

Comments on the report: “Evaluation of the regionally
differentiated social security contribution in Norway”, draft
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About the author

Frode Steen has been a professor since 2004. Presently he is the holder of NHH's NorgesGruppen-professorship in Competition Economics on grocery Markets. He has since 2005 been a research fellow in the Centre for Economic Policy Design (CEPR). He received his PhD degree from NHH in 1995, and he has an MA in Economics from the University in Bergen from 1991.

He is working in the field of econometrics and empirical Industrial Organization, and have undertaken studies of several industries and markets; telecom/media, cement, shipping, airline, gasoline, grocery, salmon etc. He has had longer research stays abroad on several occasions working with co-researchers on joint research; Simon Fraser University, Vancouver, D'Economie Industrielle (IDEI), Toulouse, Copenhagen Business School, Copenhagen and Helsinki Center of Economic Research (HECER), Helsinki, and is presently visiting scholar at the Department of Economics at U.C. Berkeley, California.

A primary interest in his work is related to market power, market regulation, cartels and their functioning. His research publications have appeared in journals as e.g., *American Economic Review*, *American Economic Journal Microeconomics*, *European Economic Review*, *Scandinavian Journal of Economics*, and *International Journal of Industrial Organization*, *Review of industrial Organization*, *Industrial and Corporate Change*.

He has also an extensive experience as external competition expert in sector specific regulation and competition cases in Norway, Sweden and the EU. He has provided written and/or oral testimony in competition and regulation matters in court in several countries.

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General comments

I have been asked by the Ministry of Local Government and Modernisation, to serve as an external expert, and to comment on the report "*Evaluation of the regionally differentiated social security contribution in Norway*" written by *Samfunnsøkonomisk analyse*. I will comment on the report chronologically, following the structure in the report.

However, my general impression is that it must have required a lot of work to undertake this study. Overall, a significant research effort has been made, and several interesting results can be found in the report, though I am more skeptical to the interpretation of these in the reports present form.

My major concerns are twofold. First, this is a tedious report to read, and I doubt that any general interest reader will take the effort to read such a report in the present state. It is technical, implicit and often more like a research paper than a report when explaining econometrics and models. If this is to be used more widespread, either a short version, or more pedagogical summaries must be included.

Second, and more importantly I think the report presents a lot of results that are interesting, but I do not find the positive conclusions on the efficiency and the positive effects from the regionally differentiated social security contributions (RDSSC) warranted by the econometric results. I do believe that this might be due to a lot noise that even after careful decisions on what data and which changes in the scheme to analyze, cannot be fully captured in the models, and thus the results are not particular precise. However, when reading conclusions/summaries, and also how one refers to the 'findings' in the econometric analyses in the later chapters on dynamic effects etc. I think the 'findings' on the effect of the RDSSC are overstated.

For instance in the summary at page XIII: "*We find effects on both wages and employment, indicating that there are direct as well as indirect effects on employment and population in the eligible areas. There are good reasons to believe that the overall effect of the scheme is significant, especially in the zones with the lowest payroll tax rate.*" The first statement is only weakly supported by the results. The latter statement is not even possible to support from the econometric analysis. Since no changes has taken part in this zone (5), how do we know anything about the effects? The next sentence is in the same fashion "*Furthermore, the scope of distortive effects on competition and trade appears to be tolerable.*" Reading the five pages attributed to this question in Chapter 9, clearly the most important takeaway is that these effects is not possible to test in a reasonable fashion using aggregated data (as is done). Then stating that the effects are 'tolerable' is simply not correct, we do not know from this report whether, or not, these effects are tolerable.

Basically, the econometrics undertaken, provides if any – very weak statistical evidence for the efficiency of the RDSSC scheme. The authors should be a lot clearer on this. It is not the researchers that are to blame here, but the actual difficulty in measuring small effects when so much other stuff is going on. But the authors need to be upfront with this, rather than make it look like they have clearer results than what they have.

Finally, I also agree with the authors, one should rather impose experiments on allowing municipalities to choose between payroll tax reliefs or direct municipal transfers. (Or, even better – but hardly politically possible – to randomize between such measures). That is the only way one in the future could test for possible effects statistically. As of now, with so many changes for some smaller areas, but no changes for the bigger areas (Zone 1 and 5) data do not lend itself to proper econometrical testing. It simply too difficult to single out the effects given that they are if any, small and other things matter also.

Specific comments

Chapter 2 Regional Differentiated Social Security Contributions

Migration figures

The discussion around Figure 2.1. I agree that there has not been a big change since 1975, but the trends change, migration between municipalities was in 1975 around 48%, went trend wise down to 38% in 1992, for then to trend wise return to nearly 50% in 2015. This is large changes, in the variable we are mostly concerned with, when it comes to the RDSSC-scheme. A change in trend, together with a large change in numbers (from 48 to 38% amounts to a relative change above 20%).

Another issue that deserves some attention is unemployment figures behind Figure 2-3. Norway experienced a lot of dynamics on the west coast after the oil-price fell some few years ago. Why not look at what happened with both unemployment and migration before and after this change – can we learn something from the dynamics. 2017 is just a one shot picture. Did people move, and where?

Static accounting of losses due to RDSSC-scheme

In Figure 2.6 and in the discussion around one undertakes an accounting of the losses in tax income. This is a very static picture. We do not know the alternative situation. I grant that this is not possible to measure either – but say so.

Averaging can also hide heterogeneity

In Table 2.1 we find numbers for population across zones. Besides Zone 4a (Bodø and Tromsø) Such numbers might hide a lot of heterogeneity. One way to highlight this would be to also show deviations in percentages of means.

Chapter 3 Theoretical framework

I have no major comments here.

Chapter 4 Data

This is very short. I presume that either in an appendix or in the main text more explanation will be provided.

Chapter 5 Empirical evidence

Most of my comments will be on this chapter.

5.1 Evaluation of the reform in 2000

Here we come to the major challenges in this study. To be able to find effects one concentrates on some changes in the regime and on some zones. Furthermore, one exclude data for various reasons to concentrate a dataset that potentially chow effects, and is affected from changes in the RDSSC-scheme.

One way of interpreting this method is to say that if you cannot find effects in such a reduced refined dataset – no effects exist. I have however two major comments on data selection:

Use excluded industries as controls

In Chapter 5, p.41 we read:

“During our estimation period (1996-2003), several industry exemptions were put in place in accordance with ESA rulings. Firms in these industries paid the maximum payroll tax rate regardless of geographic location. The industries are:

- Production and distribution of electricity
- Extraction of crude oil and natural gas
- Services activities incidental to oil and gas ex-traction excluding surveying
- Mining of non-ferrous metal ores, except ura-nium and thorium ores, as well as some firms in mining of chemical and fertilizer materials
- Building and repairing of ships

- Manufacturing of basic iron and steel and of ferro-alloys
- Financial intermediation
- Freight transport by road (firms with more than 50 full-time employees)
- Telecommunications

In addition to excluding the firms in these industries, we also exclude the public and primary sectors.”

To me quite a few of these seems as perfect controls in a diff-in-diff test of the RDSSC-scheme. They exist both before and after the scheme change, and are unaffected. Leaving them out seems like wasting relevant information.

Robustness of excluding data

On p. 41 we read several arguments for what data to exclude. The only robustness tests we see later on the effect of all these exclusion restrictions (p.51), is where one tests for the importance of trimming out outliers: But how large was the original sample as compared to the estimation sample? What happens if these data are included? Actually, you even can say something about what you should find by including more data (that is beyond my previous point on industries that should have been included in the control group), you should find weaker results since you include more noise. In itself, such a result would be a consistency test of your trimming choices.

Now, turning to the models and the results I have several comments. Let us take them in turn:

Clustering of error terms?

Tables 5.6-5.7 present results where one weights error terms using number of employees (right?), Tables 5.8-5.9 present results based on error terms that are clustered on the firm level. First, clustering is perceived as the most robust framework, why not using it in all cases? And if the weighting is better, why not use it across all four groups of estimations? At a minimum they need to discuss their choices, and I would like to know what happens with significance in Table 5.6 and 5.7 also with clustering. Another option is clustering on municipal level or industry level – why were these options not considered, and if they were, why are they not telling us?

Average treatment effects vs yearly treatment effects and trend specification

They include yearly dummies, what about other trend specifications? This also relates to their treatment estimates (diff-in-diff) that both are estimated as a full treatment periode (average) effect (2000-2003), and yearly effects for each year after the RDSSC change. (A detail: You use post-2000, why not name treatment as post-1999, given that 2000 is part of the treatment period?).

First, why should we see effect in some years but not others after the change see eg. Table 5.7)? It is difficult to understanding why this should happen. You might think that the effect took time to work, but then we should see the effect turn on for then to continue. This 'on and off' seems odd, and they never discuss it. An alternative specification would be to include a linear trend and then also, interact diff-in-diff with a trend to see whether there was a trend shift after the change in RDSSC. Testing for individual years might make sense, but then they need to come up with a good economic theory for why it is reasonable that the effect turns off and on, otherwise these results weakens the credibility in the conclusions.

Pre-treatment tests

They include yearly diff-in-diff also for 1998 and 1999 to test for pretreatment effects. As I read the models this implies that they have yearly dummies for all, then they have a diff-in-diff dummy for all years but 1997. How should we understand this model? How should we interpret the results? Are all other year dummies significant besides the diff-in-diff 1999 dummy? I find this model hard to understand, even more so given my previous point.

Robustness of results and in particular on the Tertiary sector

If we first look at their diff-in-diff overall results (*Treatment x Post 2000*, columns 1 in Tables 5.6-5.10), basically they only find significant effects once, which is in Table 5.6; Individual wage growth where the overall diff-in-diff is equal to 0.5% on a 99% level: the treated firms have an additional 0.5% wage growth. In Table 5.7 the diff-in-diff for employment growth is higher and equal to 1.8%, *but only on a 90% level* of significance. Is this enough to support their conclusions of the positive effects of the RDSSC-scheme? Hardly!

But, let us look closer on the one sector where they seem to find results, the Tertiary sector. Under I present all yearly diff-in-diff results from the four tables (stars refer to significance similar to their tables, bold implies significance on 95% level – which after all is the conventional significance requirement in econometrics):

Individual wage growth	(2000) 0.1%, (2001) 1.4%*** , (2002) -0.0% (2003) 0.4%
Employment growth	(2000) 1.6%, (2001) 4.1%*, (2002) -1.9% (2003) 5.5%***
Hours worked	(2000) 1.0%, (2001) 2.2%, (2002) 1.7% (2003) 4.4%**
Growth rate value added	(2000) 3.8%*, (2001) -0.7%, (2002) 1.4% (2003) 4.9%**

They find yearly effects across all regressions, however these are very strange. For individual wage growth there is an increase in 2001, but not in any other year. For the next three regressions there

are significant growth in one year only, 2003. And, we find a strange 'up-and-down' patterns for the three years before. How can we explain that the RDSSC-scheme change that stayed the same after 1999 had an effect, but only after three years. It is tempting to doubt such a result. It is much more likely that something happened in 2003 not related to the change more than three years earlier in the RDSSC-scheme.

How I read this – it opens up for questions about what is not accounted for, and it seems as this 'what' might be within services – the tertiary sector. What kind of firms are we talking about, for instance public services are not included. This needs a thorough discussion before I believe in these results.

Given that they are excluding a lot of noise, they should find results here, if the RDSSC-scheme really matters. Well as I read it, they do not. They might claim that they have a weak overall result on wage growth, but do we think a 0.5% increase is a *significant economic* effect. To me this is a very small number.

5.2 Evaluation of the reform in 2004

In this section I think the Regression Kink Design (RKD), using kinks in the schedule of payroll taxes to pay and study only *the* firms that are close enough to the threshold is the most robust analysis. The method gives them causal effects of an increase in the payroll tax rate on wages. However, since the method is data demanding, they only look at zones 2 and 4.

Also here I would like to see robustness results related to the data selection – obviously this method if anything, is even more sensitive to which data exclusions are imposed.

This method provides more conservative and believable results, and they are causal. Here they do indeed find that a smaller part of the increased total wage costs is shifted onto workers, i.e. that most of burden of payroll taxation resides with the employer. (range 4- 17 pct. for firms in zone 2 and in the range 0.5-4 pct. for firms in zone 4).

The degree of the shift in the case of the tax-rate increase is very much lower than in the case of tax-rate decrease studied in the diff-in-diff approach (where numbers as high as 90 % were found for of the incidence of the tax reduction resided with employees, p.64). In addition to the lack of causality in the diff-in-diff (as explained also by the authors), the results are extremely sensitive due to the calculations of relative (very small) numbers (see eg., footnotes 45 and 47). I am pretty sure that if one test for these relative numbers properly (easily done in Stata) they will come up with very wide confidence intervals (and this should be done by the way).

A small comment on Tables 5.15 and 5.16, use stars for significance, or take them away everywhere, though me personally, I find stars helpful.

5.3 Estimating behavioural responses

I think the major problem in this part is how to prevent comparing apples with oranges. The wage figures (by necessity) is averaged figures from accounting statistics. This implies that there is an enormous heterogeneity hidden behind the averages (education levels differs across firms and also very likely across RDSSC zones). This is in fairness to the authors referred to when discussing the results, where they find a very surprising and highly unlikely effect of more than one from payroll tax changes and wage costs. The authors try to mend this by various other specifications, but they still find this 'wrong' result. I think that non-accounted-for-heterogeneity is driving the results in this whole section. I would therefore be more than cautious when evaluating the results, and in particular I would not use this section as support for summary conclusions on behavioural responses. Basically, the result here are not robust.

A small last comment on the long run elasticity of labour demand on p.78, which is larger than one in absolute value ($=1.102$). This is for me a high number, and I think it deserves to be tested. All the numbers entering in the calculation (see footnote 59), are from the model and has separate variances and covariances and you can easily test this number. Does the confidence interval include -1, or for that matter 0?

Chapter 6 The dynamics of regional population growth

Here I feel that the starting point is wrong: p79: *"Discussions and empirical evidence in the previous chapters show that RDCCS to a certain extent contributes to higher employment in reduced tax zones than would otherwise have been the case."* I do not agree, we are not there yet – not with the empirics produced in Chapter 5!

Disregarding this, Chapter 6 provides a discussion of dynamics and local population growth. On p. 82 I find the argumentation to be a bit biased. They state: *"Regional policy may be the main reason why Norway has a lower share of its population located in urban settlements compared to the other Nordic countries, and especially Sweden. In this case one can argue that the weaker urbanisation process in Norway than other Nordic countries in itself indicate an effect of regional policy measures such as RDSSC."* I disagree. We have so many other things that might explain the Norwegian regional settlement. In particular differences in topography, communications (roads and trains), coastline with traditional fishery and highly protected agriculture (also compared to all other Nordic

countries), slower urbanization due to so many other factors and what not. Even national independence and differences in wealth matters here, which both were lower/weaker in Norway than eg., Sweden for most of the time when this settlement was formed. Not to mention Denmark. To attribute the development in the Norwegian regional settlement to RDSSC alone, that started first in 1975 is thus problematic..

On the same page in the report one goes through literature on demand and supply driven population patterns. I do not know this literature, but the citations seem old (1964, 1966, 1971 and meta analysis from 1987...., the newest from 2005).

Another more principal question, is how urbanizations theories applied on larger cities applies to building up larger population in rural Norway with very small settlements. Indeed, I do agree with the argument on p. 83 *“a growing population represents a market for firms, and especially firms which supply services to the population such as wholesale, services and construction. There may also be a multiplier working in the way that growth in knowledge-based companies give larger marked for services of different kind.”* But, does it apply also on small scale rural labor markets to the same degree?

For instance, a 20% increase in the number of jobs at Værøy or Røst will probably not matter much to these ‘services’ or ‘knowledge-based companies’. I think one should be more careful applying this theory on small-scale communities as we are focusing on here. At least discuss whether these effects are subjected to thresholds, in the sense that the might apply to cities as Bodø, but not rural communities that are so much smaller. If so, this also have bearings on correlation in employment growth and population growth that will be weaker.

On p. 84/85 one talk about correlation between population and employment. *“Municipalities with positive population also experience positive employment growth. This is the case for all municipalities, inside and outside the rural policy region. The regional policy region consists of the municipalities with particular challenges regarding population and employment growth, and hence are eligible for various rural policy measures. However, the statistical correlation is not that high at 0.42 (0.32) within (outside) the rural policy region, which implies that about one third of the variation in employment is explained by population growth and vice versa. There are, however, not possible to identify a causal direction with such correlation coefficients. The relationship is not particularly strong either, as we would expect a correlation coefficient close to one[me underlining].”*

Yes indeed the correlation is low. Causality can explain correlations; correlations cannot explain causality (without a theory at hand and statistical identification methods.) But, I will not expect a

high correlation here. The point is that even though, say an engineer can have a job in the rural areas, he might not want to, because there are so many other things that drives settlement patterns. Thus finding correlation which is one-to-one here would be highly unlikely. Even when they later find higher correlations accounting for commuting I am not convinced. We all know that correlation is influenced by trends, two time series that are departing over time both having a positive trend can be very correlated – but this is due to the positive trend, not causality or interdependency.

Which is more in line with how I read the Tervo (2010) study of Finland. In sum, I think this chapter is 'too positive', given the evidence presented. I would be more careful making the conclusions drawn here. When they start sub-section 6.5 at p. 88 with *"Evidence from aggregate studies support the hypothesis that people follow jobs."*, I would add, yes, but the causality in correlation is not clear here, neither from the empirics in this report, nor from all studies cited. Which indeed is done to a certain extent later in the report, but I would give the part being critical more space and weight.

Chapter 7 Alternative measures

Again the starting point is overly positive towards RDSSC (p.95): *"Our empirical tests also indicate that RDSSC lead to an increase in wages and owner income and thus household's disposable income within target regions."* No, this is not proven in your empirical tests!

Two additional comments here.

First, on p. 92; *"About 3,5 billion kroner was transferred to the agriculture and fishery sector, one billion kroner to rural measures and the remaining measures to other sectors and areas."* I think that this statement is positively wrong. The agricultural sector gets another 20 billion plus in support through the import tax system in 'shielding support, which is by far the reason why we have agriculture in the rural areas across Norway and not only in some few counties. This shielding-support is also far higher in numbers both compared to other Nordic countries and what is represented by the RDSSC-scheme. As the Chapter is now, the RDSSC-scheme is presented as bigger than most other regional supports. This is positively wrong.

Second at p. 98 a study by Cappelen et al. (2015) is discussed. They find that companies with rural loan and grants perform better than other companies without such support. This I believe, but the question is rather why? Is it because this support makes them better, or is it because the selection process for which firms receiving such support. Does the rural support system choose better

managed/entrepreneuried firms to support. Untangling these effects from each other is not possible, but needed to answer if you want to conclude on the usefulness of the support.

Chapter 8 Ripple effects

Again the challenge is what we have found earlier? At p. 104 we read: "The econometric estimations are revealing possible total effects of the scheme by means of so-called exogenous shocks, related to changes in the scheme. The objective of the ripple effects analysis is to decompose the total effect into direct and different types of indirect ripple effects." Well if we have very small effects, if any at all, uncovered in the first place, how then to find any ripple effects in this chapter.

I am not familiar with these models, but from reading the chapter I do not understand how the regional component is treated to account for the local effects. More particular, they are forced to look at municipals/firms shares within counties as long as the RDSSC-scheme follows municipal borders, not county borders. How large are these shares, if small, how can we be sure that the Panda results are driven by these municipals/firms? But again, the chapter is very short, and I am not very familiar with these models. But, I would like to see a more critical discussion about the ripple result of "37,1pct"... (By the way commas in English is written as ".")

Chapter 9 Distortive effects

This is supposed to be the part of the report I should comment most on, but 5 pages is hard to write too much about.

The first part tries to look at distortive effects domestically by looking at value added across municipals and zones. In the Table below, I report the dummy estimates with stars together with payroll tax levels.

Zone	Estimate from Section 9.4	Payroll tax
1a	0.0169	14.10
2	0.0235	10.60
3	0.0171***	6.40
4	0.0169*	5.10
4a	0.0126	7.90
5	0.0137	0.00
Correlation between estimate and payroll tax		0.46

They do actually find some significant differences (when making strong assumptions on data), for two zones 'in the middle', but not for Zone 5 where the tax relief is at it's highest. More worrying is the fact that the estimates are positively correlated with the payroll tax, the more tax the higher added value?

This exercise seems driven by other things than payroll zones. I cannot see what we learn at all. I would have removed it in its present form.

Hence, when they state at p. 111: *"In short, the result indicates that there may be few general competitive distortions due to the RDSSC scheme. However, that does not imply that no firm is using the reduced social security contribution in order to gain competitive advantage, but the results indicate that on average the reduced social security contribution does not spill over into distorting competition."*

I am just more confused. And, they continue:

"However, as noted earlier only actual price data or trade data can be used, in a case by case basis, in order to corroborate any price or trade advantages due to operating in a region with lower social security contribution."

Yes indeed, **case studies are needed**, which is exactly what I suggested in my report May 10th 2015 – some three years ago when talking about doing aggregated studies.

I do not think we get so much wiser from the next subsection, *9.5 Location and the extent of the market*. On the chosen aggregation level so many things can be driving the results, thus listing industries in these two groups, one not gaining from being located in zones, one gaining, is still too aggregated to give much information.

When it comes to trade, I realize that the data is not good enough. But again, then the reasonable solution is to look at cases. This has not been done, thus we have no empirical knowledge from this report whether the payroll tax system has an impact on trade. I will be surprised if such an effect is present, but as long as we do not really know empirically whether the payroll tax reliefs carry over to the workers or stay in the firm we can at best speculate. Statistical evidence could have been found if one had started out with the right angle: Case studies, but this aggregated attempt fails.

In general this distortion chapter seems more like an early version than the others (except 'Data'), which makes me suspect that the distortion issue has not been given much attention in this report/study.

Thus when they conclude on p. 112: *“In sum, there may be effects, but they are hardly detectable and must be resolved on a case by case basis where price and (geographical) market share data are utilized in order to ascertain the degree of competition.”* I agree, but why then write Chapter 9?

Chapter 10 Concluding remarks and recommendations

As I have said before, I simply have a hard time in accepting that the empirical tests give solid evidence in support of the RDSSC-scheme. Thus, Chapter 10 is overly optimistic. When they state on p 115: *“Based on our data it is reasonable to assume that RDSSC contribute to between 2 and 5 percent higher employment in the eligible regions than would otherwise have been the case.”* I simply disagree.

Whether this implies that the RDSSC-scheme has, or has no effect is another issue, but that cannot be proven in an econometric fashion from this report.

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About the author

Frode Steen has been a professor since 2004. Presently he is the holder of NHH's NorgesGruppen-professorship in Competition Economics on grocery Markets. He has since 2005 been a research fellow in the Centre for Economic Policy Design (CEPR). He received his PhD degree from NHH in 1995, and he has an MA in Economics from the University in Bergen from 1991.

He is working in the field of econometrics and empirical Industrial Organization, and have undertaken studies of several industries and markets; telecom/media, cement, shipping, airline, gasoline, grocery, salmon etc. He has had longer research stays abroad on several occasions working with co-researchers on joint research; Simon Fraser University, Vancouver, D'Economie Industrielle (IDEI), Toulouse, Copenhagen Business School, Copenhagen and Helsinki Center of Economic Research (HECER), Helsinki, and is presently visiting scholar at the Department of Economics at U.C. Berkeley, California.

A primary interest in his work is related to market power, market regulation, cartels and their functioning. His research publications have appeared in journals as e.g., *American Economic Review*, *American Economic Journal Microeconomics*, *Economic Journal*, *European Economic Review*, *Scandinavian Journal of Economics*, and *International Journal of Industrial Organization*, *Review of industrial Organization*, *Industrial and Corporate Change*.

He has also an extensive experience as external competition expert in sector specific regulation and competition cases in Norway, Sweden and the EU. He has provided written and/or oral testimony in competition and regulation matters in court in several countries.

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General comments

I have been asked by the Ministry of Local Government and Modernisation, to serve as an external expert, and to comment on the report "*Evaluation of the regionally differentiated social security contribution in Norway*" written by *Samfunnsøkonomisk analyse*. I have earlier made two reports on their earlier plans and drafts, this time I am asked to focus on Chapter 8, on potential impact on competition and trade from the social security contribution.

I have also read Chapters 4-7 of the report to be able to comment on Chapter 8 and though the chapters are more complete, better written and extended in this last version, I still feel that the conclusions are stronger than what the empirics warrant. This is not due to lack of analysis or substantial effort from the researchers, rather from the fact that the empirical effects from the differentiated social security system are difficult to trace when so many other things also happen in the Norwegian economy. Thus, in my mind one should still be careful when drawing positive policy conclusions from the empirical evidence in the report.

When it comes to Chapter 8, I think it has improved. In particular, the part where they analyze potential effects from trade. The discussion and the section on impact on domestic competition lack any form of analysis, and is as it stands, useless to evaluate the question whether regional differentiated social security tax influence domestic competition. As such, the Chapter only give some useful insight into the potential effects of the differentiated social security scheme on trade.

Under I will comment Chapter 8 in more detail.

Specific comments: Chapter 8 Regional Differentiated Social Security Contribution's impact on competition and trade

First on the focus provided to this topic. The report itself is pretty comprehensive, in total 154 pages, whereof only 10 is attributed to competition distortions. Furthermore, if we look closer on the analysis undertaken on this topic, the focus is even smaller. Chapter 8 has five subsections, the first two and the last is just repeating what is said in the two subsections focusing on domestic (8.3) and international (8.4) competition. To illustrate my point I show the focus of this analysis in terms of number of characters in the report in the Table under.

	Report	Chpt 8	Chpt 8.1,8.2 and 8.5	Chpt 8.3 Impact on domestic competition	Chpt 8.4 Impact on International Competition
Characters	328 899	23 477	8 473	2 662	12 277
% of report		7.1 %	2.6 %	0.8 %	3.7 %
% of Chapter 8			36.1 %	11.3 %	52.3 %

Chapter 8 represents only 7% of the report, but what I will consider as the 'analysis' represents just above 4% of the report, and when we scrutinize on the focus given to the impact on domestic competition, it is less than 1% of the report, (and just one tenth of Chapter 8). Why do I show these numbers? Basically, to underline two major conclusions: (i) The topic is not given serious attention generally, and (ii) absolutely no analysis is provided on domestic competition distortions.

Obviously, the number of characters provided on a topic is not a good measure of quality and focus, but in this particular case, I think it makes a representative picture of the status of the analysis of this topic. However, and to be fair, in this version of the report, the impact discussion on international competition and potential distortions from Norwegian trade and exports are providing some interesting evidence. However, not so much on potential distortion, but rather on the consequences and the size of the potential problem.

I see several challenges that need to be solved before a final report is delivered and I will go through these by discussing the subsections in turn.

Chapter 8, subsections 8.1, 8.2 and 8.5

Here the authors really only summarize and states the conclusions from the remaining two sections. Attributing more than one third of the chapter to this seems odd, when so little analysis is actually undertaken in the rest of the Chapter. Much of what is stated here, is just repetitions of what is argued elsewhere in the report. In particular subsection 8.2 in its present form can be dropped. For instance, the last paragraph of this section is simply difficult to understand. First they say that *“there are few studies measuring the impact on competition and trade due to differentiated payroll tax schemes”*, but absolutely no such (‘few’) studies are cited. Instead they cite six studies totally unrelated to the question (studies on eg., impact on employment, wages etc.). Then they ‘conclude’ that *“there is no methodology on how to measure to what extent a differentiated payroll tax has an impact on competition and trade.”* Within research, recipes for each problem seldom exists. However, it does exist a large empirical industrial organization literature (quite some papers was cited in my 2015 report), that provides methodology on everything from reduced form tests to full structural models that can be used for the purpose of testing how various market imperfections, regulation schemes or market particularities can be tested for. No such papers are however cited, and only one textbook is cited in footnote 89, Davis, Garcés (2010), which does not exist in their reference list, and which actually was published in 2009.¹

Again, I get the feeling that also the previous versions of the report have uncovered, the research group does not possess the necessary competence when it comes to the analysis of competition and trade.

Chapter 8, subsection 8.3 Impact on domestic competition

This is a very short subsection. First, they (again) describe the purpose of the Norwegian differentiated payroll tax scheme as well as other countries similar schemes. Then they tell us that they start the *analysis*: *“To analyse the impact on competition and trade, we will first analyse the impact on domestic competition. We return to the question on international competition below.”* By now they have spent 1249 characters of subsection 8.3, which amounts to 47% of the entire subsection!

Now, what I presume is meant to be the analytical part starts, and for completeness, it is rendered under:

¹¹ The correct citation is Peter Davis & Eliana Garcés (2010) “Quantitative Techniques for Competition and Antitrust Analysis”, Princeton.

“When firms in the eligible zones provide goods or services in competition with firms falling outside the scheme, the first group of firms will benefit from an advantage compared to the former. Thus, competition between firms will be distorted.

However, it could also be the case that firms in more rural areas were experiencing a competitive disadvantage, due to inefficiently high wages, larger distances between consumers and producers and transportation costs. This competitive disadvantage is reduced with the lower rate of social security contribution.

We do not find locational advantage of locating a firm within zones with reduced social security contribution, cf. chapter 7. In sum, there may be effects, but they are hardly detectable and must be resolved on a case by case basis where price and (geographical) market share data are utilized to ascertain the degree of competition.”

The only result referred to is the lack of effects found in Chapter 7 – well I cannot see that this very aggregated ripple effect analysis gives any help here, and if this result was correct why then be supportive for the scheme at all – no locational advantage? What does this tell us about competition? I also think that my major concerns from my report written in April 2018 still applies when it comes to Chapter 7.

Now they state that a test could have been performed – but lack of data makes it impossible:

“To analyse the impact on competition and trade, it would have been useful to examine the correlation of prices on goods and services across zones with different social contribution rates. Unfortunately, there are no regionalised time-series data of prices, nor any reliable annual intra-national trade statistics which can be used.”

Well, now (suddenly) there exists a method after all? The question is however, would it have worked across differentiated products and industries? Hardly. But again, case studies of industries might have worked, one could have analysed markups across firms in different zones, one could have looked at margins. And interestingly, an analysis that was done properly not showing any local domestic effects clearly would have implied that the export effects would have been even smaller.

Now they conclude their *analysis* of domestic competition distortion:

“Given our findings in chapter 4 and 7, our main conclusion when it comes to RDSSC’s impact on domestic competition is that it does enhance beneficiaries’ competitiveness. We find significant direct and indirect effects on employment in in eligible regions and increased profitability for firms. However, this is the objective of the scheme. We argue that RDSSC enhances competition between firms in Norway by lowering the costs of firms that initially had a competitive disadvantage.”

I have read chapter 4 in its new version. As I also wrote in the beginning, I am not convinced that they indeed find such strong and econometrically supported conclusions as they here (and elsewhere) claim, but as the (whole) rendered 'analysis' from subsection 8.3 shows – absolutely no *analysis* of distortion from competition is undertaken here.

Hence, the question of whether domestic competition is distorted is still unanswered.

Chapter 8, subsection 8.4 Impact on international competition

In this subsection the authors provide an analysis of basically two issues, shares of exporting firm across different zones and the size of the tax benefits across exporting firm.

The first part I think suffers from aggregation problems. It is not obvious why differences in the aggregated shares of exporting firms across zones tell us anything about potential distortion. When looking at the aggregate there are so many local factors that influence the shares that it is difficult to attribute differences to the social benefit scheme. Factors as closeness and availability to natural resources is clearly more important for why we see more fishing related industry and exporters in the North. Likewise, why we see salmon farming distributed around the coast and not the inland, and why oil centers have grown up only in some counties. When aggregating across all these industries you will not know whether it is the social security scheme or other factors that underlies the differences.

A significant more interesting approach would be to look at shares across zones within important export industries *one at a time*. If we then see patterns where the zones with the lowest tax (highest subsidies) is more prone to export, we might be worried that this stems from the social security differentiation. Thus, either the authors need to repeat the exercise on a substantially finer disaggregation level, eg., six digit NACE, or this part should be dropped. I will also recommend to estimate simple econometric models on differences in shares (dummy-models) across all industries to see whether differences are significant or not. Comparing average shares is not enough.

The second part of this subsection makes more sense to me. Here the authors try to look at the size of the potential problem, rather than trying to conclude on whether distortion towards international trade takes place or not. They look at how much industries that receives tax subsidies through the social security scheme actually receive per year. They show that this amount to a large extent is below the critical level of € 200 000 for a three year period (*de minimis aid*). They find that their defined large exporting firms receive a relatively low share of the subsidies (7% on average over the period 2004 to 2014), suggesting that the tax subsidies are mostly received by firms that produce for

the domestic market. They find that only in one out of eleven years (2011) the average subsidy was above the EU threshold. Hence, I think the exercise behind Figure 8.4 makes good sense, and shows that even if competition in international markets are influenced by the Norwegian social security scheme it is not significant enough to be seen as a problem by the EU.

Two qualifications are needed though, for some industries and firms, also other subsidies exist, for these I would anticipate that the EU requirement applies to all their annual subsidies, leaving less scope for the social security subsidy. Secondly, and here I have not access to the interpretation of the EU-*de minimis* requirement, but to me it is not obvious that the three-year cap is not binding. By binding I refer to the fact that an incidence of subsidies over a three-year span that is low enough is not considered problematic. However, subsidies that takes place indefinitely and far beyond three years might still be problematic to the EU.

Chapter 8 My conclusions and recommendations

Looking at Chapter 8 I find the following major challenges:

1. The chapter is very short, and I think that the topic of distortion and competition has been given far too little focus in the report.
2. There is absolutely no *analysis* of distortion to domestic competition.
3. The analysis of shares of exporters as it stands is impossible to judge due to the aggregation level, where the shares are aggregated across all exporting industries that are eligible for the social security benefits.

The problem with potential domestic competition distortion is probably less of a political challenge. As others, Norway choose to favor certain industries domestically, whereof the agriculture and food industry probably is the biggest receiver. This is a policy chosen by the government, and defines competition authorities' possibility to restrict practices in such industries and markets. However, I find it surprising that Samfunnsøkonomisk Analyse has made no real effort to analyze this issue.

The trade issue is more important since the EEA-treaty restrict the government's ability to freely adapt various discriminative subsidy schemes for the Norwegian industries. Also here, the analysis suffers, and I will strongly advise that a more disaggregated analysis of shares of exporters across individual industries is performed in the final version of the report. If no differences in favor of seeing more exports from the benefitting zones are found here, one might also argue that export is not significantly affected by the social security scheme.

Finally, my concerns on the interpretation of the EU's *de minimis* rule and how it is to be applied remain. I recommend that one has a competition-law expert look at the use of the *de minimis*

requirement and its interpretation in this report to make sure that the argument on the annual 'allowed size' of the subsidies, and which subsidies to consider, applies also for a longer period than three years.