Using Statistics Canada's Microdata to Evaluate Policy Impacts: The AgriInnovation Stream C - Commercialization Program

March 28, 2024

Motivation

The AgriInnovation-Stream C and AgriInnovate Programs

AgriInnovation-Stream C (2013-14 to 2017-18) and AgriInnovate (2018-19 to 2022-23) aimed to accelerate the commercialization phase of the innovation process in the sector using interest-free, repayable contributions for eligible innovation projects.

Economic theory

Government innovation subsidies incentivize firms to invest more in such activities (Afcha and Lopez, 2014), thereby improving the performance of the individual firms as well as the broader economy (Chudnovsky et al., 2006; Masso and Vahter, 2008; Hall et al., 2009; Cin et al., 2017).

Research question:

Are AAFC's AgriInnovation Stream C and AgriInnovate Programs effective in improving the economic performance of the recipient firms?

Data: firm-level

Source: Business-Linkable File Environment (B-LFE) and Diversity and Skills Database (DSD)

Time frame: 2005-2020, some of the main series end in 2017.

- Financial information (income tax data): total revenues, operational expenses;
- Employment information (payroll deduction accounts): number of employees, salaries and wages;
- Diversity information: share of immigrant or female employees in workforce, average age of employees;
- Other variables: Location, research and development and gender of owner.

Table 1. Program participants, AgriInnovation-Stream C and AgriInnovate programs, 2013-2020

Program	AgriInnovation-Stream C					AgriInnovate			Total
Year	2013	2014	2015	2016	2017	2018	2019	2020	
Number of participants	9	10	9	12	5	6	8	5	**64**

Table 2. Number of observations and firms in the dataset, 2005-2020

	Participants		Non-participants	
	Firms	Obs.	Firms	Obs.
All (in scope)	64	809	106,147	945,383
Used in main regressions	39	377	676	1,160

Note: Some series end in 2017. As such, the 19 firms that participated in the program after 2017 could not be used in the main regressions. Similarly, good matches could not be found for six firms, leaving 39 in-scope firms for the main model.
Source: Statistics Canada's LFE and authors' estimations.

Methodology

Objectives:

- To investigate a causal link from participation in the program to net income; and
- To explore factors that affect program participation.

Approach:

Step 1: Build control group

Matching on observables: a participant is matched with non-participant(s) of similar traits from the same industry category, year, and exporter status (Annex 4).

Step 2: Estimate program impact

Apply two-way fixed effects (TWFEs) difference-in-difference (DID) regressions to the matched observations.

- DID refers to the difference of differences in the outcomes of participants and non-participants before and after the program (Annexes 2 and 5).

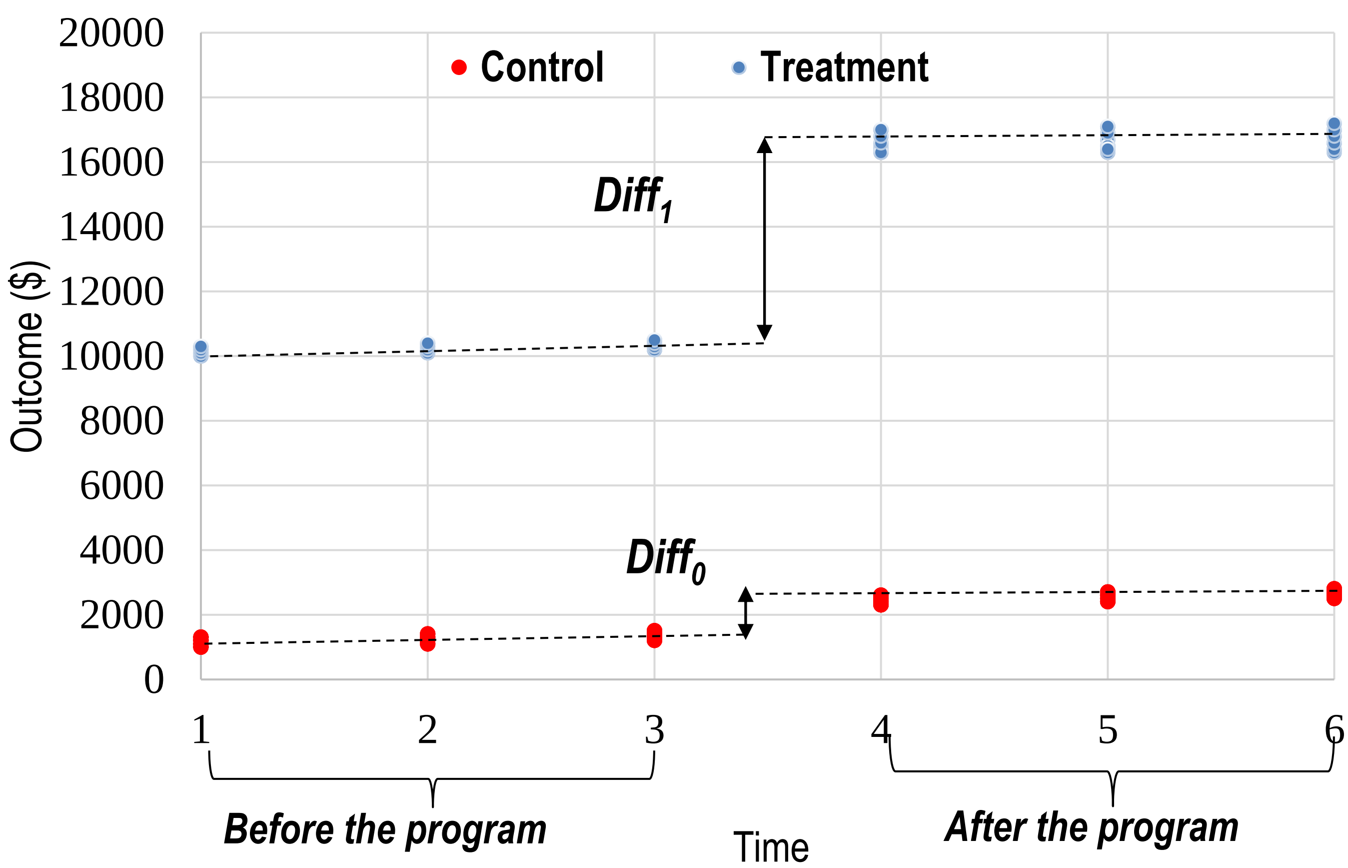
Advantages:

- Matching: Assurance of random selection – i.e., no false comparisons;
- TWFEs DID: Control for some firm-specific factors – e.g., managerial talent; and
- Together, matching and TWFEs DID mostly address self-selection bias.

Note: This approach was first introduced by Heckman et al. (1997) and Heckman et al. (1998)
Other applications: Arnold and Javorcik (2009), Gorg et al. (2008), and Volpe and Carballo (2008).

DID Models

Figure 1. Treatment effect in difference-in-difference models: a hypothetical case

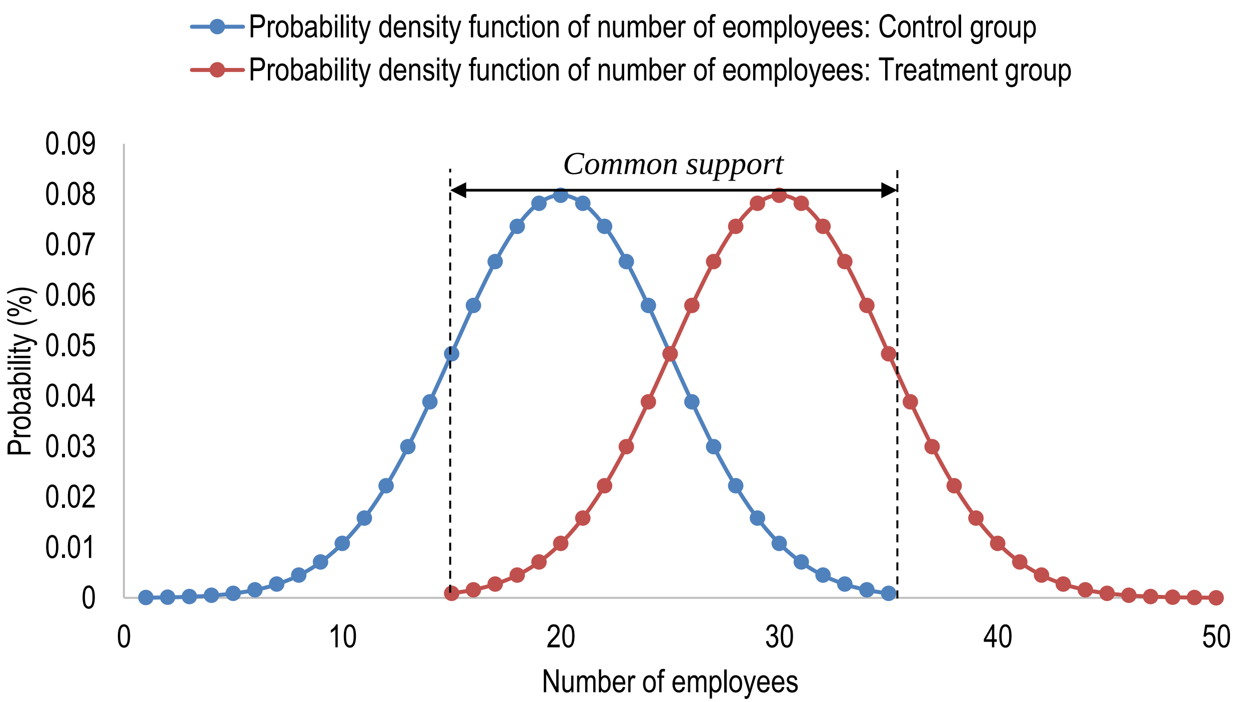


Treatment effect= $Diff_1$ - $Diff_0$

Common support

Common support: the overlapping area of two distributions.
Common support assumption: the treatment and the control group must have common support (i.e., be comparable) for *all* observable characteristics.

Figure 2. Example of common support for number of employees for hypothetical control and treatment groups



Propensity Score Matching

1. To match participants and non-participants, we first **estimate the propensity of participation** (using a logistic regression model) as a function of firms' observable characteristics:
    - *Share of immigrant employees* (%)
    - *Share of female employees* (%)
    - *Average age of employees* (years)
    - *Number of employees*
    - *Salaries and wages* ($)
    - *Total assets* ($)
    - *Year* (2005-2020)
    - *Province* (where the firm is headquartered)
    - *Industry category* (56 distinct categories based on their NAICS codes)
    - *Exporter status* (represents whether the firm is an exporter)
    - *Years since birthdate* (represents age of the business)
2. Using *Greedy matching* algorithm, **each participant is then matched with non-participants from the same industry category, calendar year, and exporter status** whose propensity score is within a pre-specified threshold (*radius caliper*) of that of the participant.

Note: matching within calendar year is necessary in staggered designs.

Panel DID regressions

The **DID** model is estimated as TWFEs panel of the following form:

(2) $Y_{it} = \alpha + \beta(\boldsymbol{Treatment_{it}}) + X_{it}\lambda + \gamma_i + v_{it},$

where Y_{it} is a measure of financial performance – e.g., net income – of firm *i* in time *t*, $\boldsymbol{Treatment_{it}}$ is a dummy variable that takes the value of 1 for participant firms after participation and 0 otherwise, and the set of other explanatory variables X_{it} is as follows:

- *Share of immigrant employees (%)*
- *Share of female employees (%)*
- *Salaries and wages ($)*
- *Total assets ($)*
- *Year* (2005-2020)
- *Year* × *Province*: interaction of calendar years and provinces
- *Year* × *Industry category*: interaction of calendar years and industry categories

Notes:

1. α is the constant term, β is the parameter that measures the effect of the program, λ is a set of parameters to be estimated, γ_i represents firm-specific (observable and unobservable) time-invariant characteristics or fixed effects, v_{it} reflects the remainder stochastic disturbances.
2. *Province* and *Industry category* are time-invariant for each individual firm and thus their effect is already captured in the firm-specific fixed effects – i.e., including them in the model would be redundant.

Self-selection bias (picking winners)

Problem: there may be something intrinsic about the participating firms or their managers that positively affects both their likelihood of participation and their outcome – e.g., opportunistic vs. less opportunistic managers.

Implication: The estimated program impact is confounded with the effect of the firms' or their managers' intrinsic abilities.

Solution: If source of bias is:

1. ***Observable characteristics*** (e.g., size, value of assets) → ***Matching***
2. ***Unobservable time-invariant*** (e.g., the manager's character) → ***FEs in regression models***

The remaining concern is:

3. ***Unobservable time-variant*** (e.g., temporal changes in an industry, province, or firm's conditions)
   a) At industry level→ ***Year*** × ***Industry*** *category* interaction in ***regression models***;
   b) At province level→ ***Year*** × ***Province*** interaction in ***regression models***;
   c) At firm level → A supplementary approach (Annex 6).

Note: Heckman et al. (1997) claim, "this estimator [matching DID] is effective in eliminating bias, especially when it is due to temporally-invariant omitted variables." As argued by Blundell and Costa Dias (2002) and cited in Görg et al. (2008), "a combination of matching and difference-in-differences analysis may be a particularly suitable approach" to address self-selection bias.

Summary Statistics

Table 3. Distribution of small, medium, and large firms in the sample, before matching, 2010-2019

	Participants		Non-participants	
Number of Employees	Obs.	Share (%)	Obs.	Share (%)
Small (1 to 99)	412	58.2	454,281	97.3
Medium (100 to 499)	229	32.3	11,223	2.4
Large (>500)	67	9.5	1,388	0.3
Total	708		466,892	

Table 4. Distribution of firms across industries, before matching, 2010-2019

	Participants		Non-participants	
Industry	Obs.	Share (%)	Obs.	Share (%)
Farming	201	19.3	689,911	71.4
Manufacturing	477	45.9	111,769	11.6
Wholesale	102	9.8	143,108	14.8
Other*	260	25.0	21,827	2.3
Total	1,040		966,615	

Notes: 1. “Other” includes smaller categories such as real estate, finance, insurance, supporting services, etc.; 2. Number of observations are different in the two tables. This is because they are obtained from different sources and thus their missing observations do not necessarily align with one another – e.g., while Table 3 is based on *Number of employees*, which originates from payroll deduction accounts and ends in 2017, Table 4 is based on North American Industry Classification System (NAICS) codes, which originate from income tax filings and end in 2020. *Source*: Statistics Canada’s LFE.

Summary Statistics: B-LFE Variables

Table 5. Summary statistics, financial variables, before matching

	Participants		**Non-participants**	
Variable	Mean	Obs.	Mean	Obs.
Total assets **(million $)**	144	809	3.2	945,383
Total revenue **(million $)**	184	809	3.2	945,383
Total expenses **(million $)**	175	809	3.0	945,383
Net income (loss) before tax **(million $)**	9.3	804	0.2	927,275
Gross profit (loss) **(million $)**	25.1	802	0.5	892,129

Note: These financial variables originate from the **General Index of Financial Information** (GIFI) database. *Source*: Statistics Canada's LFE.

Summary Statistics: DSD Variables

Table 5. Summary statistics, financial and socio-demographic variables, before matching

	Participants		Non-participants	
Variable	Mean	Obs.	Mean	Obs.
Share of immigrant employees in workforce **(%)**	20	594	12	463,879
Business share held by immigrants **(%)**	22	315	9	598,220
Share of female employees in workforce **(%)**	23	594	35	463,879
Business share held by women **(%)***	41	183	34	307,672
Average age of all paid employees **(years)**	38	594	42	463,552
Average age across all owners **(years)**	49	315	53	597,014
Share of single majority owners **(%)**	36	315	55	598,781

Note: Number of observations are different from the previous table. This is because they are obtained from different sources and as such their missing observations do not necessarily align with one another – e.g., the financial variables are obtained from the LFE and end in 2020, while the socio-demographic variables originate from the DSD and end in 2017. *Source*: Statistics Canada's DSD.

Estimated benefits

- **Average program impact ($)**: a **$33 million** increase in the total revenues and a **$28 million** increase in the total expenses, leading to a **$5 million** increase in the net income before tax of an average participant over the 2013-2017 period.

- **Aggregate benefit**: around **$200 million** increase in net income before tax of the 39 participants over the 2013-2017 period, or $40 million per year.

Table 6. Summary of main findings: average program impact, 2013-2017.

	Program impact on an average participant	
Financial outcome	**Average effect (million $)**	**Marginal effect ($) (effect of a dollar of funding)**
Total revenue	33	8.8
Total expenses	28	7.7
Net income (loss) before tax	5	1.1

Note: To calculate the aggregate benefit, we use the average effect ($5.12 million × 39=$199.9 million) rather than the marginal effect. This is because the latter is only valid for the interpretation of small changes.

Estimated costs

- **$137.3 million in interest-free loans** were made to the participants from 2013 to 2017.
- It takes an average participant **13 years to fully repay AAFC**:
  - 2 years completion time + 1 year repayment lag + 10 years repayment schedule = 13 years
- The **opportunity cost** of the $137.3 million interest-free loans is approximately **$50 million:**
  - Interest cost of $137.3 million @ (5% interest, 13 years repayment, monthly payments)= $50 million

Effectiveness: Public perspective

Scenario I (base case):

Over the 2013-2017 period,

- **Aggregate benefit**: around **$200 million** increase in net income before tax of the 39 participants.
- The **opportunity cost** of the $137.3 million interest-free loans is approximately **$50 million.**

Aggregate benefit ($200 million) > Opportunity cost ($50 million)

Caveat:

- Program contributions could not exceed 50 percent of eligible project costs. Since program contributions are approximately $137.3 million, the participants must have spent at least another $137.3 million, leading to a total cost of at least $274.6 million.
- While non-capital expenses are fully reflected in the participants' net income before tax, capital expenses may only be partially reflected because they cannot be claimed for tax purposes all at once.
- If there are unclaimed capital costs, then the $200 million estimated program benefit may be an overestimation.

Effectiveness: Public perspective

Scenario II (65% potentially unclaimed capital costs):

- Assume that 65% of the total eligible costs – i.e., $178.5 million – is spent on Class 6 capital items such as buildings, which have one of the lowest depreciation rates (10%).
- Even for this capital class, on average, firms could claim up to 22% of the costs as capital cost allowance (CCA) within five years. The remaining 78% potentially unclaimed costs amount to $139 million.

Aggregate benefit - Potentially unclaimed capital costs > Opportunity cost

($200 million- $139 million= $61 million> $50 million)

- Even under the most pessimistic assumptions, the program has been welfare-improving from a public perspective.

Notes:

- Depreciation rates for Class 8 (e.g., equipment without motors) and Class 10 (e.g., machinery with motors) are 20 and 30 percent, respectively. Class 12 items (e.g., kitchen utensils and computer software) depreciate at 100 percent.
- In Canada, for the first year, only half of the 10 percent Class 6 depreciation rate can be claimed as CCA. By the end of the second, third, fourth, and fifth years, firms are able to claim 15, 23, 31, and 38 percent of the cost for a Class 6 item, respectively.

Conclusions

- From a private perspective, the $50 million opportunity cost is irrelevant. Thus, on average, the program is more effective from a private perspective than from a public perspective.
- The benefits of the program to the economy could go beyond the ($200 million) benefits experienced by the recipients in the regression analysis:
  - The estimated benefit applies to the 39 recipients in the regression, not all the 45 recipients;
  - The projects could benefit consumers by offering higher-quality, low-cost, or a broader variety of products; and,
  - Other firms in the sector (downstream and upstream) could benefit from the program.
- After the 5-year study period, there could be more costs (e.g., interest costs) and benefits.
- Future research could re-examine the effectiveness of the programs with an updated dataset to:
  - Capture the benefits of the programs after the 5-year study period;
  - Include the AgriInnovate Program participants; and
  - investigate treatment effect heterogeneity, apply other research designs (e.g., event study), exploit the not-yet-treated.

Acknowledgements

We are grateful for the continuous support provided by Centre for Special Business Projects (CSBP), Statistics Canada, particularly Peter Timusk and Julio Rosa, and the Programs Branch, AAFC.

Thank you for listening.

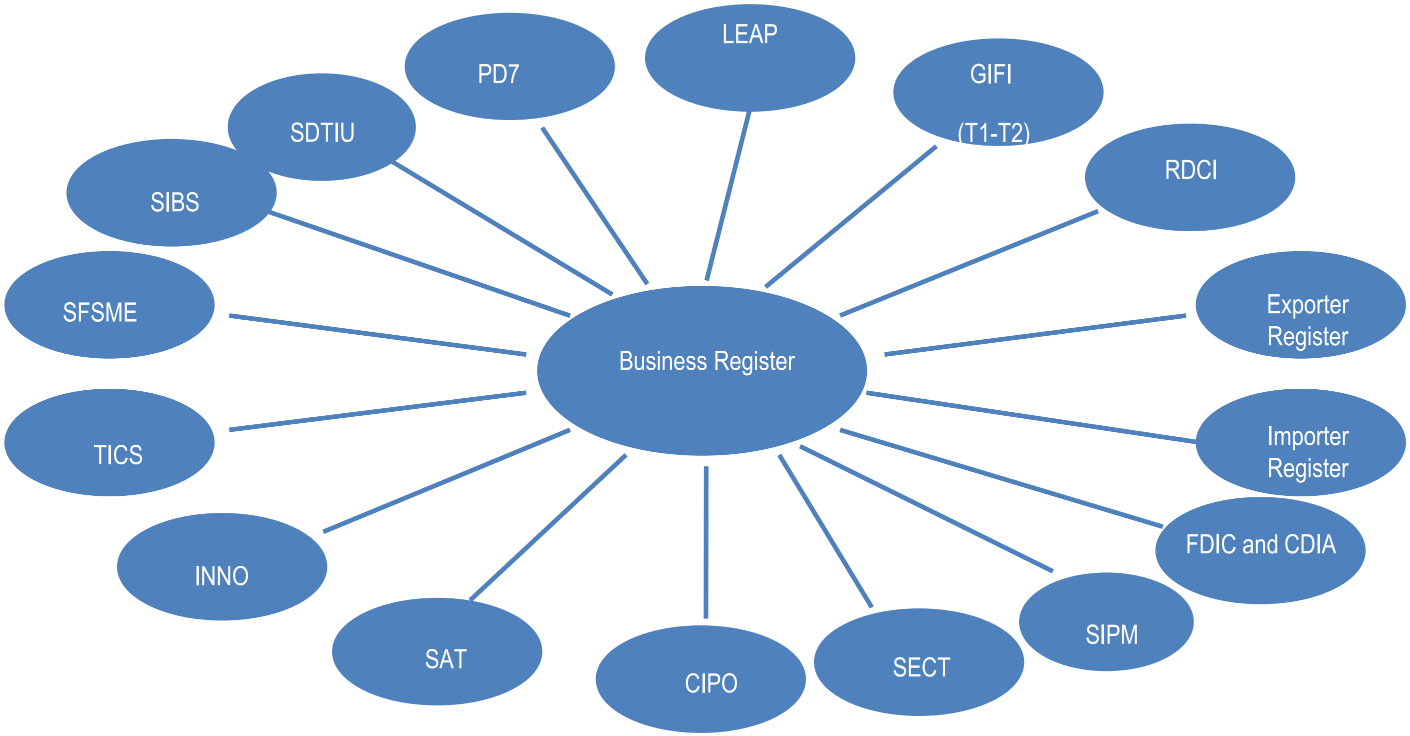
Questions or comments?

References

- Afcha, S. & Lopez, G. (2014). Public funding of R&D and its effect on the composition of business R&D expenditure. *BRQ Business Research Quarterly,* 17, 22-30.
- Arnold, J., & Javorcik, B. (2009). Gifted kids or pushy parents? Foreign direct investment and plant productivity in Indonesia. *Journal of International Economics*, 79(1), 42-53.
- Blundell, R., & Costa Dias, M. (2000). Evaluation Methods for Nonexperimental Data. Fiscal Studies, 21(4), 427-468.
- Cin, b., Kim, Y., & Vonortas, N. (2017). The impact of public R&D subsidy on small firm productivity: evidence from Korean SMEs, *Small Business Economics*, 48, 345-360.
- Chudnovsky, D., Lopez, A., & Pupato, G. (2006). Innovation and productivity in developing countries: A study of Argentine manufacturing firms behavior (19922001). *Research Policy*, 35: 266-288.
- Görg, H., Henry, M., & Strobl, E. (2008). Grant support and exporting activity. *Review of Economics and Statistics*, 90(1), 168-174.
- Hall, B., Lotti, F., & Mairesse, J. (2009). Innovation and productivity in SMEs: empirical evidence for Italy. *Small Business Economics*, 33: 13-33.
- Heckman, J., Ichimura, H., & Todd, P. (1997). Matching as an econometric evaluation estimator: evidence from evaluating a job training programme. *Review of Economic Studies*, 64 (4), 605-654.
- Heckman, J., Ichimura, H., Smith, J., & Todd, P. (1998). Characterizing selection bias using experimental data. *Econometrica*, 66(5), 1017-1098.
- Masso, J. & Vahter, P. (2008). Technological innovation and productivity in late-transition Estonia: econometric evidence from innovation surveys. *The European Journal of Development Research*, 20: 240-261.
- Rosenbaum, P., & Rubin, D. (1983). The central role of the propensity score in observational studies for causal effects. *Biometrika*, 70(1), 41-50.
- Volpe Martincus, C., & Carballo, J. (2008). Is export promotion effective in developing countries? Firm-level evidence on the intensive and extensive margins of exports. *Journal of International Economics*, 76(1), 89-106.

ANNEXES

Annex 1: Linkable File Environment Galaxy



List of acronyms:

RDCI – Research and Development in Canadian Industry
GIFI – CRA T1 and T2 Income statement and Balance sheet
LEAP – Longitudinal Employment Analysis Program
PD7 – Business Payrolls Survey (based on CRA payrolls deduction form PD7)
SDTIU – Survey of Digital Technology and Internet Use
SIBS – Survey of Innovation and Business Strategy
SFSME – Survey on Financing of Small- and Medium-sized Enterprises
TICS – Trade in Commercial Services
INNO – Survey of Innovation
SAT – Survey of Advanced Technology
CIPO – Canadian Intellectual Property Office
SECT – Survey of Electronic Commerce and Technology
SIPM - Survey of Intellectual Property Management
FDIC – Foreign Direct Investment in Canada
CDIA – Canadian Direct Investment Abroad

Annex 2: Self-selection bias (picking winners)

Slide 9 shows that MDID can mostly address the issues of self-selection bias as follows.

If source of bias is:

1. ***Observable characteristics*** (e.g., size, value of assets) → ***Matching***
2. ***Unobservable time-invariant*** (e.g., the manager's character) → ***FEs in regression models***
3. ***Unobservable time-variant*** (e.g., temporal changes in an industry, province, or firm's conditions)
    a) At industry level→ ***Year*** × ***Industry*** *category* interaction in ***regression models***;
    b) At province level→ ***Year*** × ***Province*** interaction in ***regression models***;

A concern remain, however, regarding **unobservable time-variant factors at firm level** →

Supplementary approach 1: Create a control group comprised of future *participants* in the years before they become participants (i.e., the not-yet-treated). In this fashion, both the treated and the untreated are from the pool of "winners." Thus, they are likely to be similar with respect to their intrinsic abilities. This approach exploits the time lag in participation to address the "good managers" versus "poor managers" problem or similar patterns resulting in self-selection bias.

Table A1. Using the not-yet-treated in the control group to overcome self-selection bias

Year	**Participating firms**	**Matching**
t-1 (the year before the program)	-	-
***t* (1st year of the program)**	**Firm *A***	**Treatment group: Firm *A* Control group: Firm *B*, Firm *C***
t+1 (2nd year of the program)	Firm *B*	Treatment group: Firm *B* Control group: Firm *C*
t+2 (3rd year of the program)	Firm *C*	-

Annex 2: Self-selection bias (picking winners)

Supplementary approach 2: In the previous studies, we used the not-yet-treated as effective controls. In this study, we do not have enough not-yet treated-units for this approach (recent studies show that in staggered designs and in the presence of treatment effect heterogeneity, a unit can be used as only treatment or control but never both). Thus, we employ an alternative approach. We test whether the rejected applicants experienced a significant increase in their performance after their rejection date and relative to a **control group**.

- The **control group:** firms that are similar to the rejected applicant with respect to observables but never applied for the program.
- The **logic**: if the increase in performance is all due to unobservable factors – e.g., having an innovative idea or having an opportunistic manager – and not affected by the program at all, then the rejected applicants would be as likely as the accepted applicants to experience an increase in their performance.
- If the rejected applicants do experience a significant increase in their financial performance after rejection date and compared to a control group, self-selection bias becomes a strong possibility; the larger the magnitude of the increase, the larger the size of the bias.
- However, the rejected applicants are often not as good as the accepted ones in one (often observable) way or another – that's why they are rejected. Thus, if the rejected applicants do not experience an increase in their performance, it could be attributed to the reason for which they were rejected.

Annex 3: Gender-Based Analysis Plus

- This study finds a positive and statistically significant relationship between the share of female employees in the workforce and the financial performance of a firm.
- However, on average, both accepted and rejected applicants have smaller shares of female employees in their workforce than a typical Canadian agri-food firm.
- This counter-intuitive pattern poses a question for future research.