years of age:

\[ P'_i = f(\xi'_i, A'_i, \Omega \eta i) = \beta + \gamma \xi'_i - \eta (A'_i - \Omega) + \delta TR'_i + \varepsilon, \]

where the middle term indicates that age is compared to a \( \Omega \)-year-old, after which performance starts to weaken. However, this is not the purpose of this article. This article has asked and answered what imprint relative age leaves on the income profile for taxpayers of different birth years across all ages.
6. Concluding Remarks and Policy Implications

The birth date, or relative age, effect is a well-established phenomenon in the literature where it is held that relative age matters in some early-selection processes and early-identification mechanisms. It presumably works through mechanisms that involve election to subsequent training and attention and thus later performance. However, there is disagreement in the literature on the duration, strength, and scope of the effect. Does it last long? Does it matter for a great number of individuals? Does it affect many selection processes across the spectrum of activities?

Economists would expect a long-lasting, strong, and widespread effect to leave a clear and visible effect on life income. If durable, substantial, and prevalent, it would make income curves across years of birth for January-born taxpayers different from those of December-born taxpayers. This article shows that this is not the case. I use a full-count register of all tax-paying residents in Norway born between 1942 and 1991 and demonstrate that the difference in the income profiles of January cohorts and December cohorts is insignificantly small. Moreover, when including taxpayers born in all 12 months and including a variable that measures the time difference between birth month and January, the estimated coefficient is not statistically significant. This article concludes that whatever the effects on selection and early performance in some activities from the date of birth effect, it does not appear to affect life incomes for most people and most vocations.

Potentially, the absence of an imprint on pay from the birth date effect could result from the absence of the birth date effect in Norway. This is not the case. I show that in sports the effect is clearly visible. Among elite soccer players who performed at the highest level in the period 2000–2010, 105 were born in January and only 52 in December. The concomitant Pearson’s
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chi-square is extremely large and thus we can firmly reject the hypothesis that the distribution of birth dates of top soccer players is identical to the general population’s. For business leaders, the effect appears reversed. Intriguingly, business leaders are found to be over-sampled from individuals born late in the year. For politicians, there is no effect. For artists, the results are mixed. There is no detectable effect among members of the national association of writers, but we do find a reversed effect among winners of prestigious awards for actors, writers, journalists, and musicians.

Establishing the duration, strength, and scope of the relative age effect may be important for policy. If it is long-lasting, strong, and widespread policy-makers may want to attempt to offset its impact. If it does not last long, is weak, or does not affect many vocations policy-makers may not feel compelled to implement counter policies. One such policy could be to sample school-starting pupils born in the first half of the year in one class and pupils born in the second half of the year in another class. Another policy would be to arrange leagues, competitions, and selection-processes by half-years instead of years. Potentially, one could even try bi-annual school-starts. However, this article finds that the birth date effect does not seem to last very long, is very strong, and affects many activities. Instead, it seems short lived and restricted to certain activities. Indeed, in some activities it may be reversed. This paper cannot therefore justify a call for changes in policy, except potentially for the recruitment of sport talents.

References


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