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Gambling with the family silver

Household consumption and saving responses to fiscal uncertainty

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Abstract:
In the early 2000s, eight Norwegian energy producing municipalities sold up to ten years of future electricity earnings and let two brokers from Terra Securities make investments on their behalf. In the wake of the 2007 credit crash the municipalities lost up to 80 percent of their assets. This paper uses a difference in difference analysis to show that this tightening of the local government budget, and accompanying uncertainty about future economic outcomes, led to a reduction in private consumption of around 2 percent in 2008. I show that the response is driven by households who are the largest recipients of public services - the young and the elderly. The reduction in consumption is a result of households saving more, and not a direct consequence of changes in their disposable income. I also find that households in the affected municipalities rebalance their portfolios to holding a lower share of risky assets. The results are interpreted as households holding back consumption, and reallocating towards safer assets, until uncertainty regarding fiscal outcomes is resolved.

Keywords: Fiscal uncertainty; consumption and saving; panel data; natural experiment

JEL classification: D12; E21; E65; H31

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Sammendrag

På tidlig 2000-tallet gikk åtte norske vannkraftskommuner med på en investeringsplan med to meglere fra Terra Securities. Meglerne administrerte store investeringer for kommunene, og under kredittkrisen i 2007 tapte de opp mot 80 prosent av de investerte beløpene. Disse tapene kom som en overraskelse på både politikerne og den lokale befolkningen. Siden tapene etter norsk lov måtte dekkes innen fire år, var det gjennom 2008 stor usikkerhet rundt hva konsekvensene ville bli for tjenestetilbud, jobber og videre drift i de rammede kommunene. Mot slutten av 2008 fikk de hardest rammede kommunene innvilget ekstrardinære skjønnsmidler for å kunne opprettholde det lovlig pålagte nivået på tjensteytingen. Dette markerte på mange måter slutten på perioden med usikkerhet, siden det ble etablert at staten ville komme kommunene til unnsetning og ikke tillate for store negative konsekvenser av hendelsen.

Artikkelen studerer hvordan husholdninger tilpasset sitt konsum under perioden hvor den finansielle framtiden til kommunen var preget av usikkerhet. Fra registerdata aggregert til familienivå benyttes informasjon om inntekt og endringer i fomue til å imputere konsum for husholdninger. Ved å benytte et difference-in-difference oppsett, hvor jeg sammenligner husholdninger i de rammede kommunene med husholdninger fra andre vannkraftskommuner med like finansielle- og populasjonskarakteristikker (basert på KOSTRA-klassifisering), finner jeg at det private konsumet i de hardest rammede kommunene falt med om lag 2 prosent. I de tre kommunene som tapte penger, men enkelt klarte å dekke tapene uten å havne under statlig administrasjon eller be om ekstraordinære skjønnsmidler, er det ingen signifikant effekt på konsumet.

Ved å studere kategorier av finansielle eiendeler og inntekt, viser jeg at det er økt sparing, og ikke redusert inntekt, som genererer det reduserte konsumet. Dette er i tråd med tidligere funn av Aaberge, Liu og Zhu (2016) og Giavazzi og McMahon (2012), som studerer lignende hendelser.

Funnene har tydelige implikasjoner for økonomisk politikk i perioder med økt usikkerhet og lavt konsum. Dersom konsumet, og dermed den generelle økonomiske aktiviteten, er lav i en nedgangsperiode preget av usikkerhet, bør myndighetene gjøre det de kan for å skape forutsigbarhet og trygghet. Økonomiske stimulansepakker kan derimot være ineffektive om usikkerheten er høy, da disse vil ha mindre gjennomslag i totalkonsum som følge av at konsumtilbøyeligheten er lav.

1 Introduction

A growing literature considers how uncertainty, through its effect on household decision making, can be important for understanding macroeconomic fluctuations (see for example Bloom (2009), Baker, Bloom, and Davis (2015), Caldara, Fuentes-Albero, Gilchrist, and Zakrajšek (2016) and Basu and Bundick (2017)). Much of this research was further motivated by the slow recovery after the Great Recession in 2008, where uncertainty has been proposed as an explanation for the cautious behaviour by firms and households, despite policies aimed at stimulating the economy. While it is well understood why uncertainty could cause households to reduce consumption in theoretical models of household optimisation, the body of empirical research documenting how uncertainty affects household behaviour at the micro level is small.

One reason could be that uncertainty is an abstract concept difficult to quantify in a way that is suitable for empirical testing. While several quantification methods exist, these typically result in aggregate measures that is unsuitable for identifying a causal mechanism on the micro level.[1] Natural experiments involving uncertainty could overcome these issues, but events that are unexpected and exogenous, are rare.[2] In addition, the availability of data at the household level that contains reliable measures of consumption and saving is often restricted.

This paper combines unique household level register data with an unexpected and exogenous event to document effects of uncertainty on consumption and saving. The data stems from the Norwegian tax register data base, and allows household level consumption expenditures to be imputed at a yearly basis. The economic uncertainty comes from a financial shock to eight municipalities in Norway, who lost a significant amount of financial assets during 2007. It is well documented that these investments, and their high-risk profiles, were unknown to current local government electives and inhabitants (Hofstad, 2008).[3] Therefore, the event was surprising to the households and independent of their individual characteristics, except for place of residence. According to the Norwegian law, the deficits these losses incurred had to be covered within four years. This meant that households living in the worst affected municipalities had reason to fear that there would be significant cuts to public spending, which could result in fewer jobs and a

[1] Uncertainty indexes and variance parameters of income process are examples of such measures.

[2] Fuchs-Schündeln and Hassan (2016) provides an overview of how natural experiments has been a useful method for establishing causal mechanisms in the macroeconomic context.

[3] They had been made in the early 2000s and were subsequently managed in full by an external brokerage.

deterioration of the publicly provided services.[4] Towards the end of 2008, much of this uncertainty was resolved when the worst affected municipalities where bailed out receiving extraordinary transfers from the central government. In addition, they received a prolonged deadline to be able to smooth the impact from the deficit over several years.

To estimate the effect of this temporary uncertainty shock, I use a difference in difference approach where households residing in similar power producing municipalities represent the control group. I separate the analysis between the group of affected municipalities that struggled to cover their deficit with the group of municipalities that covered the loss relatively easily.

I find that inhabitants in the five worst affected municipalities reduced their consumption by 1.8 percent in 2008. I interpret the effect as being driven by the uncertainty surrounding the potential consequences the loss of assets would have for future economic outcomes in the municipality. This is supported by the fact that in the three municipalities who experienced losses, but managed to cover their deficits without help from the government, there is no significant effect on consumption. Since these municipalities where able to cover their deficit relatively easy, they had less reason to fear any real consequences for the public provision of jobs and other goods and services.

To further support this interpretation, I estimate the effect for different age groups. The effect is driven by the young and elderly, which are the largest recipients of public services provided by municipalities. Although not conclusive, the pattern is consistent with the fact that these groups where the ones that were most likely to be affected by changes to public services.

I further document how households respond to an uncertainty shock by studying changes in asset classes. The effect comes from increased saving in financial assets, meaning households spend less from their income than their comparison group. I find no significant effect on disposable income among the worst hit municipalities. Therefore, the observed result is likely to come from changes in households' decision on how much to spend from a given income, and not a mechanical effect from a reduction in disposable income. This is similar to the findings of Giavazzi and McMahon (2012) and Aaberge, Liu, and Zhu (2016), who both find that episodes of political uncertainty induce households to save more.[5]

Finally, I follow the literature on portfolio choice and background risk to study whether house-

[4]This is not the first paper to use this event as an instrument for uncertainty. Bratberg and Monstad (2015) use this event to study effects on sick leave from reduced job security.

[5]See for example Carroll and Kimball (2006) for a survey on the related precautionary saving literature.

holds change their share of risky assets in response to increased uncertainty (see for example Heaton and Lucas (2000), Palia, Qi, and Wu (2014) and Fagereng, Guiso, and Pistaferri (2017)). I find that households holding risky assets rebalance their portfolios after the event, holding a lower risky share one year later. Like the findings on consumption expenditures, this effect is only present in the worst affected municipalities.

Since the loss of publicly owned assets may carry other effects than uncertainty, alternative explanations for the observed results are considered. For example, a large literature considers the relationship between public spending and private consumption and whether these are complements or substitutes.[6] It is tempting to think that the tightening of the local government budget constraint considered in this paper was a reduction in public services, and that the observed effect is informative of the relationship between private and public spending. Using municipality income statements, I show that public spending falls permanently over the period of analysis. If the changes in private spending were driven by actual changes in public spending, the decrease in private consumption should have been permanent as well, however, the effect disappears after the first year has passed, when the municipalities where bailed out. I therefore argue that the fiscal actions of the local governments cannot be the sole driver of the observed consumption drop. Considering other alternative consequences of the events that could have influenced the results, I show that there is no effect on moving, employment or disposable income. Considering the results all together, my interpretation is that households are holding back consumption until uncertainty regarding fiscal outcomes is resolved.

This paper builds on the growing literature aimed at establishing causal effects in macroeconomics through the use of natural experiments and micro data (see Fuchs-Schündeln and Hassan (2016) for a review). While several studies have investigated how consumption is affected by income dispersion and uncertainty indices using aggregate data, only a few studies have been able to look at the effect of uncertainty on consumption at the micro level. A notable exception is Giavazzi and McMahon (2012), who use a survey measure of subjective uncertainty to show that uncertainty related to the 1998 political election in Germany induced affected households to save and work more. Another example is Aaberge, Liu, and Zhu (2016) who study an event involving

[6]Examples include Blanchard and Perotti (2002), Perotti (2007), Mountford and Uhlig (2009), Fatás, Mihov, et al. (2001), Galí, López-Salido, and Vallés (2007) using VAR-techniques. Ramey and Shapiro (1998) and Giavazzi and Pagano (1990) uses a narrative approach.

political uncertainty and reforms in China and find that households increase their saving when uncertainty is high. Although uncertainty (risk) in economics often refers to a mean preserving spread in the variance of a stochastic variable, the real world situation economic agents face often involves reduced ability to foresee the set of potential future outcomes, in combination with a widening of the plausible outcome space.[7] In that regard, the current paper contributes important insights that are highly relevant for the policy conducted in situations where the future outcomes become less clear. First, the findings in this paper document that uncertainty related to the economic status of public institutions affect choices made by private households on how much to consume and save from a given income. When uncertainty is resolved, consumption rises again, showing that providing security against worst case scenarios is important for economic activity during turbulent times. This is in many ways similar to the findings by Aaberge and Langørgen (2003) and Giavazzi and McMahon (2012) studying political uncertainty, however, the event studied in this paper has a stronger flavour of surprise, in addition to being a purely financial shock to the local government. In combination with the detailed third party reported data for the full population, the paper provides unique insights in how households react to uncertainty, from total consumption to underlying asset allocation choices.

The paper proceeds as follows: Section 2 contains the narrative of the events, and describe how this is interpreted as an uncertainty shock. Section 3 explains the data and imputation process, section 4 the empirical strategy. In section 5 I show the main results, and decompose the effect. I summarize and conclude in section 6.

2 A natural experiment: "The Terra scandal"

Understanding what type of event this is, and what consequences it carried for the involved municipalities, is essential when we want to understand the effects it had on private consumption. This section concerns how the loss of public assets caused uncertainty for households living in the affected municipalities. I give a brief story of how the investments came about, the shock that arrived when these suddenly fell in value and had to be sold with large losses, and what consequences this carried for public finances, and ultimately households, in the affected municipalities.

[7] See for example the discussion of concepts of uncertainty in economics in Dequech (2000).

In the early 2000s, the electricity price in Norway was low, making hydro-electric power producing municipalities seek ways of increasing returns on their asset. This led two brokers from Terra Securities to come up with a scheme where municipalities could sell future power production at a fixed price today, and invest these funds in the stock market to achieve higher returns. Initially, the brokers approached all 174 hydro-electric power producing municipalities in Norway through their member organization (NVE). A warning against such investments due to the involved risk and the fact that they suspected it would be illegal according to the law for municipalities, was later issued by NVE. Despite these warnings, eight municipalities took up loans against future income between 2001 and 2004, and let the brokers from Terra Securities make investments on their behalf.

In the fall of 2007, some of the municipalities unexpectedly received payment claims. The U.S. credit crisis had reduced the value on assets purchased by the brokers from Terra Securities, and the municipalities were required to put up additional funds to guarantee these positions. Since the brokers had managed the investments on behalf of the municipalities, local government officials had very little knowledge about what type of assets they owned, not to mention their risk profile and clauses involving guarantee payments. Thus, they immediately started internal investigations to get an overview of what they owned and what it was worth. It soon became clear that sizeable investments would be lost, and additional payment claims could be expected to arrive. On October 31st, 2007 the story became national news when a national newspaper published an article about the investments and their potential losses. A public investigation of the investment scheme was initiated and concluded that it was illegal. As a consequence, Terra Securities lost their licence to deliver financial services in Norway and declared themselves bankrupt on November 28th, 2007. The municipalities were now left to tidy up on their own. By early summer of 2008 almost all assets had been sold, incurring large deficits.

According to the Norwegian law, municipalities must cover budget deficits within a period of four years. This meant that unless the municipality had savings to cover its losses, it would be obliged to cut running expenses or find ways of increasing income. February 3rd 2008, the Minister of Local Governments publicly stated that the municipalities would not be bailed out by the central government unless they could substantiate that they would not be able to uphold the legally required level of public services. Local processes to assess where cuts to the local

government budget could be made, and what additional income could be generated, started early in 2008. In three of the municipalities (Haugesund, Kvinesdal and Rana), it was clear that the deficits could be covered without large consequences, partly because they had accessible funds they could drain, partly because the final loss ended up being relatively small compared to their budget. In the five worst hit (Bremanger, Hattfjelldal, Hemnes, Narvik and Vik), the situation was more severe. Public records and news stories show that there was a wide range of suggested actions, ranging from removal of public services that where not legally required, merging schools, stalling maintenance on buildings and roads, to introducing private property taxation.

On November 27th 2008, after almost a year of uncertainty and turmoil, the Minister of Local Governments reassessed the situation and granted Vik, Bremanger and Hattfjelldal extraordinary cash transfers. She also more than doubled their period of time to cover the deficits. In the press statement, the Minister expressed that it could be necessary to provide new cash transfers in 2009, and that she expected to receive an application from Narvik. In October 2009, Narvik received extraordinary transfers and extra time to cover their losses. Hemnes did not receive extraordinary transfers, but where immediately put under state supervision in 2008.

The extraordinary transfers marks an end to much of the uncertainty introduced by the financial losses. While there still were cuts to be made, the government transfers were a strong signal that the municipalities would get help if needed. In that way, the bailout removed worst case scenarios and transferred the crisis-like situation to a milder and more predictable period of financial austerity.

3 Data

The data comes from the Norwegian Tax Register, containing information on households' balance sheets in the period from 1994-2010. Households in Norway are subject to income and wealth tax, and are therefore required to report their wealth holdings and income to the tax authorities. This information is not self reported, but comes from third parties such as employers, banks, financial intermediaries and insurance companies, covering their complete wealth holdings. The data is therefore not subject to personal reporting and has the advantage of being precisely measured. The data covers taxable financial assets over time and can be decomposed into different categories. Furthermore, through personal identifiers, the income and wealth registers can be linked to other

registers with information on personal characteristics, such as place of residence, age, education, family status, number of children and immigration background.

The data is produced making use of family identifiers from the population register to aggregate income and wealth to family level. Based on the address register, I keep households living in municipalities that are on the membership list of the National Association of Hydro-electricity in 2008.

To construct treatment and control groups, Kostra-code is employed. This is a grouping of municipalities based on population and financial status. The coding is updated approximately every five years. I use the closest available update, which is 2008. One might worry that the treated municipalities are affected by the shock in 2007 in a way that moves them to another Kostra-group in 2008. This seems to be the case for Bremanger, however, given that many of the potential control municipalities also change code from 2003 to 2008, I stick with the 2008 version to have the most relevant classification for the year of treatment.

Finally, I use the annual reports and income statements of the municipalities, containing information on income, expenses and financial transactions broken down by categories, for the periods 2003-2012.

Imputing consumption from the administrative data

The method of imputing consumption from administrative tax records was pioneered by Browning and Leth-Petersen (2003) and is well documented and tested (see Fagereng and Halvorsen (2017) and Eika, Mogstad, and Vestad (2017) for details on the method using Norwegian data[8]). The method has several advantages compared to using survey data, as expenditures are objectively measured and the sample covers the whole population. In the current application this is extremely important, as the number of households affected by the Terra-scandal is relatively small compared to the whole population.

The consumption expenditures are imputed by exploiting the budget constraint of households:

$$A_t = (1 + r)A_{t-1} + Y_t - C_t$$

[8] Examples of applications with the Norwegian data are Kostøl and Mogstad (2015) and Fagereng, Holm, and Natvik (2016)

where the observed assets at time t, A_t, is the assets at the start of the period with interest, $(1 + r)A_{t-1}$, plus disposable income, Y_t, minus consumption expenditures, C_t. Rewriting, we see that the yearly consumption expenditures are simply the disposable income minus the change in the stock of assets and their returns:

$$C_t = Y_t - \Delta A_t + rA_{t-1} \tag{1}$$

This means that the consumption expenditure is everything the household does not actively save from their disposable income.

Implementing the imputation strategy is not completely straight forward. First, if we are not able to separate changes in A_t stemming from an active decision to save more of the income, from passive returns to existing assets rA_{t-1}, consumption will be underestimated in the case of large returns and vice versa. A case where this is problematic is stocks. Since we only observe the total value of stock-holdings at the end of the year, we do not know if an increase is due to the household buying more stocks, or if it is simply unrealized returns as a consequence of price changes in the stock market.

Another issue is the value of houses and other real estate. Since houses are not sold often, obtaining reliable estimates of their current market values can be difficult. In Norway, houses are registered in the tax records with a rough estimate that is meant to resemble about 25 percent of its market value. However, these market values tend to be imprecisely measured. Therefore, years with housing transactions are problematic, as the debt used to finance the purchase does not match the asset side of the equation, and we tend to get large and unrepresentative observations of the consumption expenditure. Since we want unrealized returns removed from the equation, the common remedy is to remove observations where households make housing transactions.

Finally, due to the aggregation to family level, the imputation is sensitive to changes in family composition. These changes can to some extent be controlled for in the regressions, but may nonetheless create large variation in the consumption measure over time.

All of the issues mentioned above may lead to over- or underestimation of consumption. However, since I operate in a difference in difference environment, bias from measurement error will drive my estimates towards zero, unless it somehow correlates with the event studied. Removal

of observations where housing transactions are performed might seem worrisome in that regard. Therefore, using publicly available population data, I show that there is no significant effect on moving in the affected municipalities in Figure 4.

Additional noise might reduce the ability to measure small effects. I therefore follow Fagereng, Holm, and Natvik (2016) and make exclusions to create a sample not suffering from extreme observations to obtain reasonable precision of my estimates. They show that the qualitative results are the same when these are included, however the estimates have larger standard errors, likely as a consequence of measurement error. The exclusions include removing negative imputed consumption values and removing the lowest and largest percentile.

Table 1 shows descriptive statistics of the dependent and control variables of the final samples employed, disaggregated for the applied definitions of treatment and control groups. The control and treatment groups look similar in these observable dimensions.

Table 1: Descriptive statistics

Variable	Mean		Stdev		Min		Max		Obs	
	Control	Terra	Control	Terra	Control	Terra	Control	Terra	Control	Terra
					The five hit					
Log Consumption	11.94	11.98	.70	.71	2.31	3.15	15.62	15.46	1 012 260	126 353
Consumption to income	1.13	1.13	.61	.62	.168	.169	6.13	6.09	992 317	123 708
Consumption to lagged income	1.11	1.11	.536	.543	.171	.173	5.89	5.86	639 495	78 154
Saving in debt to income*	-.055	-.067	.394	.485	-4.03	-4.04	.902	.877	994 449	123 655
Saving in financial assets to income*	-.126	-.136	.607	.619	-5.12	-5.08	.831	.830	992 317	123 708
Conditional Risky share	.256	.236	.263	.256	1.43e-06	1.20e-06	1	1	236 131	24 649
Log Disposable Income	11.91	11.95	.558	.454	2.30	2.43	15.62	15.46	1 012 260	126 353
Age	56.22	55.60	19.47	19.04	25	25	90	90	1 012 260	126 353
Education Length	3.33	3.46	1.57	1.61	0	0	9	9	1 012 260	126 353
Children	.28	.28	.73	.71	0	0	10	7	1 012 260	126 353
Family Size	1.83	1.79	1.19	1.15	1	1	12	14	1 012 260	126 353
Male	.46	.46	.498	.498	0	0	1	1	1 012 260	126 353
					The three unaffected					
Log Consumption	12.03	12.02	.716	.727	2.511	2.344	15.77	15.97	685 847	247 673
Consumption to income	1.13	1.14	.62	.63	.169	.169	6.13	6.13	671 922	242 758
Consumption to lagged income	1.11	1.12	.547	.549	.171	.171	5.89	5.90	416 459	151 600
Saving in debt to income	-.068	-.070	.448	.548	-4.04	-4.03	.90	.90	670 424	242 177
Saving in financial assets to income	-.134	-.142	.623	.628	-5.12	-5.12	.831	.831	671 922	242 758
Conditional Risky share	.282	.281	.274	.272	1.22e-06	3.60e-06	1	1	173 671	61 805
Log Disposable Income	12.00	11.99	.558	.588	2.30	2.31	15.77	15.98	685 847	247 673
Age	52.82	53.42	18.60	18.84	25	25	90	90	685 847	247 673
Education Length	3.55	3.59	1.66	1.67	0	0	9	9	685 847	247 673
Children	.319	.305	.750	.741	0	0	11	8	685 847	247 673
Family Size	1.82	1.75	1.16	1.14	1	1	14	10	685 847	247 673
Male	.461	.460	.498	.498	0	0	1	1	685 847	247 673

4 Empirical strategy

Treatment and control groups

I estimate the effect using a difference in difference set-up where the treatment group is the municipalities that experienced losses. Important for identification is the parallel trend assumption, namely that the control and treatment group would have behaved similarly if it were not for the treatment. The control group is constructed from the remaining Norwegian population following two steps: First, I follow Bratberg and Monstad (2015), who used the same instrument to study effects on sick leave, and pick from the pool of households living in a municipality with income from hydro-power plants. 173 out of 426 municipalities had income from hydro-power plants in 2008. This step ensures that I compare households living in a municipality that finances some goods and services from power plant income that in principle could have been invested and lost in the financial market.[9] The second step ensures that the control group consists of households living in municipalities that have similar population size and economic conditions. To achieve this, I use the Kostra-classification, which is an official grouping of municipalities based on population size and economic status.[10] The classification is provided to municipalities by Statistics Norway and used as a tool to contrast economic performance and population growth, which seems particularly suitable in the context of this natural experiment.[11]

Five hit and three unaffected

Based on readings of the events, and the information in Table 2, there are strong indications that only five of the eight municipalities where affected in a way that increased uncertainty related to future fiscal outcomes. To avoid having municipalities in the treatment sample where the loss carried virtually no fear of economic consequences, I divide the analysis between "the five hit" and "the three unaffected" municipalities. "The five hit" are Bremanger, Hattfjelldal, Hemnes, Narvik and Vik. Haugesund, Kvinesdal and Rana are labeled "the three unaffected". The three unaffected all managed to cover their losses relatively easily. Rana because of their large income

[9] Another way of viewing it, is that I exclude all households that never had a positive probability of being treated.

[10] See Aaberge and Langørgen (2011) for a description (in Norwegian)

[11] An alternative could be to perform matching, however, the large amount of data, and set of possible matching techniques, seem lees transparent than the current set-up.

Table 2: Investment, loss and population stats

Municipality	Investment	Loss	Population	Loss per capita	Under state supervision (ROBEK)	Extraordinary transfers	Kostra-group
Rana	297	222.5	25 124	8 900	-	No	13
Hattfjelldal	103	85	1 472	57 750	2008	Yes	6
Hemnes	84	78.3	4 500	17 400	2008	No	3
Narvik	242	188.3	18 391	10 240	2009	Yes	11
Vik	149	90	2 816	32 000	2007	Yes	3
Bremanger	350	217.7	3 903	55 700	2008	Yes	5
Haugesund	227	130	32 761	3 970	2010	No	13
Kvinesdal	43	18	5 622	3 200	-	No	12

Investment and loss are given in mill NOK and comes from Hofstad (2008). The conversion to dollar at the time was approximately 5.8. Population was measured in the fourth quarter of 2007.
ROBEK - Register for Governmental Approval of Financial Obligations. For any registered municipality, either the County Governor or the Ministry is to review the legality of the budget resolution passed by the municipal council or the county council, in addition to loan and financial leasing and long term rental contracts.

stream, Kvinesdal because of small exposure and large income, Haugesund as a combination of the two. On the other hand, the five hit all experienced larger losses per capita in addition to being put under state supervision shortly after 2007.[12] Table 2 shows the amount invested and lost across municipalities, both in absolute terms and per capita. There is large variation, reflecting that municipalities vary in their population and financial situation.[13]

Being put under state supervision means that either the County Governor or the Ministry has to review the legality of the budget resolution passed by the municipal council or the county council, in addition to new loans, financial leasing and long term rental contracts. Being under supervision decreases economic freedom, and is either a consequence of running deficits, or simply based on a general assessment of the economic state of the municipality. Between 2001 and 2017, between 42 and 118 out of Norway's 424 municipalities were under supervision. As a fairly common occurrence, I argue that it should not be used as an indicator of economic turbulence independently. On the contrary, the fact that three of the eight affected municipalities were able to avoid getting on the list, is a strong indication of them being largely unaffected.

In addition to being put under state supervision, four of the municipalities received extraordinary transfers from the central Government to be able to uphold their legally required service level. This confirms that some municipalities had real economic struggles as a consequence of the

[12] Vik was already under supervision two months before the news became public.

[13] Since the hydroelectric power plants may range over several municipalities, some of them have shared ownership and split their taxes and fees between these. The fees are set by the authorities and paid separately by each plant, making it complicated to provide an overview of the income from hydroelectric power plants by municipality.

losses. While these transfers reduced the economic importance of the event, they were not granted before mid November 2008, leaving households in a state of uncertainty regarding the economic future of the municipality for most of 2008.

Empirical specification

Throughout the rest of the paper, I use the following specification when estimating the effect the Terra-scandal had on household behaviour:

$$ln(c_{it}) = \alpha_0 + \sum_{j=1995}^{2010} \theta_j year_j + \delta_0 T_{it} + \delta_1 T_{it} * D_{2008} + \delta_2 T_{it} * D_{2009} + \delta_3 T_{it} * D_{2010} + X_{it}\beta + \gamma M_j + \varepsilon_{it} \quad (2)$$

where α_0 is the intercept, θ_j is a set of yearly effects, T_{it} is an indicator being one if the individual lives in an affected municipality and 0 otherwise. $T_{it} * D_{2008-2010}$ are interaction variables for estimating the yearly treatment effects in the periods after the news emerged. The treatment effect is given by δ_{1-3}. X_{it} is a set of control variables containing observable characteristics of the household, γM_j is a vector of municipality fixed effects and ε_{it} is the error term. In the baseline specification, controls include indicator variables for education type[14], education length, age and family size as well as the log of disposable family income.

I also estimate the same specification, but instead of assuming parallel trends, I include interaction effects from 2003-2010, to give a better illustration of the parallel trend assumption.

In the robustness section (Appendix 7.1), I also allow for differing linear trends between the treatment and control group. I vary the set of control variables, and perform placebo tests, estimating 1-year effects to assess whether significant differences occur when there should be none. Following Cameron and Miller (2015), I cluster all standard errors at the municipality level, since this is the level at which the treatment is assigned.[15]

[14] A 10-category variable based on the Norwegian Educational standard.
[15] This increases the standard errors compared to clustering on the individual level.

Table 3: Interaction effects on log consumption

	The five hit	*The three unaffected*
T * D2008	-.0179***	-.00432
	(.00443)	(.00896)
T * D2009	-.00310	-.00595
	(.0061)	(.0211)
T * D2010	-.00904	.00290
	(.0093)	(.0154)
Observations	1 138 613	933 520

Note: Control variables: Log disposable income, age indicators, education length- and type fixed effect, municipality fixed effect, year fixed effect, immigration category and household size.
Cluster robust standard errors on treatment level (municipality)
* p<0.05, ** p<0.01, *** p<0.001

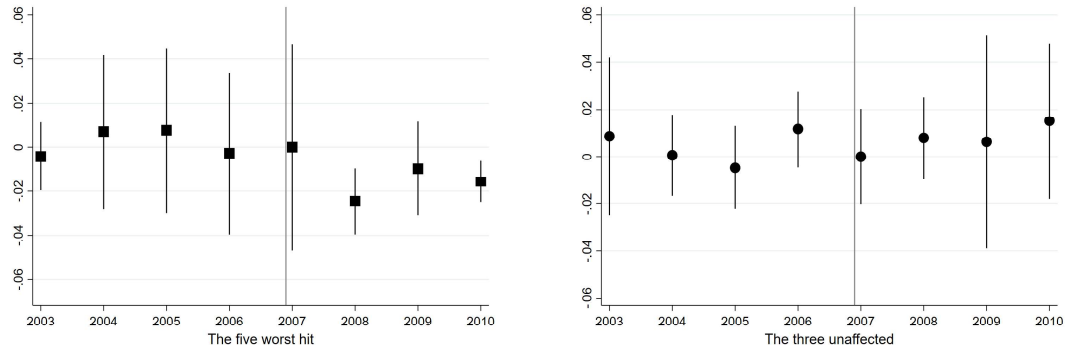
5 Results

Table 3 shows the treatment effects from the difference in difference estimation. The first column shows the effect in the five hit, and the second shows the effect in the three unaffected. There is a 1.79 percent decrease in consumption in the five worst hit municipalities in 2008, however, there is no significant effect in 2009 and 2010. In the three unaffected municipalities, we see no significant effect, with point estimates no larger than 0.6 percent.[16] To assess the economic significance of the results, consider that the mean consumption in the five worst affected municipalities was 265 000 NOK in 2007, meaning the consumption reduction was 5000 NOK ($710) per capita.[17]

To assess the parallel trend assumption, I re-estimate equation (2) and include interaction terms for the pre-treatment periods 2003-2007. I plot the interaction coefficients and their 95 percent confidence intervals in Figure 1, normalizing the interaction effect in 2007 to 0. We see that in the periods leading up to the event, consumption was slightly higher than the average difference between the treatment and control group between 2003 and 2007. In 2008, two things happened; There was a distinct fall in consumption compared to 2007. Second, the precision of the estimate increases. The reduction of noise could in principle be sampling issues, but it could also be a

[16]Some might worry that the difference in precision comes from differing number of clusters. The same conclusions apply when clustering on the individual level.
[17]The conversion to USD was 6.97 the 31st of December 2008.

Figure 1: Main results with pre-treatment interactions



Note: Yearly interaction effects for 2003-2010, normalized to zero in 2007, 95% confidence interval.

consequence of households' behaviour. If households generally act more carefully, in the sense that fewer large transactions occur, this could tighten the distribution and increase the precision of the estimates.[18] Comparing the level in 2008 to 2007, the drop in consumption is now more than 2 percent. The reason why the effect reported in Table 3 is lower, is that the difference between the treated and control group is slightly lower in 1995-2003 than that observed between 2003 and 2007. Compared to 2007, the reduction in 2010 is also significant, suggesting that the sudden drop in 2008 may have had some persistence, or there could be secondary effects of changes to policy or other factors that occured in the aftermath.

An important question is whether the drop in consumption is driven by changes in income. I therefore explore an additional specification where I use the ratio of consumption c_{it} to disposable income y_{it},

$$\frac{c_{it}}{y_{it}} = \alpha_0 + \sum_{j=1995}^{2010} \theta_j year_j + \delta_0 T_{it} + \delta_1 T_{it} * D_{2008} + \delta_2 T_{it} * D_{2009} + \delta_3 T_{it} * D_{2010} + X_{it}\beta + \gamma M_j + \varepsilon_{it} \quad (3)$$

Other than the change of dependent variable, the specification is equal to equation (2). Table 4 shows the effect on normalized consumption by disposable income. Normalized consumption drops by more than 2.5 percent in the five worst hit municipalities. This confirms that the decrease in consumption is not a mechanical result from households experiencing lower disposable income.

[18] Another reason for the increased noise before the event could be the tax reform in 2006, where the wealthy adjusted their income and wealth to avoid the introduction of tax on dividends. It is not apparent why this should lead to less precise estimates only among the five worst hit. I have also performed stricter trimming of the data to see whether that reduces the amount of noise, but the pattern remains.

This is also largely confirmed through the fact that I find no effect on unemployment (see Appendix 7.2). Thus, it seems that the shock did not carry any short term effects on the labour market.[19]

The estimation displays a similar pattern to the main specification, underlining that the effect is driven by households in the five worst hit consuming less of their income in the first year following the event. Furthermore, the zero-effect on income is confirmed in the top right panel of Table 4, using log disposable income as the dependent variable.

Robustness and an investigation of alternative effects is deferred to Section 7.1 and 7.2 in the Appendix. The placebo test in Table 7 shows that there are no other significant differences between the groups at a 0.1 percent level in earlier periods. In other words, it is highly unlikely that the estimated effects on consumption are found by chance. Furthermore, Table 8 shows that the effect is robust even when including differing linear trends between the treatment and control group. Varying the set of control variables changes the point estimates slightly, however, the qualitative pattern remains.

Decomposing the effect over asset types

To further understand the mechanism underlying the consumption reduction, I decompose the imputed consumption measure (see equation (1)) and test for differences in the two main types of active saving, namely debt and financial assets: $\Delta Assets = \Delta Debt + \Delta Financial\_assets$. Because changes in the level of an asset can be negative, the logarithmic transformation can no longer be employed. Normalizing by income showed the same qualitative results as log consumption in the main specification, so I normalize saving in debt and financial assets by income to reduce the impact of extreme observations.[20] Other than the change of dependent variable, the specification is equal to equation (2). The results are shown in the bottom panel of Table 4, and show that the difference in consumption comes from differences in the saving in debt to income ratio. Households in the affected municipality seem to be less willing, or less able, to obtain debt. Alternatively, they reduce their existing debt by making extra down payments when the shock occurs. There is no significant effect on saving in financial assets.

[19]Since disposable income affects current consumption mechanically, I show that the results are unchanged when using lagged disposable income as dependent variable (Table 6 in the Appendix).

[20]Since this creates some outliers, I trim the ratios, removing the 1th and 99th percentile observations.

Table 4: Effect on consumption to income ratios and decomposed consumption

	Consumption to disposable income		Log disposable income	
	Five worst hit	*Three unaffected*	*Five worst hit*	*Three unaffected*
T * D2008	-0.0257***	-0.0101	-0.0202	0.0227*
	(.0051)	(.0117)	(.0166)	(.008)
T * D2009	-0.00237	-0.00441	-0.0104	0.0218**
	(.0061)	(.0191)	(.0095)	(.0061)
T * D2010	-0.00242	-0.00139	0.00038	0.0204*
	(.0133)	(.0197)	(.0063)	(.0069)
Observations	1 116 025	914 680	1 138 613	933 520

	Saving in debt to income		Saving in financial assets to income	
	Five worst hit	*Three unaffected*	*Five worst hit*	*Three unaffected*
T * D2008	0.0173***	0.0089	-0.0187	0.0031
	(.0040)	(.0147)	(.0529)	(.0124)
T * D2009	0.0089*	0.0083	-0.0469	-0.002
	(.0043)	(.0135)	(.0499)	(.0209)
T * D2010	0.0037	-0.0003	-0.0623	-0.0062
	(.0082)	(.0189)	(.0669)	(.0215)
Observations	1 118 104	912 601	1 116 025	914 680

Note: Control variables: Age-indicators, education length- and type fixed effect, municipality fixed effect, year fixed effect, immigration category and household size. Log disposable income is included linearly except where it is dependent variable.
Cluster robust standard errors on treatment level (municipality)
* p<0.05, ** p<0.01, *** p<0.001

Decomposing the effect over age groups

Having established that there is an effect in the five worst hit, a way of ensuring that the response is driven by the shock, is to see whether those who benefit more from public services are the ones who changed behaviour. Identifying subgroups that should be more responsive is not straight forward for several reasons. In Norway, public jobs and goods can be financed either directly by the state, or via municipal budgets. As an example, not all publicly provided jobs are under direct control of the municipality. Therefore, a worker in a private firm that does maintenance for the municipality could, in principle, have more reason to worry than a public employee. Furthermore, unemployment and health benefits are both provided by the state and unaffected by the shock.

Finally, a worker might not be employed in the same municipality as the one where he or she resides.

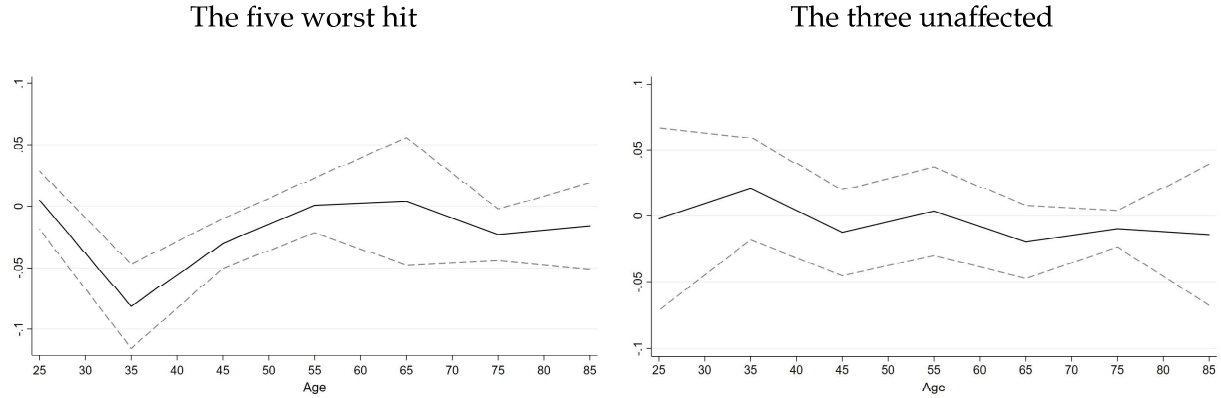
The ideal strategy is to study receivers of publicly provided goods financed by the municipality, since these are provided where households reside. Examples of such services include health services, schools and kindergardens, but could also be other services that the municipality has decided to offer their inhabitants. Since there is no variable stating which households are the largest recipients of services, I need to use indirect measures. While it is highly desirable to identify high receivers, doing it via observable characteristics may lead to imprecise identification of receivers as well as challenges with small samples. For example, there are not enough single mothers with kids in the affected municipalities to perform a meaningful estimation on this subsample.

To avoid complications from identifying receivers, small samples and separating between state and municipality, I rely on the fact that publicly provided services are mainly aimed at the young and the old. Specifically, I estimate the response based on the observed age of the family head. Figure 2 shows how the effect in the five worst hit municipalities is distributed over seven age groups. These results are obtained by splitting the sample in ten-year birth cohorts and performing the exact same difference in difference analysis as before. We see that the effect is driven mainly by young households. Interestingly, but less clear, retired households (the group between 70 and 80) respond as well. There might be other explanations for differences across age groups (for example, credit constraints, level of assets). However, we note that the age groups that has the largest recipients of public goods and services are the ones who reduce their consumption. The effect is only present in the worst affected, while there is no effect for any age group in the unaffected.

Risk-taking behaviour of affected households

Given that uncertainty is the driver of the consumption response, I perform an additional estimation on risk-taking behaviour. When households have preferences in accordance with prudence and temperance, they are sensitive to their overall risk exposure (Heaton and Lucas, 2000). Therefore, if households in the affected municipalities perceive the economic outlook of the municipality as more uncertain, a way of reducing their overall exposure to uncertainty is to reduce the amount of risky assets held. I follow Fagereng, Guiso, and Pistaferri (2017), who define the risky share of financial assets, RS, as the share of stocks and mutual funds to the overall sum of stocks, bonds,

Figure 2: Consumption effect by age (2008)



Note: Estimation based on splitting sample in 10-year cohorts. Control variables: Age-indicators, education length- and type fixed effect, municipality fixed effect, year fixed effect, immigration category and household size. Log disposable income entered as a control linearly. Dashed 95% confidence intervals based on cluster robust standard errors.

mutual funds, non listed stocks and bank deposits. Since only a small fraction own such assets, the following analysis is done on the subset of the sample owning risky assets in 2007. I test whether there is a reallocation to holding less risky assets by estimating the same specification as used in the main estimation for consumption, only changing the dependent variable to the risky share, RS, and conditioning on ownership in 2007:

$$RS = \alpha_0 + \delta_0 T_{it} + \sum_{j=1995}^{2010} \theta_j year_j + \delta_1 T_{it} * D_{2008} + \delta_2 T_{it} * D_{2009} + \delta_3 T_{it} * D_{2010} + X_{it}\beta + \gamma M_j + \varepsilon_{it} \quad (4)$$

Table 5 shows that the risky share decreases by 1.56 percent in the worst affected in 2008. This is, again, in contrast to the zero effect found in the three unaffected. Following the same pattern as consumption, we see that there is no effect in 2009 and 2010. The result is consistent with a story of uncertainty. Households living in municipalities that struggled with financing their public services, and eventually had to be bailed out, did not only hold back on consumption by saving more, they also reallocated their assets to holding less risky ones. Once these municipalities received extraordinary transfers, there is no significant difference in risk-taking behaviour measured by the share of risky assets.

One could argue that the reallocation to safer assets could be a news-shock effect: Households in the affected municipalities are reminded that the stock market is risky when news about the

Table 5: Effects on the share of risky assets held

	The five worst hit	The three unaffected
T*D2008	-.0156**	-.0009
	(.0054)	(.0117)
T*D2009	-.0044	-.0073
	(.0078)	(.0110)
T*D2010	-.0056	-.0019
	(.0073)	(.0114)
Observations	260 780	235 476

Note: Control variables: Age-indicators, education length- and type fixed effect, municipality fixed effect, year fixed effect, immigration category and household size.
Cluster robust standard errors on treatment level (municipality)
* p<0.05, ** p<0.01, *** p<0.001

municipal losses arrive, and reallocate. If being reminded of stock market risks are the sole driver of reallocation, we would expect there to be an effect in the three unaffected municipalities as well, since they were exposed to the news story in a similar way. No such effects are found.

6 Summary and conclusion

I have used a difference in difference approach to evaluate how private consumption of households changed when their local government experienced large losses of public assets, increasing the uncertainty of future fiscal outcomes. Households in the five worst hit municipalities reduced their consumption by 1.8 percent the first year after the event. In the three municipalities that did not struggle economically afterwards, no effect is found on private consumption. The effect was temporary, meaning I only find an effect the first year after the event.

The fall in consumption is driven by age groups who are the largest recipients of public goods - the young and the elderly. In other words, groups that are the largest uses of public services acted more careful. I show that the change in consumption comes from increased saving, meaning households spend less of their income than their comparison group. Finally, I find that households holding risky assets rebalanced their portfolios after the event, holding a lower share of risky assets one year after. This is consistent with prudence - households' allocational choices seem to be sensitive to their overall exposure to uncertainty. The findings of the paper is interpreted as

wait and see behaviour: households respond to uncertainty by holding back consumption until uncertainty regarding fiscal outcomes is resolved.

Taken together, these findings provide important insights about how households make economic choices when they are exposed to increased uncertainty that has clear implications for policy. When households react by holding back consumption until uncertainty is resolved, it means that policy should be aimed at providing security against worst case scenarios and help providing as much information as possible. Such measures could prove important for economic activity during turbulent times, and be important alternatives to for example cash transfers, since the wait and see behaviour implies lower marginal propensities to consume.

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7 Appendix

Table 6: Effect on consumption to income ratios

	Consumption to disposable income		Consumption to lagged income	
	(1)	(2)	(3)	(4)
	Five worst hit	*Three unaffected*	*Five worst hit*	*Three unaffected*
T * D2008	-.0257***	-.0100	-.0283*	-.0043
	(.0051)	(.0120)	(.0136)	(.0118)
T * D2009	-.0024	-.0044	.0077	-.0341
	(.0061)	(.0191)	(.0148)	(.0165)
T * D2010	-.0024	-.0014	-.0069	-.0016
	(.0133)	(.0197)	(.00623)	(.0231)
Observations	1 116 025	914 680	717 649	568 059

Note: Control variables: Age-indicators, education length- and type fixed effect, municipality fixed effect, year fixed effect, immigration category and household size. Log disposable income included as a linear control.
Cluster robust standard errors on treatment level (municipality)
* p<0.05, ** p<0.01, *** p<0.001

7.1 Placebo analysis and robustness checks

In this section I perform placebo and robustness checks. The placebo exercise is displayed in Table 7. Here, I estimate a one-year effect using 1999-2008 as the treatment periods. The overall results from this exercise shows that measuring an effect at 0.1 percent significance level on log consumption is rare in my treatment and control samples. I find no significant effect at 0.1 percent level in the three unaffected in any of the years. I also perform the same placebo exercise for consumption to income ratio and log disposable income. The specification is similar to the one used for the main results, except that I estimate 1-year effects excluding the future sample. This means that for the first column of Table 7, the treatment year is $T = 1999$, and the sample employed is 1995-1999.

$$x_{it} = \alpha_0 + \sum_{j=1995}^{T} \theta_j year_j + \delta_0 T_{it} + \delta_1 T_{it} * D_T + X_{it}\beta + \gamma M_j + \varepsilon_{it} \tag{5}$$

Second I do robustness checks. These are displayed in Table 8 and show estimation of the main specification varying controls (columns 1-3). The qualitative pattern is the same across all the specifications: No effect in the three unaffected, and a significant effect in the five worst hit. The point estimate is sensitive to the controls included, which is to be expected given that there are time-varying differences between the municipalities in the period that affects consumption. Specification (4) includes a separate linear trend between the treated and control group. None of the interaction terms for different trends are statistically significant, and the point estimates are close to zero for all the groups. Although the treatment effect becomes less precisely estimated, it is still significant at the 5 percent level. The point estimate of consumption reduction is now 3.18 percent for the five worst hit, which is larger than in the main specification without linear trend terms. There is still no effect in the three municipalities with no change in their public spending, consistent with my main results. Specification (5) shows that inclusion of treatment specific income controls reduces the point estimate.

Table 7: Placebo exercise - Only coefficient of treatment effect (δ_2) reported

	Post treatment period, T =									
	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008
All Terra Municipalities										
Log Consumption	0.0158*	0.00903	0.00805	0.0154***	-0.00282	-0.00000657	-0.00242	0.00202	-0.000411	-0.0115*
	(0.00603)	(0.00560)	(0.00537)	(0.00454)	(0.0113)	(0.00867)	(0.00923)	(0.00769)	(0.00831)	(0.00567)
Consumption to income ratio	0.0158**	0.0146**	0.0149*	0.0148*	-0.00537	0.00222	-0.00713	0.00133	-0.00309	-0.0163*
	(0.00521)	(0.00523)	(0.00693)	(0.00739)	(0.0118)	(0.00885)	(0.00897)	(0.00954)	(0.0100)	(0.00743)
Log disposable income	-0.00289	-0.00919	-0.00884	-0.0108	-0.00855	0.00280	-0.00491	-0.00873	0.00503	-0.000404
	(0.0105)	(0.00634)	(0.00452)	(0.00630)	(0.00545)	(0.00612)	(0.00464)	(0.00590)	(0.00968)	(0.00862)
The five worst hit - T5										
Log Consumption	0.0128	-0.00480	0.00677	0.0125*	0.00685	0.0174	0.0165	0.00478	0.00727	-0.0177***
	(0.00774)	(0.00640)	(0.00463)	(0.00501)	(0.00765)	(0.0172)	(0.0169)	(0.0155)	(0.0193)	(0.00448)
Consumption to income ratio	0.0129	0.00157	0.00929	0.0104	-0.00378	0.0211	0.00352	0.00534	0.0131	-0.0255***
	(0.00903)	(0.00609)	(0.00521)	(0.00990)	(0.00780)	(0.0172)	(0.0161)	(0.0251)	(0.0177)	(0.00513)
Log disposable income	-0.0106*	-0.0138	-0.0121	-0.0108	-0.00980*	-0.00120	-0.0115*	-0.0136*	-0.0107	-0.0201
	(0.00473)	(0.00937)	(0.00675)	(0.00631)	(0.00442)	(0.00362)	(0.00516)	(0.00570)	(0.00774)	(0.0166)
The three unaffected - T3										
Log Consumption	0.0123	0.0201**	0.00596	0.0119	-0.00934	-0.0163*	-0.0197*	-0.00187	-0.0135	-0.00429
	(0.00854)	(0.00499)	(0.00934)	(0.00607)	(0.0159)	(0.00618)	(0.00764)	(0.00939)	(0.0102)	(0.00893)
Consumption to income ratio	0.0112*	0.0239***	0.0152	0.00903	-0.00969	-0.0174**	-0.0214	-0.00538	-0.0228	-0.00999
	(0.00503)	(0.00474)	(0.0131)	(0.00981)	(0.0178)	(0.00573)	(0.0108)	(0.00832)	(0.0123)	(0.0118)
Log disposable income	0.00765	-0.00163	-0.0000762	-0.00420	-0.00230	0.00925	0.00211	-0.00211	0.0200	0.0223*
	(0.0161)	(0.00876)	(0.00571)	(0.00872)	(0.00748)	(0.00935)	(0.00648)	(0.00828)	(0.0130)	(0.00787)

Note: Control variables: Age-indicators, education length- and type fixed effect, municipality fixed effect, year fixed effect, immigration category and household size. Log disposable income enters linearly as control except when being the dependent variable.
Cluster robust standard errors on treatment level (municipality)
* p<0.05, ** p<0.01, *** p<0.001

Table 8: Main results with varying set of control variables

	The five worst hit				
	(1)	(2)	(3)	(4)	(5)
T*D2008	-.0458**	-.0342*	-.0179***	-.0318*	-.013*
	(.0148)	(.0146)	(.0044)	(.0129)	(.0051)
T*D2009	-.0263**	-.0125	-.0031	-.0189	.0026
	(.0096)	(.0088)	(.0061)	(.0122)	(.0058)
T*D2010	-.0215	-.0083	-.0090	-.0267	-.0028
	(.0161)	(.0145)	(.0093)	(.0245)	(.0105)
Municipality fixed effect	Yes	Yes	Yes	Yes	Yes
Year fixed effect	Yes	Yes	Yes	Yes	Yes
Age fixed effect	No	Yes	Yes	Yes	Yes
Other characteristics	No	No	Yes	Yes	Yes
Separate linear trend	No	No	No	Yes	No
Separate income control	No	No	No	No	Yes
Observations	1 138 613	1 138 613	1 138 613	1 138 613	1 138 613
	The three unaffected				
	(1)	(2)	(3)	(4)	(5)
T*D2008	.0133	.0083	-.004	-.0007	-.0027
	(.0128)	(.0157)	(.009)	(.0115)	(.0094)
T*D2009	.0106	.0062	-.0059	-.0018	-.0040
	(.0236)	(.0296)	(.0211)	(.0198)	(.0200)
T*D2010	.0227	.0163	.0029	.0075	.0049
	(.0195)	(.0267)	(.0154)	(.0159)	(.0144)
Municipality fixed effect	Yes	Yes	Yes	Yes	Yes
Year fixed effect	Yes	Yes	Yes	Yes	Yes
Age fixed effect	No	Yes	Yes	Yes	Yes
Other characteristics	No	No	Yes	Yes	Yes
Separate linear trend	No	No	No	Yes	No
Separate income control	No	No	No	No	Yes
Observations	933 520	933 520	933 520	933 520	933 520

Note: Other characteristics : Indicators for Education length- and type, immigration category and household size. Log disposable income enters linearly.
Cluster robust standard errors on treatment level (municipality)
* p<0.05, ** p<0.01, *** p<0.001

7.2 Alternative effects and public spending

Unemployment, income expectations and moving

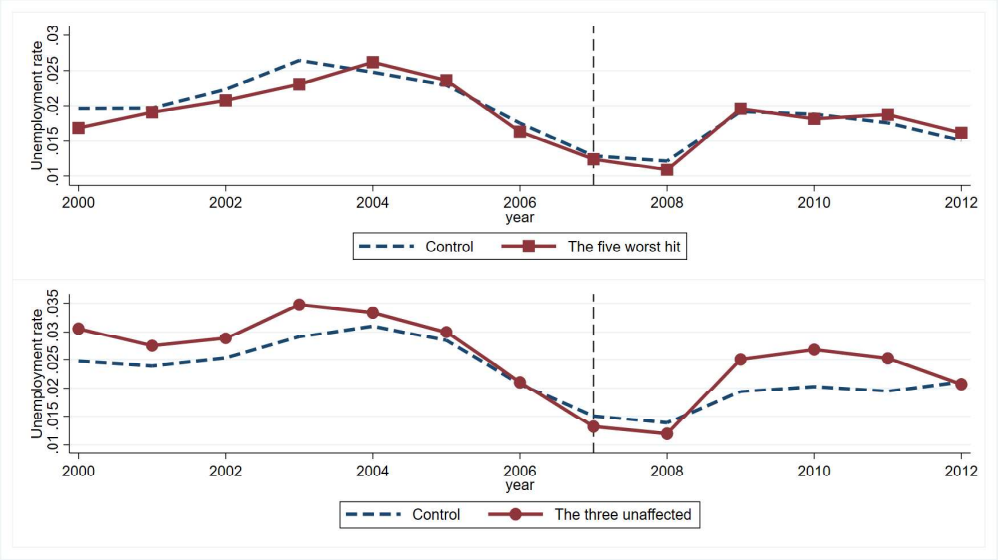
In this subsection, I address other effects in the municipalities, that potentially could affect or drive the observed results. Specifically, I show that there are no effects on unemployment and moving in the affected municipalities.

The issue of changes in disposable income was addressed in the main results section, and is addressed in particular in the robustness section, but I revisit the question with a different perspective here. Even though the decrease in consumption is not driven by decreases in the disposable income of households *within* my estimation sample, changes in *overall* employment could influence expectations and uncertainty regarding future income. Furthermore, Basten, Fagereng, and Telle (2016) show that households save more, anticipating future unemployment. Therefore, I check whether there was any changes to the labor market after the events. I collect yearly unemployment data on municipality level. Estimating the difference in difference on these data, I find no significant changes in unemployment after the shock. Figure 3 plots the unemployment rate in the treated and control group for the five worst hit and the three unaffected. We see that there is no difference in the five worst hit, while there is a small increase in the three unaffected. Note that the unemployment rate lies above in the pre-treatment period in the three unaffected. Therefore, the spike in 2009 might represent natural variation. To the extent unemployment risk could be a concern, or driver, there are no indications in the data of effects on labor market conditions in the five worst hit, represented through unemployment.

As discussed in the data section, moving could be affected by the treatment, and mechanically affect the results. Given the need for strict sample selection to impute consumption (including the removal of observations where people move), it could be the case that the effect I find is simply driven by a selection of stayers. To address this, I use publicly available data containing quarterly population data at the municipality level. Since there are large differences in population between municipalities, I use relative changes and estimate the difference in difference. I find no significant effect on changes in population, meaning that the event did not lead to people moving away.[21] Figure 4 plots the relative changes in population in the treated and control groups, net of

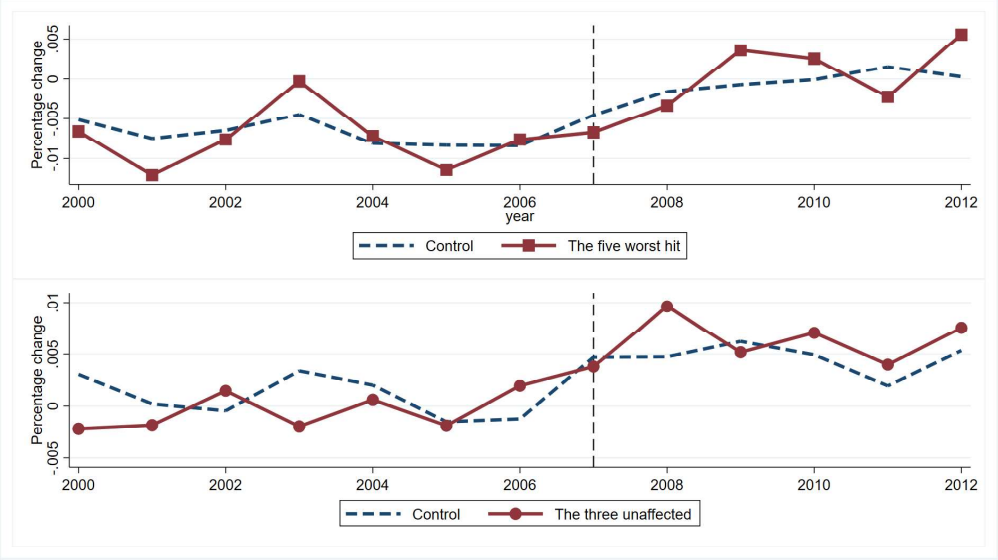
[21] There are no indications of this mentioned in the annual reports of the municipalities.

Figure 3: Municipality unemployment rates before and after the event



Note: Average yearly unemployment rate data from Statistics Norway. Rate averaged across municipalities in the treated and control group respectively.

Figure 4: Percentage change in population before and after the event



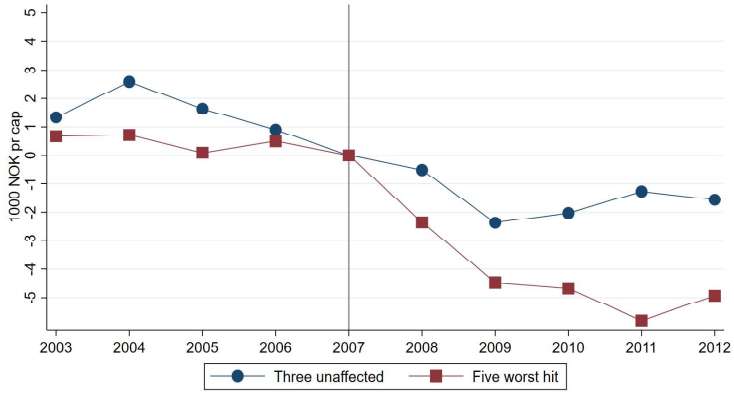
Note: Average yearly growth rate of population. Data from Statistics Norway. Rate averaged across municipalities in the treated and control group respectively, net of municipality fixed effects.

municipality fixed effects. These indicate that changes to the population do not affect the results. This is also supported by the sample employed from the register data, where the number of observations per year is unaffected by the event.

Results from municipality income statements

Since the financial choices of local governments can be of importance for private consumption, I use data from municipality annual reports between 2003 and 2012, and estimate changes to public expenditures per capita in the affected municipalities. Specifically, I use the same treated and control groups as in the main set up, and estimate yearly interaction effects the same way as was done for Figure 1. The specification includes municipality and year fixed effects. I also control for population. Figure 5 plots the interaction terms of the five worst hit and the three unaffected.[22]

Figure 5: Interaction effects on per capita public expenditures, normalized to zero in 2007



Note: Estimation of interaction effects done separately for the two plots. Controls include municipality fixed effects, year fixed effects and population size.

The plot for the five worst hit shows that there was a parallel trend prior to 2007. Public expenditure per capita went down in 2008, and continued to do so for the remaining period of analysis. There was, in other words, a permanent drop in expenditures.

The plot for the three unaffected has a downward sloping trend, compared to their comparison group, from 2004 to 2008. This continued until 2009, when there was a small distinct drop, before

[22]Note that only the individual interaction coefficient for the five worst hit in 2011 is significantly different from zero on a five percent level. Given the number of effects estimated relative to observations, it is no surprise that this estimation has low power.

it caught up marginally towards the end of the period. While the observed drop from 2007 to 2010 do not chime well with a narrative claiming these municipalities were unaffected, the measures taken looks less severe in these municipalities.

Table 9 gives a full overview of the results from estimating the average treatment effect in 2009-2012 on all elements of the income statement normalized by population. The estimation includes time- and municipality fixed effects. The results show that the most effective measures taken are on the cost side. Specifically, purchases of goods, social expenditures, wage expenditures and transfers were significantly reduced in the five worst hit. In the three unaffected, the reduction was mainly related to a reduction in wage expenditures. The local governments' ability to cover losses through increases in income is limited in this context. Although there is a point estimate increase in user fees, real estate taxes and other direct/indirect taxes, there is also a reduction in transfers offsetting those. None of these are statistically significant. In the three unaffected, there is a decrease in income similar to their decrease in costs.[23]

Given that the drop in public expenditures is permanent, and the effect seen on private consumption is temporary, it seems unlikely that changes in public services themselves are the sole reason for the drop in private consumption in 2008. While there are valid reasons why consumption should fall permanently, it seems unlikely that the initial drop can be fully explained by changes to public services. Interesting to note in this case, is the small decrease in the five worst hit in 2010, seen in Figure 1. An interesting extension could be to test if there is a persistent component to the consumption drop that can tell us something about the relationship between public and private spending as well.

Although uncertainty is difficult to quantify in this context,[24] several elements in the time line point towards uncertainty as a plausible explanation for the sudden drop of private consumption in 2008. As the previous section showed, households in the worst hit municipalities had real reasons to be concerned about the fiscal future of the municipality, however, this was a concern that where greatly reduced when the central government provided additional funding for essential services. Thus, when the central government opened up the bail out, households understand that

[23] This might reflect that Haugesund tried to downsize their operational level in the period.

[24] The literature on aggregate uncertainty often uses the spread of forecasters or stock market returns to create measures of uncertainty. Baker, Bloom, and Davis (2015) provides such an index. On a local level, quantifying uncertainty is an even more challenging task.

Table 9: Treatment effects (2009-2012) from municipal annual reports (1000 NOK pr capita)

	The five worst hit		The three unaffected	
	ω_2	St.error	ω_2	St.error
User fees	0.431	(0.301)	-0.0577	(0.328)
Sale and rental	-0.434	(0.505)	-1.507*	(0.726)
Transfer with claims	-1.895***	(0.649)	-0.916	(0.839)
Grants	0.323	(1.052)	-0.931	(1.530)
Other state transfers	1.010	(1.815)	0.238	(0.309)
Other transfers	-1.810***	(0.530)	-0.457	(0.310)
Income taxes	-0.837	(0.865)	0.141	(0.513)
Real estate taxes	0.529	(0.549)	0.0281	(0.824)
Other direct/indirect taxes	1.226	(0.847)	-0.0602	(0.265)
Sum operating income	-1.456	(2.418)	-3.522	(2.858)
Wage expenditures	-1.718*	(0.950)	-2.646**	(0.934)
Social expenditures	-0.370***	(0.109)	-0.100	(0.461)
Purchase of goods	-2.664***	(0.424)	-0.114	(1.010)
Purchases of services	-0.163	(0.648)	0.290	(0.504)
Transfers	-1.277***	(0.299)	-0.351	(0.477)
Depreciation	0.0706	(0.176)	0.123	(0.256)
Distributed costs	1.025**	(0.404)	-0.810	(1.007)
Sum operating expenses	-5.095***	(1.719)	-3.608	(2.423)
Gross operating profit	3.405**	(1.479)	0.653	(0.741)
External financial income	1.535	(1.317)	0.453	(0.415)
External financial expenditures	-4.988**	(1.917)	-0.614	(0.711)
External financial transactions	6.523**	(3.160)	1.057*	(0.496)
Net operating profit	9.999**	(4.384)	1.834*	(0.860)
Observations		783		126

Note: All specifications include year and municipality fixed effects in addition to controlling for population.
Cluster robust standard errors on treatment level
* p<0.05, ** p<0.01, *** p<0.001
All variables in 1000NOK

there are limits to how bad the cuts will be. Having experienced this reduction in uncertainty, households go back to normal behaviour, despite the fact that local governments makes further cuts to spending. I therefore regard uncertainty as the most likely driver.

Table 10: Full results from main specification

	The five worst hit		*The three unaffected*	
Variable	Coefficient	Standard error	Coefficient	Standard error
T*Year=2008	-.0179384***	(.0044273)	-0.00432	(0.00897)
T*Year=2009	-.0030807	(.0061107)	-0.00595	(0.0211)
T*Year=2010	-.0090366	(.0092757)	0.00286	(0.0154)
T=1	-.0358463***	(.0007517)	0.00412	(0.00245)
Year=1994	-.120284***	(.0054784)	-0.146***	(0.00852)
Year=1995	-.1170369***	(.0070317)	-0.152***	(0.00763)
Year=1996	-.1131295***	(.004476)	-0.133***	(0.00670)
Year=1997	-.1103248***	(.0056616)	-0.134***	(0.0102)
Year=1998	-.0857439***	(.0055135)	-0.107***	(0.0103)
Year=1999	-.1160272***	(.0054098)	-0.127***	(0.0105)
Year=2000	-.0799354***	(.0062698)	-0.107***	(0.0113)
Year=2001	-.078754***	(.0055581)	-0.0945***	(0.0126)
Year=2002	-.0672315***	(.0039775)	-0.0756***	(0.00824)
Year=2003	-.0590211***	(.0060827)	-0.0800***	(0.00769)
Year=2004	-.0461364***	(.0039343)	-0.0551***	(0.00854)
Year=2005	-.0728492***	(.0041392)	-0.0808***	(0.00930)
Year=2006	-.026162***	(.0037554)	-0.0401***	(0.00691)
Year=2007	0	.	0	.
Year=2008	.0046423	(.0040291)	-0.0248*	(0.00976)
Year=2009	-.0420549***	(0057247)	-0.0679***	(0.0104)
Year=2010	-.0138301**	(.0045893)	-0.0378*	(0.0130)
Log disposable income	.7452732***	(.0044803)	0.759***	(0.00532)
Age=25	0		0	
Age=26	-.0174847***	(.0041238)	-0.0160***	(0.00349)
Age=27	-.0248699***	(.004767)	-0.0203**	(0.00634)
Age=28	-.024698***	(.0047917)	-0.0306***	(0.00368)
Age=29	-.0271885***	(.0060674)	-0.0289***	(0.00581)
Age=30	-.0367847***	(.005387)	-0.0372**	(0.00949)
Age=31	-.0407553***	(.0052691)	-0.0252*	(0.00895)
Age=32	-.0330198***	(.0061442)	-0.0293***	(0.00376)
Age=33	-.0435534***	(.0057564)	-0.0422***	(0.00538)
Age=34	-.041346***	(.0057851)	-0.0394***	(0.00629)
Age=35	-.0426793***	(.0054616)	-0.0496***	(0.00808)
Age=36	-.0506713***	(.0052755)	-0.0451***	(0.00919)
Age=37	-.04455***	(.0053432)	-0.0398***	(0.00720)
Age=38	-.0500981***	(.0059894)	-0.0525***	(0.00853)
Age=39	-.0527731***	(.0048551)	-0.0503***	(0.00723)
Age=40	-.043085***	(.0060621)	-0.0542***	(0.00885)
Age=41	-.0531906***	(.0052523)	-0.0506***	(0.0107)
Age=42	-.0566646***	(.0052159)	-0.0537***	(0.00718)
Age=43	-.0505491***	(.0058246)	-0.0421**	(0.0101)
Age=44	-.047203***	(.0055876)	-0.0522***	(0.00935)
Age=45	-.0439403***	(.005417)	-0.0375***	(0.00841)
Age=46	-.0405389***	(.0055619)	-0.0387***	(0.00637)
Age=47	-.0356618***	(.0049859)	-0.0414***	(0.00900)
Age=48	-.0355295***	(.0061278)	-0.0406***	(0.00633)
Age=49	-.0420633***	(.0066317)	-0.0390***	(0.00821)
Age=50	-.0368304***	(.0061596)	-0.0256**	(0.00653)
Age=51	-.0304367***	(.0054204)	-0.0261*	(0.00941)
Age=52	-.0331119***	(.005182)	-0.0268**	(0.00722)
Age=53	-.0380445***	(.0054033)	-0.0301***	(0.00498)
Age=54	-.0286734***	(.0047996)	-0.0237*	(0.00792)
Age=55	-.0418***	(.005454)	-0.0343***	(0.00778)
Age=56	-.0287622***	(.0057046)	-0.0252**	(0.00687)
Age=57	-.031259***	(.0057941)	-0.0313**	(0.00797)
Age=58	-.0260882***	(.0075324)	-0.0297***	(0.00588)
Age=59	-.0317721***	(.0066997)	-0.0366***	(0.00694)
Age=60	-.040365***	(.0053694)	-0.0180*	(0.00734)
Age=61	-.0270308***	(.0050714)	-0.0161**	(0.00443)
Age=62	-.0328059***	(.0061594)	-0.0262**	(0.00752)
Age=63	-.0151357*	(.0066247)	-0.00341	(0.00510)
Age=64	-.0177499**	(.0053958)	0.0100	(0.00618)
Age=65	-.0274711***	(.0060446)	-0.0112	(0.0110)
Age=66	.0004119	(.0059494)	0.00159	(0.00931)
Age=67	-.0344252***	(.0069343)	-0.00727	(0.00511)
Age=68	-.0305188***	(.0049218)	-0.0107	(0.00803)
Age=69	-.0312929***	(.0049054)	-0.00998	(0.00573)
Age=70	-.0237178***	(.0056804)	-0.00782	(0.00402)
Age=71	-.0326435***	(.0058932)	-0.0103	(0.00799)
Age=72	-.0292321***	(.0059577)	-0.0196**	(0.00477)
Age=73	-.0315004***	(.0049932)	-0.0235***	(0.00370)
Age=74	-.0414585***	(.0057702)	-0.0217***	(0.00398)
Age=75	-.0362503***	(.0052815)	-0.0222***	(0.00387)
Age=76	-.0466856***	(.0054033)	-0.0324***	(0.00537)

Age=77	-.0496838***	(.0053669)	-0.0334***	(0.00696)
Age=78	-.0498062***	(.0048859)	-0.0419***	(0.00524)
Age=79	-.0568583***	(.0056252)	-0.0511***	(0.00466)
Age=80	-.0527337***	(.0051894)	-0.0388***	(0.00617)
Age=81	-.0623773***	(.0055442)	-0.0591***	(0.00632)
Age=82	-.0704677***	(.0049254)	-0.0621***	(0.00564)
Age=83	-.0711324***	(.0050979)	-0.0599***	(0.00730)
Age=84	-.0798396***	(.0046442)	-0.0629***	(0.00782)
Age=85	-.0874148***	(.0058625)	-0.0672***	(0.00703)
Age=86	-.0876697***	(.0054509)	-0.0780***	(0.00786)
Age=87	-.0891694***	(.0063961)	-0.0887***	(0.00779)
Age=88	-.1026602***	(.0051798)	-0.0796***	(0.0109)
Age=89	-.0985077***	(.0057729)	-0.101***	(0.00811)
Age=90	-.0976277***	(.0061135)	-0.0871***	(0.0107)
Non-/pre school	0		0	
Elementary	.0144474	(.0182318)	-0.0117	(0.0209)
Secondary	.0063745	(.016478)	0.0105	(0.0198)
High School Basic	.0515164**	(.0169074)	0.0542*	(0.0208)
High school	.0560955**	(.0175359)	0.0626*	(0.0218)
High school + add on	.0762848***	(.0162819)	0.0682**	(0.0222)
University/college lower	.0793507***	(.0177999)	0.0765**	(0.0218)
University	.1076615***	(.019468)	0.0870**	(0.0219)
Research	.1132591***	(.0261574)	0.0994**	(0.0276)
Not given	.017758	(.0108874)	0.0206	(0.0128)
General	0		0	
Humanities Art	-.0311863***	(.0051734)	-0.0220**	(0.00553)
Teacher pedagogy	-.0055095	(.0041063)	-0.00764*	(0.00308)
Social Legal	.0026311	(.0078826)	-0.00495	(0.00712)
Econ Adm	-.0007322	(.0038893)	0.00186	(0.00319)
Natural engin	-.0087095**	(.0028323)	-0.0138**	(0.00363)
Health sports	.0005921	(.0026872)	-0.00413	(0.00362)
Primary	-.0256651***	(.0047917)	-0.0277***	(0.00640)
Transport service safety	.004334	(.0049268)	0.00597	(0.00482)
Not given	-.0264575	(.01367)	-0.0265	(0.0131)
Immigr:A	0		0	
Immigr:B	-.0012241	(.0040899)	-0.0226**	(0.00685)
Immigr:C	-.0230954	(.0401478)	0.000743	(0.0242)
Immigr:E	.0234705	(.0168392)	-0.0290***	(0.00579)
Immigr:F	.004326	(.0061182)	-0.00709	(0.00489)
Immigr:G	.0003855	(.0124339)	-0.0241**	(0.00786)
Children=0	0		0	
Children=1	.0498309***	(.0026963)	0.0375***	(0.00281)
Children=2	.1016081***	(.0033053)	0.0720***	(0.00627)
Children=3	.1261921***	(.0064106)	0.0944***	(0.00972)
Children=4	.1600423***	(.0148656)	0.115***	(0.0134)
Children=5	.1707393***	(.0242581)	0.0652	(0.0419)
Children=6	.198247***	(.048908)	0.177*	(0.0661)
Children=7	.4848973***	(.1175907)	0.324*	(0.117)
Children=8	.3691062	(.2405031)	0.235*	(0.0861)
Children=9	.4768396	(.4106495)	0.488**	(0.157)
Children=10	.305546***	(.0637233)	0.0344	(0.0857)
Children=11			0.907***	(0.0693)
Family size= 1	0		0	
Family size= 2	.068592***	(.0023317)	0.0684***	(0.00324
Family size= 3	.066746***	(.0026339)	0.0865***	(0.00503
Family size= 4	.096362***	(.0042366)	0.142***	(0.00613
Family size= 5	.0853685***	(.0050717)	0.144***	(0.00953
Family size= 6	.0627678***	(.0094748)	0.119***	(0.0101)
Family size= 7	.070423**	(.021981)	0.104**	(0.0281)
Family size= 8	.0224686	(.0336154)	0.112	(0.0602)
Family size= 9	-.1702951*	(.0823804)	0.0255	(0.0490)
Family size= 10	-.2129183*	(.1068991)	-0.0811	(0.0936)
Family size= 11	-.0143542	(.0634729)	-0.236	(0.228)
Family size= 12	-.2905356	(.4101984)	-0.175*	(0.0715)
Family size= 13			0.160*	(0.0700)
Family size= 14			-0.0695	(0.0859)
Constant	3.119949***	(.0493425)	2.951***	(0.0542)
Observations	1 138 613		933520	

Table 11: The five worst hit

Municipality name	Municipality code
Audnedal	1027
Balestrand	1418
Bardu	1922
Beiarn	1839
Berg	1929
Berlevåg	2024
Bindal	1811
Bremanger	1438
Brønnøy	1813
Bygland	938
Tysfjord	1850
Engerdal	434
Etnedal	541
Evenes	1853
Flå	615
Folldal	439
Fyresdal	831
Gildeskål	1838
Grane	1825
Gratangen	1919
Grong	1742
Kåfjord	1940
Hamarøy	1849
Hattfjelldal	1826
Hemnes	1832
Hjartdal	827
Hjelmeland	1133
Hol	620
Hornindal	1444
Høyanger	1416
Iveland	935
Jondal	1227
Kvalsund	2017
Kvam	1238
Kvinnherad	1224
Kvænangen	1943
Lebesby	2022
Lesja	512
Lierne	1738
Luster	1426
Lærdal	1422
Marnardal	1021
Masfjorden	1266
Meråker	1711
Målselv	1924
Namsskogan	1740
Narvik	1805
Nissedal	830
Nome	819
Nord-Aurdal	542
Nord-Fron	516
Norddal	1524
Nordreisa	1942
Nore og Uvdal	633
Notodden	807
Oppdal	1634
Rauma	1539
Rendalen	432
Rindal	1567
Roan	1632
Rollag	632
Saltdal	1840
Sauda	1135
Sel	517
Snillfjord	1613
Snåsa	1736
Sortland	1870
Spydeberg	123
Steigen	1848
Storfjord	1939
Suldal	1134
Surnadal	1566
Sørfold	1845
Tokke	833
Tolga	436
Tynset	437
Ullensvang	1231
Ulvik	1233
Vaksdal	1251
Valle	940
Vang	545
Vestre Slidre	543
Vik	1417
Vinje	834
Voss	1235
Åmli	929
Åseral	1026

Table 12: The three unaffected

Municipality name	Municipality code
Alta	2012
Bod	1804
Fauske	1841
Haugesund	1106
Kvinesdal	1037
Lenvik	1931
Meløy	1837
Odda	1228
Rana	1833
Sarpsborg	105
Sunndal	1563
Sør-Varanger	2030
Tinn	826
Årdal	1424