

## **Evaluation of the Regionally Differentiated Social Security Contribution in Norway:** Comments on the draft report xx-2018

by prof.dr. Jan Oosterhaven, University of Groningen, April 2018

The more important comments are given first, chapter by chapter. Then comments on several details follow, page by page.

### **Summary**

There is one important conclusion in the report that I do not share and that is not substantiated by research done in the report itself, namely that RDSSC represents a societal cost (page VI, line 2) that is estimated at 0.25% of GDP (p. VIII, line 14). This conclusion is only based on one reference mentioned at p.108 (Rattsø, Stokke, 2017). The methodology of this study is not even summarized, as opposed to that of the comparable US study. Such a weak underpinning does not justify giving the conclusion of this one reference such a prominent place in the Summary. One might equally well argue that RDSSC represents a societal gain as I will do in this comment. Adding my alternative reasoning in Chapter 3 would provide for the underpinning of a more balanced Summary.

### **Ch.3 Theoretical framework**

The core of this chapter summarises the standard micro economic theory of individual firms' labour demand decisions and individual people's labour supply decisions, and shows how RDSSC influences the equilibrium on the labour market. Besides, it very briefly discusses on p.25 and p.30 how Norway's centralized wage negotiations positively influence the welfare gain of introducing RDSSC. This analysis is essentially correct, but unbalanced and incomplete.

As opposed to the summarized textbook case of a closed economy free market, *regional* labour demand is not simply the sum of existing firms' labour demand functions: in time new firms and in-migrating firms also contribute. On the supply side, in time inactive people becoming active, and growing in-commuting and in-migration have to be added to the summation of the supply functions of existing workers. The latter addition makes the emphasis in the text on the theoretical possibility of backward bending individual labour supply curves rather irrelevant.

Moreover, for a balanced exposition, Figure 3.3 should also be drawn with a nationally fixed regional wage level as is done for regions with a labour demand shortage at the going nationally fixed wage in my comment on the earlier RDSSC feasibility study (Oosterhaven, 2015). In that way, it may be clarified how actual Norwegian rural labour markets function somewhere in between these two extremes.

In fact, the exposition might become even more balanced and complete if the opposite case of a nationally fixed wage in urban regions with a supply shortage would also be added. Because, only in that way it can be made clear that a shift of labour demand from central regions with a supply shortage to peripheral regions with a demand shortage will result in a national growth of employment, i.e., in an national welfare gain of RDSSC, on top of the now

only briefly mentioned regional welfare gain (see Elhorst & Oosterhaven, 2008, for comparable effects in case of new transport infrastructure between regions with a labour supply and regions with a labour demand shortage). This theoretical possibility needs to be explained, along with an assessment of whether it applies RDSSC or not.

### **Ch.5 Empirical evidence**

In my comments on the feasibility study, I expressed strong doubts whether the difference-in-difference approach would be appropriate to estimate the impact of RDSSC. These doubts were based on the (implicit) assumption that the d-i-d approach would be applied on aggregate municipal data. The applications of d-i-d in ch.5, however, are done on micro (firm and person) data, and that makes my earlier critique irrelevant. At first reading this chapter, I still got the impression that not only individual firm and person control variables were needed, but also municipal control variables, such as, especially, a shift-and-share type of industry mix variable as done in Boersma & Oosterhaven (2009). At second reading, however, it became clear that adding municipal or industry level control variables is redundant in view of the already present municipal and industry fixed effects.

In sum, I appreciate the careful application of d-i-d in ch.5 and have only two comments on details later on.

### **Ch.6. The dynamics of regional population growth**

The reasoning why the impact of RDSSC on employment is analysed by means of the methods of ch. 5, whereas the impact of employment on population cannot be analysed in this way, is faulty. Multiple factors are involved in both cases. Labour demand is also influenced by multiple factors, such as product demand and technological change, while an increase in labour demand not necessarily leads to more employment. It may also lead to more unfilled vacancies if labour supply does not adapt, e.g., because of a nationally fixed wage that is too low for the region at hand. And how precisely labour supply adapts, such as by in-migration or by working more hours, co-determines the amount of job growth. Consequently, it is not clear why the d-i-d approach, of course with different control variables, has not been applied to population growth too.

In addition, Ch.6 is based on the assumption that the question whether *people follow jobs* or *jobs follow people* is crucial to the effectiveness of RDSSC in preventing population decline in rural Norway. The reasoning behind this assumption most clearly follows from p. 84, where it is stated that RDSSC is relevant when *people follow jobs*, whereas other measures, such as investments in amenities and individual (income and other) benefits, are relevant when *jobs follow people*. Chapter 5, however, clearly shows that not all of the reduced payroll tax is shifted to employers. Part of it is shifted to employees, leading to higher wages, which are relevant when *jobs follow people*. Hence, RDSSC works along both lines, which makes the extensive literature review in this chapter much less relevant.

### **Ch.7. Alternative measures**

The argumentation that alternative factor (capital, energy, research, land) cost reducing measures may have comparable output increasing effects, but no substitution effect and therefore do not represent a credible alternative to RDSSC, is convincing.

The income effect of RDSSC on regional household consumption, however, can be reached by a variety of other measures. The report discusses the different factors that influence the size of this effect for different measures, and comes to no clear conclusion. In this case, simulations with the PANDA model might give a much better indication than the present text.

When discussing the impact of investments in infrastructure, the text does not make the core distinction between intra-regional and interregional infrastructure, where a region needs to be defined at the scale of local labour and housing markets. Both types of infrastructure investments, of course, suffer from diminishing returns, which are reached at lower levels of expenditure in rural as compared to urban regions. Investing in intra-regional infrastructure unambiguously improves the attractiveness of a region for both firms and people. Investing in interregional infrastructure, however, basically makes both exports and imports cheaper, and thus has a much more ambiguous spatial impact (see Oosterhaven & Knaap, 2003).

#### **Ch.8. Ripple effects**

It is a pity that PANDA uses the common pool method of modelling interregional trade. This leads to an underestimation of the trade with nearby regions, which is relevant for the control regions, as they are mostly located next to the treatment regions. Instead of less than 1 %point, which is already more than the wrongly mentioned less than 1% on p.107, my guestimate of the spill-overs to the control regions would be 1-3%point.

#### **Ch.9. Distortive effects**

This second important chapter starts with one big, but hidden assumption, namely that the world without RDSSC is perfect, i.e., without any market distortion. Alas, the world is not perfect, as is convincingly shown in Figure 3.3, which clearly points out that a decrease in payroll taxes leads to a reduction in deadweight loss, i.e., to an increase in regional welfare (on top of which there also may be an increase in national welfare as I have argued earlier). Unfortunately, this important conclusion of ch.3 is forgotten in ch.9.

The only objection one can have against the analysis in Figure 3.3, in this context, is that it is a partial equilibrium analysis of the labour market only, and thus disregards possible interaction with other markets. The most important other market in this respect is the capital market. In Norway, as well as in almost all other countries, capital is taxed much, much less than labour. Countries internationally compete for very mobile foreign direct investments by means of low capital taxes, while firms lower the existing capital taxes even more by using tax havens and engaging in tax evasion. As to labour the opposite happens. Being a relatively immobile factor of production it is taxed heavily, while most countries make this factor even more immobile by having high barriers to foreign in-migration.

Consequently, the most important international and thus also national tax distortion is the relative over-taxation of labour and the relative under-taxation of capital. Adding this major market distortion to the partial equilibrium analysis of Figure 3.3 would represent the single

most relevant extension in the direction of a general equilibrium analysis of the welfare impact of RDSSC. Without a spatial CGE model with both labour and capital taxes and factor mobility or lack thereof, it is not possible to put numbers to what should be the obvious conclusion of ch.9, namely that RDSSC reduces the two above mentioned market imperfections, and thus represents an unambiguous welfare improvement.

The interpretation of high value added/sales ratios as indicative of the impact of RDSSC on competition and trade does not make sense. This ratio primarily is indicative of the non-roundaboutness of production, i.e., of using few externally produced inputs per unit of output. Secondly, it may be an indication of low labour productivity, i.e., of using more labour per unit of output, as labour roughly comprises 2/3 of value added. Returns to capital only constitute about 1/3 of value added, while profits only represents an unknow small percentage of the returns to capital. And finally, as noted on p.112, profits per unit of output may be high because of lack of local competition in non-tradeables, which will typically occur in RDSSC municipalities. In sum, the current interpretation of the VA/sales ratio in the text does not make sense.

**Ch.10 Concluding remarks and recommendations**

Here, and repeated in the **Summary**, the clear discrepancy between the two major conclusions of the draft report becomes most evident. On the one hand a clear employment effect is reported, supported by extensive econometric work in ch.5, and on the other hand no measurable effect on trade and competition is reported, with weak, mainly verbal support in ch.9. One can only have both substantial employment effects and a zero impact on competition and trade, if all of the employment effect only occurs in local non-tradeable services, which is unlikely and which is not shown to be the case in the draft report either.

I believe the conclusion on the positive impact on employment is by far the most plausible of the two main conclusions, which implies that there must also be effects on competition and trade. However, opposed to the draft report, for reasons indicated earlier, I believe that the effects on competition and trade represent a welfare gain and not a welfare loss.

=====

**Comments on details**

1. P.13, p.25, p.112, and some pages in between. RDSSC is not a *labour subsidy*, it is not a *tax*, it is a *labour tax reduction*.
2. Page 15 and p.25. Under full competition, prices of tradeable products are *not* equal everywhere. Since there are economies of scale in practically every line of production there will be locations of production that differ from the locations of consumption and thus prices will differ depending, inter alia, on unit transportation cost and distances to production locations.
3. Figure 3.2. Point C is unreachable as it is outside the new budget constraint XY<sub>2</sub>. The substitution effect should be shown on the lower IC<sub>1</sub>.
4. Second paragraph of Section 3.4 and second paragraph on p.33. The reasoning that the bargained wages, over time, counteract the effect of a reduced tax rate is derived from

research at the national level. Under national wage bargaining it hardly applies to the regional level. It should be clarified that this effect is theoretically expected to be small instead of large.

5. The, against theoretical prediction, positive impact in the last period 2006 in Zone 4 in Table 5.13 is not discussed, despite its being highly significant. According to Table 5.14, this strange result originates from Type 3 firms. An explanation is required.
6. P.68. The paragraph with the words *spells*, *bin* and *bootstrapped* is incomprehensible, as definitions are lacking.
7. P.80, last paragraph. Population *growth* should be changed in population *density*, as follows from the text as well as from the literature (see Oosterhaven, 2010).
8. P.87, first par. The stated difference in correlation coefficients in case of *place of residence* versus *place of work* are inconsistent with the  $R^2$  in Figure 6.2 and 6.3. Furthermore, note that the later are more supportive to the *jobs follow people* case, whereas the numbers mentioned in the text are more supportive to the reverse case. The correct version of the two possible conclusions should be added to the text.
9. P.107. Using the data from Figure 8.1, my calculation of the indirect-to-direct effect ratio is 58.5%  $(100.0-63.1)/63.1$ . The origin of the 37.1% in the text is not clear.
10. P.114. Nowhere it has been shown that RDSSC lowers the price of labour below market wages, as this report does not contain an analysis of what local wages would have been in absence of national wage bargaining.
11. The quality of the English used differs considerably from chapter to chapter. Also, there are many repetitions of arguments, both within chapters and between chapters, while the numbering of tables and figures between text and table and figure titles is not always consistent. A final version of the report needs a considerable editing job.

## References

- Boersma, L. & J. Oosterhaven (2009) Regional Labour Productivity in The Netherlands, Evidence of Agglomeration and Congestion. *Journal of Regional Science* 49/3, pp. 483–511.
- Elhorst, J.P., J. Oosterhaven, F.J. Sijtsma & T.M. Stelder (1999) Welfare Effects of Spatial Deconcentration: A Scenario Study for the Netherlands. *Tijdschrift voor Economische en Sociale Geografie* 90/1, pp. 17-31.
- Elhorst, J.P. & J. Oosterhaven (2008) Integral cost-benefit analysis of Maglev projects under market imperfections. *Journal of Transport and Land Use* 1/1, pp. 65-87.
- Oosterhaven, J. & T. Knaap (2003) Spatial Economic Impacts of Transport Infrastructure Investments, in: Pearman, Mackie & Nellthorp (eds) *Transport Projects, Programmes and Policies: Evaluation Needs and Capabilities*, Ashgate, Aldershot, pp. 87-105.
- Oosterhaven, J. (2010) *Concentratie versus Spreiding: Over oude concepten, nieuwe theorie en krimp*. Valedictory lecture, University of Groningen.
- Oosterhaven, J. (2015) Evaluation of the Regionally Differentiated Social Security Contribution scheme in Norway: Comments on the feasibility study. Note for the Norwegian Ministry of Lower Governments and Modernisation.