# Report on: "Evaluation of SkatteFUNN"

Ingvil Gaarder*

January 7th, 2018

Samfunnsøkonomisk Analyse (SA hereafter) evaluates SkatteFUNN on several dimensions. The main part of the analysis is chapter 4 and 5 where they aim to empirically answer questions like; has SkatteFUNN induced firms to invest more in R&D? Have more investment in R&D resulted in more innovation and higher labor productivity? And are there externalities, like peer-effects, from the program?

There are two main challenges with empirically evaluating SkatteFUNN. The first is data availability. The main outcome variable of interest, firm specific R&D investment, is not available from administrative data. Instead, one would have to rely on survey data with potentially inaccurate information and small sample sizes. I think SA handles these issues in a good way.

The second challenge is to find a credible research design. I think the approaches used are in general good, but I have some comments on how to potentially improve the strategies and how to further test the identifying assumptions below.

## Comments on ch. 4: Input additionality of SkatteFUNN

In chapter 4 SA uses two approaches to evaluate whether SkatteFUNN actually resulted in increased R&D investment (measured as increased R&D expenditures).

### Empirical Approach 1

Approach 1 uses the expansion of SkatteFUNN in 2009 as a source of variation. I think this is in general a very good idea, but at the same time, I think there's some scope of improvements in the implementation.

The identifying assumptions behind empirical approach 1 is illustrated in equation (4.2) in the report:

$$\ln(R\&D_{it}) = \alpha + \gamma \ln(sales_{it}) + \chi \ln(subsidies_{it}) + \sum_t \delta_t D^{\text{year}t} + \sum_t \phi_t D^{\text{year}t} \times D^{\text{below cap}} + \eta_i + \varepsilon_{it}. \quad (1)$$

Here, the parameter of interest is $\phi_{2009}$. To get identification, they utilize a naturally occurring discontinuity built into the tax credit scheme. Firms that invest more than 5.5 million NOK in R&D before 2009 will have no incentives to increase their investment as a result of the policy change in 2009, while firms investing less than 5.5 million NOK will have an incentive as they receive an increased subsidy

*Email: ingvil.gaarder@gmail.com. Affiliation: Harris School of Public Policy, University of Chicago, USA

on the marginal investment. The empirical strategy consist of comparing firms that invested less than 5.5 million NOK to firms that invest more than 5.5 million NOK. The identifying assumption behind this strategy is that firms above and below the 5.5 million NOK cap would have had parallel trends in ln(R&D) absent of the expansion in SkatteFUNN (conditional on firm fixed effects). It is therefore, worrying that several of the estimated $\phi$s prior to the reform in 2009 are statistically different from zero as this violates the common trend assumption.

I think it would be more reasonable to compare firms just above and just below the cut off as one would do in a Regression Discontinuity (RD) design.[1] The identifying assumption would be that no other determinants of R&D changes discontinuously around the 5.5 million NOK cap. I would be curious to know if SA has considered using this type of identification strategy?

Equation (1) includes the contemporaneous control variables ln(sales) and ln(subsidies). Both of these controls are endogenous and could very well have been outcome variables in the report. Including ln(sales) as a control is proposed by Hall and van Reenen [2000] and their argument is that under some assumptions on the production function and the market, the specification proposed is equal to the first order condition of the firm's profit maximization problem. However, output is a function of inputs, where R&D is one of the inputs. This implies that output (here proxied by sales) should be directly affected by the treatment itself and hence is an outcome variable. Angrist and Pischke [2008] labels these types of control variables as *bad controls*. Let's assume that the variable $D^{2009} \times D^{\text{below cap}}$ is indeed exogenous in this specification (excluding sales and subsidies) and $\hat{\phi}_{2009}$ gives us the causal effect of expansion of SkatteFUNN on R&D expenditures. Then, including a bad control will create a selection bias. If we think R&D makes the firm more productive and more productive firms sells more output/have higher revenues. Higher output firms are then more likely to be the firms doing R&D, and the reform in 2009 will change the composition of high output firms resulting in a biased estimate. A similar argument can be made for subsidies as they are likely to be complementary to SkatteFUNN (cf. chapter 7 in the report). I would advise SA to instead of using contemporaneous sales and subsidies to use pre-determined values of sales and subsidies in 2001. If this is not possible, it would be necessary to drop the two controls all together.

How are the standard errors calculated? The standard errors should be clustered standard errors which are robust to heteroskedasticity and to correlation over time within a firm.

**Empirical Approach 2**

Approach 2 combines a difference-in-differences (DiD) strategy with more than one treatment period combined with propensity score matching. This is a good idea. However, the identifying strategy has some testable empirical implications which SA has not investigated.

The identifying assumption in a DiD model is common pre-reform trends. Using data on R&D expenditures before the implementation of SkatteFUNN in 2002, one can investigate the determinants of the pre-reform trends, The idea is to find out how much of the difference in the pre-reform trends are explained by later treatment status. This can be done by taking the expression in equation (2) in

[1] See Imbens and Lemieux, 2008 for a guide on how to implement RD designs.

section 4.4:

$$Y_{it} = \gamma^0 + \sum_{T \neq 0} \gamma^T G_i^T + \sum_T \tau^T D^T + \sum_{T_0} \sum_{T_1 \geq T_0} \alpha^{T_0 T_1} G_i^{T_0} D^{T_1} g_{it} + \sum_j \beta^j X_{it}^j + \varepsilon_{it} \tag{2}$$

and add in additional terms. We would like to estimate $\alpha^{T_0 T_{0-k}}$ where $k > 1$. E.g for the first user generation ($T_0 = 1$) we would like to estimate $\alpha$ for the period before $T_0$. The test would be check if $\alpha^{T_0 T_{0-k}}$ is statistically different from zero or not for different user generations and as many pre-reform periods as possible.[2]

Again, I am worried about the inclusion of subsidies as a covariate and I would like to know how the standard errors are calculated.

## Comments on ch. 5: Output additionality of SkatteFUNN

In this chapter SA investigates whether R&D results in more innovation and higher labor productivity, and whether there are heterogenous effects by SkatteFUNN (and NRC) take-up.

### Innovation and patents

Equation (2) in chapter 5.2 is given by

$$INNO_{it}^* = b_0 + b_1 D^1 + b_2 S_i + b_3 D^1 S_i + \delta_1 \cdot r_{it} + \mathbf{X}_{it}^{inno} \beta + \eta_{it} \tag{3}$$

where $r_{it}$ is R&D intensity and the other notation is the same as before. SA interpret $b_3$ as the "additionality" of SkatteFUNN in terms of additional innovation effort as a result of the policy (pg. 68). However, a more accurate interpretation of $b_3$ would be: the additional innovation effort as a result of the policy, holding R&D intensity fixed. The hypothetical policy is therefore to expand SkatteFUNN and at the same time holding R&D intensity fixed. However, SkatteFUNN is designed to increase the R&D intensity and it is through this mechanism we would expect increased innovation (at least as the first order effect). In addition, we have a simlarempirical issue as commented under chapter 4; we cannot control for (contemporaneous) R&D intensity as this is an outcome variable of SkatteFUNN and would create a bias in the estimated $b_3$ through the channel that SkatteFUNN changes the composition of firms at a given level of $r$.

I would instead try to get at the intention to treat effect of SkatteFUNN on innovation. This would have a second advantage, as one limitation with the analyses in chapter 4 is that SkatteFUNN might have an effect on how firm reports R&D expenditures (or simply classifies R&D expenditures within the firm, potentially in a legit way). If firms who are receiving SkatteFUNN are more inclined to report more of their expenses as R&D we will overestimate the effect of SkatteFUNN on R&D expenditures. It would therefore be good to expand the analysis in chapter 4 including more outcome variables that are potentially harder for the firms to manipulate, but still related to R&D (like measures of innovation and patents).

[2] See Autor [2003] for an example of this approach.

### Labor productivity

Also here I think it would be good to supplement the existing analysis with an intention to treat analysis where one uses the model in equation (2) to estimate the reduced form effect of SkatteFUNN on labor productivity.

## References


Joshua D Angrist and Jörn-Steffen Pischke. *Mostly harmless econometrics: An empiricist's companion*. Princeton university press, 2008.

David H Autor. Outsourcing at will: The contribution of unjust dismissal doctrine to the growth of employment outsourcing. *Journal of labor economics*, 21(1):1–42, 2003.

Bronwyn Hall and John van Reenen. How effective are fiscal incentives for r&d? a review of the evidence. *Research Policy*, 29(4 – 5), 2000.

Guido W. Imbens and Thomas Lemieux. Regression discontinuity designs: A guide to practice. *Journal of Econometrics*, 142(2), 2008.