Return to (non) schooling:

The potential impact of school holiday reform

Fu Zixuan

EMPV research proposal

December 8, 2024

Abstract

This research proposal applies the marginal treatment effect (MTE) framework to evaluate school holiday reforms. Two commonly discussed reform directions are analyzed: (1) modifying the school calendar by shortening holidays to compensate for reduced school hours during weekdays, and (2) introducing targeted programs to ensure that underserved students benefit from holiday opportunities. Applying the treatment effect literature, this study wants to compare the effectiveness of these two policies against the status quo. The proposal concludes with a discussion on potential data sources and policy implications.

Contents

1 Introduction

The return to schooling has been studied extensively in economics, representing a rich strand of literature that combines the empirical results and the development of novel econometric methods. Well-known examples include Angrist and Krueger (1991); Imbens and Angrist (1994); Card (2001). These studies have not only influenced education policies but have also advanced the methodological toolkits.

Much attention has been devoted to the returns to schooling, yet in the meantime the question of non-schooling—school holidays—is almost as old as that of schooling itself. The appropriate length and structure of school holidays have long been a subject of debate among policymakers, educators, and researchers given its different implications for educational outcome and social equality. For example, summer learning loss has been widely documented in the education literature (Cooper et al., 1996). The extent of the loss varies across subjects, grade levels, and socio-economic groups. Research consistently shows that students from low-income households are disproportionately affected (Morgan et al., 2019), as they often lack access to resources and opportunities that sustain or enhance learning during the summer, such as summer camps, tutoring programs, or educational travel. As a result, the academic achievement gap between socio-economic groups often widens after the holiday, raising equity concerns among policymakers. Additionally, long holidays can impose significant burdens on working parents, who may struggle to balance childcare with work obligations. On the other hand, school holidays are cherished for the opportunities they provide for rest and leisure, fostering well-being and creativity among students. Indeed, the cultural and social significance of holidays cannot be overlooked—after all, who doesn't enjoy a break?

In light of these competing perspectives, one potential approach is to rearrange the school calendar by shortening the summer break and compensating for it with a shorter daily in-class schedule throughout the academic year. In fact, French President Emmanuel Macron made a classical comparison between France and Germany in terms of school calendar. He argues for a reform that follows the German system with shorter holiday and longer daily afternoon off-school activities. A similar reform in the US called year-round schooling is studied by McMullen and Rouse (2012). Others believe that addressing the education and equality issue requires ensuring all children, particularly those from low-income families, have access to meaningful vacation experiences, rather than simply shortening breaks. This is the objective of the "Vacances apprenantes" program, which was initially introduced as a remedy for learning losses caused by the pandemic and has since been maintained as an ongoing initiative.

The proposal aims to evaluate two policies using different methods. The first policy requires minimal attention to issues associated with treatment effect estimation due to its mandatory nature. However, the second method applies the marginal treatment effect (MTE) framework proposed by Bjorklund and Moffitt (1987); Heckman and Vytlacil (2005) which is designed to address issues of selection bias and heterogeneous treatment effects. To identify the MTE, I consider two potential instrumental variables, one continuous (distance) and one discrete (cohort enrollment). Since this is an empirical exercise, I discuss the availability of data and the potential directions for data collection.[1] Finally, the proposal concludes with a brief discussion of the policy implications.

2 Methodology

To evaluate the effects of the policies, I assume that students take standardized tests at two points during the academic year: at the beginning of the year, with performance denoted as Y^s, and at the end of the academic year (or the start of the summer holiday), with performance denoted as Y^e.

The performance measures considered in this evaluation include: (1) academic scores, and (2) physical and mental health examinations. One hypothesis is that the policy changes may introduce trade-offs between these dimensions. Therefore, I take both into considerations.

The two policies differ in several aspects, but the primary distinction in treatment effect estimation lies in the take-up of the new policy. The first policy, focused on school calendar reform, is universally applied, whereas the second policy targets economically disadvantaged children by providing subsidies or opportunities without requiring mandatory enrollment.

2.1 Policy 1: school calendar reform

Treatment and outcome The treatment in this context is the length of the holiday, denoted by D. Since the number of days in a year is fixed, any reduction in holiday length will result in a proportional increase in school days. Thus, a specific holiday length $D = d$ corresponds to a particular school calendar. The potential outcomes under different school calendars can be expressed as $Y^s(D)$ and $Y^e(D)$, representing student performance at the start and end of the academic year, respectively.

While the reform may involve an earlier return to school rather than a later start to the holiday, without loss of generality, we can adjust the academic year's span to make these scenarios equivalent. See figure 1 for an illustration.

Previously, I only considered the difference in outcomes before and after the holiday, expressed as $Y_2^s(D) - Y_1^e(D)$, and compared this difference under varying holiday lengths. However, this approach provides a narrow look on the policy reform, as it focuses solely on the holiday period while ignoring its impact on the regular academic semesters. Naturally, changes to the length of the holiday will also influence learning during the longer school term, e.g., the daily in-school time may be shorter. Therefore, the outcome of interest should be evaluated from a more holistic perspective, encompassing the entire academic year, that is $\Delta Y(D) = Y_2^s(D) - Y_1^s(D)$.

Parameter The parameter of interest is the Conditional Average Treatment Effect (ATE),

$$\text{ATE}(X) = \mathbb{E}\left[\Delta Y(d') - \Delta Y(d)|X\right]$$

[1] However, one should not be overly optimistic in this regard.

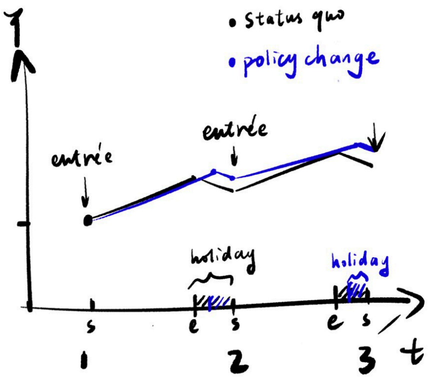

Figure 1: Illustration of status quo and policy change

First, the conditional independence is more likely to hold if we condition on the observed characteristics X (1) socio-economic status and (2) start-of-year performance Y_1^s or the average performance in the previous years. Second, it is of empirical interests to condition on X to examine the differential impact of the policy on various population groups. This allows for an analysis of potential equity concerns. Since this policy is mandatory across all French schools, issues related to selection into treatment can be safely ignored.

2.2 Policy 2: learning holiday programs

Treatment, outcome and selection I simplify the change introduced by the second policy as a binary enrollment treatment, denoted by D. Under this policy, the school calendar remains unchanged, meaning the only source of variation arises from participation in the new programs. However, for comparability with the first policy, we still define the potential outcome variable as $\Delta Y(D) = Y_2^s(D) - Y_1^s(D)$.

The learning holiday program[2] targets a subpopulation, primarily students in precarious socio-economic situations or from rural areas. As is common with such targeted programs, there is a self-selection issue that can either attenuate or amplify the estimated treatment effect. To address this, I rely on the marginal treatment effect (MTE) framework (Heckman and Vytlacil, 2005) to account for selection bias.

The potential outcome of attending the program is modeled as:

$$\Delta Y(1) = \mu_1(X) + U_1$$

where X represents observed covariates, including (1) socio-economic characteristics such as household income and (2) baseline performance Y_1^s, or the average score from previous years. U_1 represents unobserved factors affecting the outcome.

The choice to participate in the program is determined by the following equation:

$$D = \mathbb{1}\left\{\mu_D(X, Z) - U_D \geq 0\right\}$$

[2] For further details, consult `https://www.education.gouv.fr/les-vacances-apprenantes-303834`.

where X is the same set of covariates as in the potential outcome equation, and Z is an additional instrumental variable that is crucial for identifying the marginal treatment effect. Two natural candidates for Z are (1) The distance between the student's home and the program location. (2) The number of participants (take-up rate) from the student's class (peer effect).

Treatment effect The parameter of interest is again the conditional average treatment effect (ATE). To estimate the ATE, the marginal treatment effect (MTE) is first estimated, and the ATE is then obtained as a weighted (weight $h = 1$ for the ATE) sum of the MTE. Given the importance of MTE, I review the local instrumental variable (LIV) approach in estimating the MTE $\Delta^{\text{MTE}}(x, u)$.

Recall the choice equation $D = \mathbb{1}\{\mu(X, Z) \geq U_D\}$, which is equivalent to

$$D = \mathbb{1}\{F_{U_D}(\mu(X, Z)) \geq F_{U_D}(U_D)\} = \mathbb{1}\left\{\tilde{\mu}(X, Z) \geq \tilde{U}_D\right\}$$

with $\tilde{U}_D \sim U[0, 1]$. Therefore, it is without loss of generality to assume $U_D \sim U[0, 1]$. Thus

$$\mu(X, Z) = \mathbb{P}(D = 1|X, Z) \tag{1}$$

The essence of LIV is this equality. Since we do not directly observe the U_D, we can not directly get $\mathbb{E}[Y|X = x, U_D = u]$. However, we can directly observe $\mathbb{P}(D = 1|X = x, Z = z)$ and therefore $\mathbb{E}[Y|X = x, P(D = 1|X = x, Z = z) = p]$. Heckman and Vytlacil (2005) shows that

$$\mathbb{E}[Y|X = x, U_D = p] = \frac{\partial \mathbb{E}[Y|X = x, P(D = 1|X = x, Z = z) = p]}{\partial p}$$

In order to get the standard treatment effect parameter ATE(x), we need

- $P(D = 1|X = x, Z)$ has the full support on $(0, 1)$.
- $\mathbb{P}(D = 1|X = x, Z)$ is continuous so as to take the derivative.
- a polynomial MTE model if the local instrument is discrete (Brinch et al., 2017).

The first candidate Z_1–distance–gives a continuous local instrument $\mathbb{P}(D = 1|X = x, Z)$. I intend to use a nonparametric approach to directly estimate the derivative function since I do not include too many covariates.

The second candidate Z_2–the number of classmates enrolled–is an instrument that deserves a closer examination. Recall some of the assumptions maintained in the analysis are:

1. $\mu_D(Z)$ is a nondegenerate random variable conditional on X.
2. (U_1, U_D) and (U_0, U_D) are independent of Z conditional on X.
3. $1 > P(D = 1|Z) > 0$.

First, it is reasonable to assume that other's enrollment affect my decision and assumption 1 and 3 are likely to hold. Second, even though my decision $D = \mathbb{1}\{\mu_D(X, Z) - U_D \geq 0\}$ affects the number of participants Z_2 by peer effect, it should be negligible since the class size is not too small. In this case, I may assume that assumption 2 holds. Lastly, since if Z_2 takes on N values, a polynomial of order $L \leq N$ MTE model is to be specified and identified as in Brinch et al. (2017).

3 Discussion

3.1 Data

One potential source is the OECDs Programme for International Student Assessment (PISA), a project that evaluates the education systems of countries around the world by testing the skills and knowledge of 15-year-old students. PISA provides information on student performance in mathematics, reading, and science, along with data on socio-economic background and school characteristics.

Ideally, it would be best to collect not only academic data but also physical and mental health data of students at both the beginning and the end of the academic year. For the learning holiday initiative, the current information is limited to anecdotal evidence from news reports and participant feedback since its launch in 2020. As the program is relatively new, more data needs to be collected to evaluate its quantitative impact comprehensively.

In addition to the performance outcomes Y and student characteristics X mentioned earlier, other relevant information, such as the distance between the student's home and the program location, as well as the number of participants from the same class, is essential for dealing with selection on unobservables. However, the data collection plan described above represents an ideal scenario that may not be fully attainable in practice.[3]

3.2 Pilot study

Since different countries and regions operate under varying school calendars, there are natural control and treatment groups available for analysis. However, this is not exactly what I originally intended. The PISA surveys test different cohorts of students triennially, rather than following the same cohort over time. At a pilot or exploratory stage, it may be of interest to adopt the following standard approaches:

- Regard students from different countries as directly comparable when conditioned on a set of individual and school characteristics. The treatment effect of holiday length can be estimated by regressing performance on holiday length and other covariates.
- If there is a policy change in some countries but not in others, a difference-in-differences (DiD) heterogeneous two-way fixed effects model De Chaisemartin and d'Haultfoeuille (2023) can be employed to estimate the effect of holiday length, d.

[3]Sometimes in a research proposal, one can dream big and bold.

References

Angrist, J. D. and Krueger, A. B. (1991). Does compulsory school attendance affect schooling and earnings? *The Quarterly Journal of Economics*, 106(4):979–1014.

Bjorklund, A. and Moffitt, R. (1987). The estimation of wage gains and welfare gains in self-selection models. *The Review of Economics and Statistics*, pages 42–49.

Brinch, C. N., Mogstad, M., and Wiswall, M. (2017). Beyond late with a discrete instrument. *Journal of Political Economy*, 125(4):985–1039.

Card, D. (2001). Estimating the return to schooling: Progress on some persistent econometric problems. *Econometrica*, 69(5):1127–1160.

Cooper, H., Nye, B., Charlton, K., Lindsay, J., and Greathouse, S. (1996). The effects of summer vacation on achievement test scores: A narrative and meta-analytic review. *Review of educational research*, 66(3):227–268.

De Chaisemartin, C. and d'Haultfoeuille, X. (2023). Two-way fixed effects and differences-in-differences with heterogeneous treatment effects: A survey. *The Econometrics Journal*, 26(3):C1–C30.

Heckman, J. J. and Vytlacil, E. (2005). Structural equations, treatment effects, and econometric policy evaluation 1. *Econometrica*, 73(3):669–738.

Imbens, G. and Angrist, J. (1994). Identification and estimation of local average treatment effects. *Econometrica*, 62(2):467–475.

McMullen, S. C. and Rouse, K. E. (2012). The impact of year-round schooling on academic achievement: Evidence from mandatory school calendar conversions. *American Economic Journal: Economic Policy*, 4(4):230–252.

Morgan, K., Melendez-Torres, G., Bond, A., Hawkins, J., Hewitt, G., Murphy, S., and Moore, G. (2019). Socio-economic inequalities in adolescent summer holiday experiences, and mental wellbeing on return to school: analysis of the school health research network/health behaviour in school-aged children survey in wales. *International journal of environmental research and public health*, 16(7):1107.