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Misreporting in the Norwegian business cash support scheme

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Abstract
We analyze the reporting response to an ambitiously targeted government support scheme for Norwegian businesses at the very start of the Coronavirus crisis in 2020. Our empirical design is based on cross-checking self-reported data in the applications for support with administratively reported data used for VAT. We find strong evidence that strategic misreporting was present but conclude that its remaining quantitative extent after enforcement actions already taken by the tax authorities was relatively small. Firms tend to misreport 4% more often than expected, and the actual support paid out was 5% higher than it should have been. We discuss possible reasons for the relatively limited extent of non-compliance and more general lessons for the design of transfer programs.

Keywords Government support programs · Policy design · Cash transfers · Firm behavior · Misreporting · COVID-19

JEL Classification E61 · H25 · H32 · M48

1 Introduction

The onset of the COVID-19 pandemic stimulated a wave of policies intended to mitigate the impact of the economic shocks. Governments pursued a variety of forms of assistance to the public and businesses, implemented over a very short period. While the economic damage was widespread, it was very heterogeneous, and the assistance was costly and potentially subject to abuse. Thus, policymakers faced the choice

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between (1) providing more cost-effective assistance targeted toward needs but subject to abuse and costly monitoring or (2) relying on more generous and closer-to-universal programs. This applied to various forms of assistance but was particularly stark in the case of business support.

Different countries picked different approaches. For example, while the US Paycheck Protection Program gave generous (forgivable) loans to most businesses with fewer than 500 employees (Autor et al., 2022; Dahl and Emmons, 2022) and the Canadian scheme focused on payroll costs (Smart et al., 2023), the Nordic countries introduced direct transfers to cover fixed costs, proportional to firm-specific sales losses (Alstadsæter et al., 2020; Andersen et al., 2022). Within the Nordic countries, the approach to monitoring varied—while the Norwegian scheme primarily relied on self-reporting and unsystematic verification ex-post, the Swedish scheme required that applications were approved by a certified public accountant (Swedish Tax Agency, 2020; Swedish Fiscal Policy Council, 2021).

We study the case of cash business assistance in Norway. The Norwegian program was targeted based on estimates of losses due to the shock, with multiple self-reported input variables and distinctions based on the exposure to policy-induced business closures. These reports were difficult to verify in real-time and cannot be precisely verified ex-post, absent audits. The Norwegian Tax Administration pursued some enforcement activities that we will discuss shortly and return to again in the final section, but our analysis focuses on the remaining undetected noncompliance. By statistically comparing the reports to the related information from administrative Value Added Tax (VAT) returns, we conclude that while strategic misreporting was present, its magnitude was relatively small.

While we provide a case study in this particular case, we hope it contributes to the broader literature on the design of government programs. In the low-income support context, there is a long line of work on the consequences of screening and, in particular, its effect on non-take up (Currie, 2006) of benefits, the types of targeting errors induced, and the trade-off between better targeting and imperfect take up (Kleven and Kopczuk, 2011). These issues interact with application costs (e.g., Deshpande and Li, 2019) and the mode of delivery of benefits (e.g. Meckel, 2020). In the tax context, an extensive literature on tax evasion documents the consequences of lax monitoring (e.g., Alstadsæter et al., 2019). Our context is unique but provides evidence that extensive targeting without explicit monitoring can be successful in some circumstances. Therefore, it raises further questions about features of the environment that can make it so. We speculate on these issues in the Sect. 5.

2 Institutional background

In response to the onset of the pandemic, the Norwegian government implemented a statewide lockdown on March 12, 2020. Over the next few weeks, 12% of the labor force signed up for unemployment benefits. The government unveiled a set of steps to address the economic effects of the coronavirus outbreak following the lockdown. In particular, on March 27, 2020, the government announced a cash support

program to partly cover firms' unavoidable fixed costs for firms that experienced a drop in revenue of at least 20% (March) or 30% (April and afterward).

The application portal opened on April 18, and the first application was approved on April 20. In total, more than 100,000 applications were submitted, with about a quarter of them manually or automatically rejected before payout. For the period March–August 2020, the support scheme was administered by the Norwegian Tax Administration. During that period, 76,500 monthly payouts were made (potentially including multiple per firm), in total NOK 6.5 Billion, according to the Tax Administration.[1]

The program's objective was to prevent needless bankruptcies and job destruction by offering financial compensation to businesses suffering significant income losses due to the pandemic. The focus was on getting the cash support quickly out to the firms without incurring unnecessary costs or delays. In order not to delay payouts, a confirmation from an accountant was not due until the end of the year. Firms could apply, be granted, and be paid the cash support quickly based on self-reported information. By regulation, the subsidy payment had to occur as soon as possible and no later than three weeks after the decision had been made. The majority of applications were handled automatically, and applicants received a decision within a short time after submission. When approved, the grant was transferred to the provided bank account within a few working days. This was in stark contrast to the structure in other countries, particularly neighboring Sweden, where an accountant had to sign off at the time of application and where the payout process was much slower (Swedish Fiscal Policy Council, 2021). Note that this is not the only notable differences between Norway and Sweden. First, the eligibility criteria for support were higher and stricter in Sweden and Sweden lacked the transparency of Norway, as it did not publicly disclose which firms received COVID support. Additionally, the two countries' lockdown strategies diverged greatly, with Sweden continuing to take a more lax approach. These factors complicate direct comparisons of the outcomes in the two countries (even if the data on Sweden was available) and goes beyond the scope of the current paper.

The danger of a system based on self-reporting and without the ability to verify information in a timely manner is misreporting. Aware of that, the tax administration informed very clearly on the application portal that audits might occur, wrongfully paid out support would have to be repaid, and that there would be sanctions in case of non-compliance with the rules.[2] Furthermore, all approved applicants were made public immediately on a dedicated governmental website, with full company name,

[1] The average exchange rate for 2020 was 1 USD = 9.4 NOK.

[2] According to paragraph 12 of the Act, administrative sanctions are imposed on support recipients who intentionally or grossly negligently provide inaccurate or incomplete information or fail to deliver mandatory details. Note that the sanctions can be applied up to 5 years after the compensation decision. The magnitude of the administrative sanction ranges from 30 to 60% of the unjustified support received. Moreover, the Ministry can issue regulations to define administrative sanctions further. Furthermore, the Act stipulates punitive measures under paragraph 13; specifically, Sections 378 to 380 of the Criminal Code on tax evasion. Depending on the severity of the violation, fines or imprisonment of up to 6 years can be imposed. *Source*: https://lovdata.no/dokument/NL/lov/2020-04-17-23 (in Norwegian).

Vis 10 linjer Filtrer på dato Eksporter som Søk:

Sak	Navn på søker	Org.nr.	Fylke	Næringskode	Tildelingsdato	Måned	Utbetalt (kr)
20	HAREID FYSIKALSKE INSTITUTT AS	911654156	Møre og Romsdal	86.902	20.04.2020	mars 2020	16 102
21	AKTIV TERAPI KIROPRAKTIKK OG IDRETTSKONSULENT AS	999620027	Oslo	86.909	23.04.2020	mars 2020	7 958
22	MONA MÅSTADSKAUG	992869828	Viken	96.020	03.06.2020	mars 2020	6 237
24	KIROPRAKTOR ØYSTEIN NORDGAARD	995805324	Viken	86.909	23.04.2020	mars 2020	12 411
32	ELM STUDIO BY JEANETT MOE	921116683	Nordland	96.020	03.05.2020	mars 2020	10 380
33	FYSIOTERAPEUT TONE JENNIE ERIKSEN	887115362	Nordland	86.902	23.04.2020	mars 2020	6 025
35	KJØRESKOLEN LEIV JARLE HÅBESLAND	912882888	Agder	85.530	22.04.2020	mars 2020	6 999
46	KLINIKK AUDREY HYNES	979717377	Møre og Romsdal	96.020	22.04.2020	mars 2020	6 426
47	EVENSEN BEAUTYSTUDIO	999128130	Oslo	96.020	22.04.2020	mars 2020	16 106
49	SALONG-PETIT AS	911575434	Agder	96.020	23.04.2020	mars 2020	178

Til toppen

Viser 1 til 10 av 76 606 linjer

Forrige 1 2 3 4 5 ... 7661 Neste

Fig. 1 Screenshot, public database with all approved cash support applications. *Source*: https://www.skatteetaten.no/kompensasjonsordning/innsyn/

organizational number, sector, and geographical info, as well as approval date, the month to which the support applied, and the approved amount of support, as shown in Fig. 1, which is a screenshot of the first page from the website.

The FCCS support recipient information was updated daily, downloadable, and widely used by investigative journalists and others. The Tax Administration stated in a press release that for the period April–August 2020, this transparency portal had 132,000 page views. During the same period, the tax administration opened audit cases for ten firms following reports of suspected fraud covered in the media.[3] Such transparency imposes a reputational risk to companies relying on a good reputation among their clients, adding a potential cost to receiving fixed cost compensation if the company may, for outsiders, appear as profitable. The company may also repay on its own initiative for other reasons. Until August 2nd, 384 firms voluntarily repaid 57 Million NOK of previously received support. In total, the Norwegian Tax Administration conducted around 1100 risk-based audits,[4] which resulted

[3] See https://www.skatteetaten.no/presse/nyhetsrommet/skatteetaten-roser-medienes-dekning-av-kompensasjonsordningen/.

[4] The Norwegian Tax Administration's audit strategies remain undisclosed. Nevertheless, we summarize available information from press releases of the Tax Administration, shared with us via email (available upon request), and from the Auditor General of Norway's audit of the tax administration (Office of the Auditor General of Norway, 2021). Before disbursing any compensation, applications underwent automated checks. These checks categorized applications into three categories: automatic approval, automatic rejection, or referral for manual review in cases where the fulfillment of support conditions was ambiguous or the applicant was deemed potentially risky. Identification of risky applicants was primarily sector-based, drawing from the Tax Administration's experience with fraud in other areas. At the firm level, risk assessment considered factors such as prior convictions or penalty taxes for fraudulent activities within the Tax Administration's internal records (information unavailable to us). The filtering criteria

in demands for re-payment of 160 Million NOK. In addition, 7 Million NOK were issued in penalties. This relatively low detected evasion rate, constituting 2.5% of all FCCS payments based on self-reported information, either indicates a truly low evasion rate or hints at potentially too few or too ineffective audits.

When a company received a claim for repayment, the information about it was removed from the overview of decisions on grants awarded (our source of data). The quantitative goal of the current paper is to analyze the remaining undetected misreporting and we will put these findings in context in the final section.

2.1 The exact rules for calculating the compensation

All firms with pandemic-induced turnover loss of at least 30% in any given month were eligible for the unavoidable fixed costs compensation scheme. The exception to that was the month of March, when the minimum turnover loss to qualify was 20%, recognizing that the beginning of the month was still "normal". The amount of compensation was a portion of unavoidable fixed costs adjusted for the firm's turnover loss, and it was calculated for each firm on a monthly basis. The regulation defined unavoidable fixed costs by reference to the specific accounting items in the income statement of the firm.[5] Important items were rent of premises, rental payment for machinery, fixtures and means of transport, public utility fees, insurance fees, IT costs, accounting services, heating, and net interest payments.[6]

The turnover loss was defined as the difference between the actual and the counterfactual normal turnover, where the latter was operationalized as the corresponding month's turnover last year, scaled up or down with a firm-specific and time-variant adjustment factor. The logic of the adjustment factor is that the trend in turnover differs across firms.

More specifically, the amount of compensation for each firm f in month m and year t was calculated as

$$C_{fmt} = \gamma_{fm}^{j}\left[\frac{E(Y_{fmt}) - Y_{fmt}}{E(Y_{fmt})}\right](F_{fmt} - \kappa_{fm}^{j}), \tag{1}$$

Footnote 4 (continued)
were continuously updated based on incoming information, including new risk areas or cases of fraud in other forms of Covid support. Most rejections stemmed from misunderstandings, such as inaccuracies in application submissions or misinterpretations of support conditions. Norwegian Tax Administration (2020) provides further insights into rejection reasons, citing issues like ambiguity regarding government-mandated closures, inconsistencies between application details and public statements, the inclusion of fixed costs for firms established in 2020 but with no previous financial records, and closures unrelated to Covid, such as renovation-related shutdowns. Post-payouts, the Tax Administration conducted numerous audits, focusing on perceived fraud risks based on sector and firm-level data from previous fraud cases and risk assessments. Additionally, audits were prompted by tips from the media and the public, leveraging publicly available lists detailing firms receiving support and their respective amounts.

[5] Please see regulations at https://lovdata.no/dokument/LTI/forskrift/2020-04-17-820 and income statement form at https://www.skatteetaten.no/globalassets/skjema/2020/rf-1167b.pdf (in Norwegian).

[6] See details in "Appendix D".

Table 1 Fixed costs compensation scheme (FCCS), March 2020–June 2021

Month	Eligibility criteria: Minimum sales loss	Compensation rate, (γ^j_{fm}) j = Closed	j = Not Closed	Fixed cost deduction, (κ^j_{fm}) j = Closed	j = Not Closed	Adjustment factor(a_f)
March 2020	0.2	0.9	0.8	0	10 000	Yes
Apr–Aug	0.3	0.9	0.7	0	5 000	Yes
Sept–Oct	0.3	0.7	0.7	0	0	No
Nov–June 2021	0.3	0.85	0.85	0	0	No

See https://www.regjeringen.no/no/aktuelt/legger-frem-ny-kompensasjonsordning/id2784471/ (in Norwegian only)

where the subscript j indicates one of the two possible types of industries that a firm may belong to: either closed by restrictions or not required to be closed. Variables entering the formula were as follows: γ^j_{fm} is the compensation rate, Y_{fmt} is self-reported turnover, F_{fmt} is the fixed cost, and κ^j_{fm} is a fixed cost deduction. The turnover loss is defined as the percentage difference between the counterfactual normal turnover ($E(Y_{fmt})$) of firm f in (the crisis) month m in year t and actual reported turnover (Y_{fmt}). The counterfactual normal turnover was defined as

$$E(Y_{fmt}) = a_f Y_{fmt-1} \quad \text{where} \quad a_f = \frac{Y_{f1t} + Y_{f2t}}{Y_{f1t-1} + Y_{f2t-1}}, \tag{2}$$

i.e., the monthly turnover from the previous year was scaled up or down with a firm-specific factor (a_f). This adjustment factor during the first phase of the program that we will rely on in the analysis was based on turnover in January and February of 2020 relative to the same two months of 2019. The idea behind it was to make it firm-specific, recognizing that trends in turnover may vary across firms. The adjustment factor was further winsorized at 0.8 (min) and 5 (max). For completeness, this formula was modified in the second phase of the program (which we do not use in this paper) from September 2020 onwards, when the counterfactual turnover was simply equal to turnover from the same month of the previous year (i.e., $a_f = 1$).

The various parameters in the compensation scheme over time and by sector are presented below in Table 1. For sectors considered to have been closed by the government through the lockdown, the compensation was more generous. The compensation rate (γ^j_{fm}) changed over time between 0.7 and 0.9 and varied depending on whether the industry was closed by the government or not. The deduction (κ^j_{fm}) was removed from September 2020.

The minimum payable compensation was NOK 5000 and the maximum was NOK 80 Million. For any compensation above NOK 30 Million only 50% of the amount exceeding this threshold was paid out.

2.2 Incentives to misreport

Businesses may be tempted to misreport while applying for government support to maximize their financial gains or fulfill eligibility requirements. This motive should lead to inflating the amount of assistance granted. The lack of rigorous verification processes makes it easier for firms to intentionally manipulate their reported data. Misreporting may also stem from a lack of comprehension or uncertainty regarding regulatory guidelines. Arguably, the presence of this latter type of mistake should not lead to systematically overstating the amount of assistance.

The compensation scheme offers clear incentives for strategic misreporting as businesses have the opportunity to increase their support amounts and receive immediate payouts by exaggerating reductions in turnover during the crisis and inflating pre-crisis growth figures, and overstating unavoidable fixed costs. Strategic misreporting may involve both under- and over-reporting of different variables. The turnover loss increases by underreporting today and overreporting the turnover last year. The adjustment factor gives the opposite incentives as the counterfactual growth is higher if this year's (January/February) turnover is overreported or if last year's turnover is underreported.

Using the publicly available application data, we can quantify these incentives directly in terms of the marginal effect on the compensation (exact formulas in "Appendix C") for each individual firm that we use in the analysis (we will describe the data in the next section). The average marginal effect of increased turnover during the pandemic is close to − 0.1. Thus, on average, if the firm reduced its reported turnover by 1 NOK, the compensation increased by 0.1NOK. The incentive to overreport turnover last year is weaker, with an average effect on compensation of 0.04, simply because the effect on relative turnover loss is smaller. The incentives for misreporting the January–February turnover in 2019 and 2020 are fairly symmetric and around 0.05 on average. These results are also reported in the last column of Table 2, which we will discuss below.

Opportunities for misreporting were increased because the government decided to use a brand new activity-based turnover concept that does not match either accounting or VAT definitions, the two natural and established approaches used in other tax and non-tax contexts. In the VAT and accounting approaches, turnover is booked at the time of sending out the invoice or receiving payment in direct customer-based sectors. The difference between the two is that there is no periodization for VAT turnover, while there is in the accounting data. Periodization means that an invoice for rent income for a year is distributed across several accounting periods rather than fully accounted for when the rent is due. Also, there are slight differences in which type of income constitutes turnover under various regulations.

In contrast, the pandemic turnover concept included only goods and services delivered in the month in question. Some other items, such as business transfers, including sales of equipment, are turnover in the VAT statement but are usually not part of the turnover that determines the cash support. Moreover, other grants/support from the Government/municipality (to sectors such as culture or transportation) are treated as turnover in the cash support calculation (to prevent firms from getting support from multiple sources) but not in the VAT.

Thus, the two other turnover concepts that are more easily available for both firms and tax administration did not match the one used for determining support.

This alternative turnover definition not only made automatic checks by the tax administration difficult, as there were no comparable turnover data in their systems, but it also made compliance more difficult, as the support-scheme turnover that needed to be reported had to be calculated using these new rules. This increased compliance costs for firms and the likelihood that eligible firms may choose not to apply due to perceived administrative difficulties.

3 Data

Our benchmark data source is the publicly available cash support database (FCCS data). This data reflects information after limited audits and other forms of fraud detection which led to the recapturing of some of the disbursed support by the tax authorities.

There are two main data challenges when analyzing any potential misreporting under this cash support scheme, the same challenges that also the tax administration faced:

(1) Lack of monthly turnover data for comparison;
(2) Different definitions of turnover concepts.

The activity-based concept of turnover used for determining cash support has no analog in administrative data sources in Norway.[7] In light of these two issues, the closest we can get is data for turnover from VAT reporting. Because the turnover concepts in application and VAT data differ, with adjustments in both positive and negative directions, we effectively treat the second issue as a source of randomness.

The VAT data is reported at bi-monthly frequency. Therefore, in what follows, we will also aggregate application information to the same frequency and use "term" to refer to a bi-monthly period, which addresses the first challenge. We focus on the first phase of the program between March and August, so that in 2020, there are three program terms—March/April, May/June, July/August—and the pre-program

[7] The concept of turnover under the FCCS is defined in Section 2-2 of the Regulations for the completion and implementation of the Act. Turnover encompasses income from sales of delivered goods and performed services. Payments replacing such income, including public subsidies and income protection during the pandemic, are considered turnover, with some exceptions that included subsidies for training, skills development, and infection control. Returns from capital and financial assets are excluded. The same applies to returns on real estate other than rental income. Further clarification on turnover was provided by the Tax Administration. The updates clarified that income from licenses, royalties, and franchise fees are considered as turnover. Special rules for the construction industry clarified how projects in progress enter the definition. Value-added tax and excise duties are not considered income. For sole proprietors and partners in partnerships, sickness benefits, parental benefits, and care benefits are to be regarded as turnover. Please see https://lovdata.no/dokument/SF/forskrift/2020-04-17-820/ and https://www.skatteetaten.no/en/kompensasjonsordning/regelverk/avklaringer/ (in Norwegian) for details and clarifications.

term of January/February that's used as the base for adjustment factor in the cash support formula.

Lastly, another element in the compensation formula is fixed costs. The closest one can get to this in administrative data is by selecting the most relevant components of the firms' annual accounts. However, accounting data for 2020 are not yet available to us. We, thus, leave the fixed costs out of the remaining exercise, assuming that fixed costs are reported correctly in the applications or—alternatively—quantifying the extent of noncompliance conditional on fixed cost reports.

Below, we describe each data source and sampling procedure in more detail.

3.1 Cash support

The cash support database is publicly available,[8] as described above. The Norwegian Tax Administration initially administered FCCS and, after our period of interest, passed it on to Brønnøysund with some changes to the scheme. In our analysis, we focus on the first phase of the program that was in place throughout March–August 2020. The data covers monthly approved applications with information about firm identification number, five-digit industry code, location, self-reported turnover in the application-relevant month(s) of 2019 and 2020, self-reported January–February turnover for 2019 and 2020, self-reported overall unavoidable fixed costs, compensation rate, and the amount of compensation received.

The initial dataset contains approximately 78,000 applications. We eliminate applications lacking essential turnover-related information and with no compensation. We also only keep applications from firms that applied in both months within the term. This process results in a refined sample of approximately 53,000 monthly applications. Finally, we aggregate monthly application information to the term level, which leaves us with 26,640 bi-monthly applications.

A simple test for the presence of misreporting pursued in academic public finance literature is to look for evidence of bunching at key policy thresholds. Reporting the loss of at least 30% (20% in March) was necessary to be eligible for the assistance. Hence—if misreporting is present—one might expect to see an unusual number of applications just above 30%. Such a pattern would indicate that some noncompliance happened on the take-up margin.

To test for the presence of such bunching, we plot the distribution of sales loss calculated from Eq. (1) using self-reported application data in Fig. 2. There is no visual evidence of bunching just above the threshold either in the first or in the following months, indicating that overstating turnover loss just to qualify for the scheme was not an important way of noncompliance and, more generally and previewing our further results, suggesting that noncompliance may not be in fact widespread in our data.

[8] Data can be downloaded here: https://www.skatteetaten.no/en/kompensasjonsordning/innsyn/.

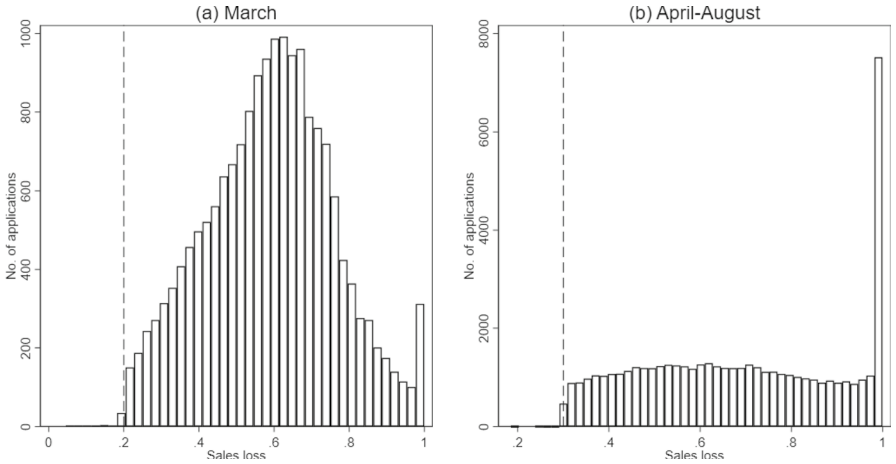


Fig. 2 Distribution of sales loss. Distribution of sales loss from Equation (1) using the raw data. Dashed lines represent the sales loss eligibility threshold of **a** 20% and **b** 30%

3.2 VAT and administrative data

We use VAT statements as the closest comparable alternative to the records in the application data. In addition to the annual tax statements, firms submit VAT forms on a bi-monthly basis. These forms include detailed information on turnover.[9] For our analysis, we construct VAT-based turnover by summing up information on domestic turnover and exports.[10] Moreover, the VAT reports are at the establishment-term level, and we aggregate them to the firm-term level. The VAT-based analogs of the four turnover elements in the self-reported FCCS application data are denoted as V_{fbt}, V_{fbt-1}, V_{f1t}, and V_{f1t-1}, where b denotes a bi-month (term), with b = 1 corresponding to the sum of January and February turnovers.

Note that not all firms are obliged to file tax forms. Some goods and services are not subject to value-added tax (VAT), so only the vatable part of the firm's turnover is included. There also are some monetary limitations (NOK 50,000, not including VAT, for enterprises and NOK 140,000, not including VAT, for charitable and non-profit organizations) for firms to be registered in the Tax Administration Office. Moreover, some sectors, such as the financial sector, are VAT-exempt. As a result, not all application data matches with the VAT data. The sample is further restricted to applications with non-missing and positive VAT records. Additionally, we exclude joint applications.[11] Our final sample consists of 14,667 applications (firm-term observations).

[9] See form RF-0002 at https://www.skatteetaten.no/en/forms/ for all variables that can be observed.

[10] These correspond to fields 3–8 in the tax form.

[11] Enterprises/establishments that are part of one company/firm can apply as a group 'as if a group is one company,' and the applications are based on consolidated accounts. Though we can observe which applications are part of the group, we cannot identify those groups.

Table 2 Application data descriptives

	Mean (median)	SD	Marginal comp. effect
Reported sales			
Pandemic (Y_{fbt})	1010 (257)	6034	−0.102
Last year same bi-month (Y_{fbt-1})	2589 (765)	19,179	0.041
Jan–Feb 2020 (Y_{f1t})	2201 (653)	14,350	0.052
Jan–Feb 2019 (Y_{f1t-1})	1969 (575)	13,485	−0.053
Fixed costs-(F_{fbt})	234 (99)	909	0.513
Support received-(C_{fbt})	111 (44)	433	

Summary statistics on bi-monthly applications in our sample. $N = 14,667$ and $f = 11,752$. In 1000 NOK (approximately 100 USD). The last column presents marginal effects from Eqs. (C.1a) to (C.1e)

The sample construction steps are presented in Table 8. This table also reveals that applications were not balanced across terms—many more firms applied for assistance in March/April than in the following months.

3.3 Comparison of cash support and administrative data

We start by describing the five items that determined the compensation. Table 2 reports the means and medians of the bi-monthly items in the application data.

The average (self-reported) turnover during the first phase of the pandemic dropped by 61% compared with the same period in 2019. Turnover in the last pre-covid months was up 12% from the previous year, i.e., an average adjustment factor of 1.1. Fixed costs were 9.0% of turnover the year before. The average cash compensation was about 47.4% of the fixed costs. The large difference between the means and medians reveals substantial skewness in turnover. Many firms are small and the average turnover is much larger than for a typical firm.

Next, we compare the turnover from applications with their VAT analogs. Figure 3 illustrates the distributions of turnover, and summary statistics of turnover elements are given in Table 3. The distributions are not identical, but they are close.

As discussed above, applicants would increase their fixed cost compensation (FCCS) if they can exaggerate the reduction in turnover during the crisis or their pre-crisis growth. We proceed by inspecting the difference between each of these four reporting items and their analogs from the VAT data. This is shown in Table 4 that presents log differences in application-reported and VAT items.

For the initial months of the pandemic (March–April 2020), the mean difference between the turnover reported in the FCCS applications and the one in the VAT data is approximately 5.4%. Despite the incentives to under-report, FCCS turnover is larger. 48% of the applicants report lower turnover in the FCCS application than recorded in the VAT data. A similar pattern is present in other months of 2020. This suggests that the systematic differences in the definitions of the two concepts may be important, something that we will return to below.

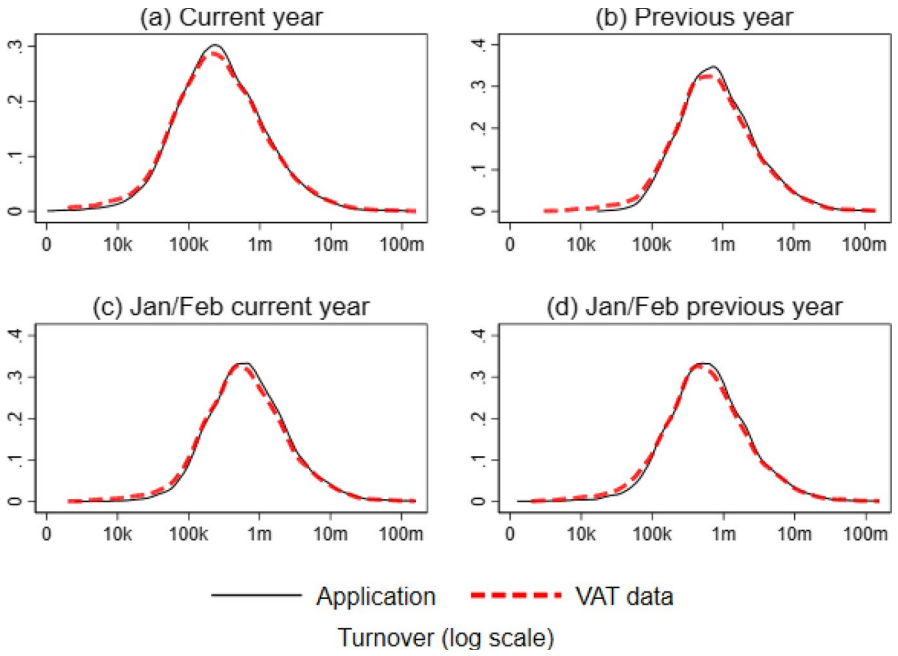


Fig. 3 Application vs. VAT data. The distributions of bi-monthly turnover in FCCS and VAT data

Table 3 Key descriptives

NOK 1000	Average	Standard deviation	Median
Turnover today			
Y_{fbt}	1010	6034	257
V_{fbt}	1053	6142	248
Turnover previous year			
Y_{fbt-1}	2589	19,179	765
V_{fbt-1}	2590	19,475	701
Turnover Jan-Feb this year			
Y_{f1t}	2201	14,350	653
V_{f1t}	2171	14,342	590
Turnover Jan-Feb previous year			
Y_{f1t-1}	1969	13,485	575
V_{f1t-1}	2002	13,790	528

Summary statistics on bi-monthly turnover in applications and VAT. $N = 14,667$ and $f = 11,752$. In 1000 NOK

However, when we look at the reports for 2019, for which the applicant would benefit from exaggerating the firm's turnover and compare them to pandemic months (March–August 2020), the differentials are much larger, in line with the incentives. On average, the corresponding difference between turnover in the

Table 4 Reported sales differentials (FCCS-VAT data)

Term	Year	Misreporting incentive	Log(Sales FCCS)-Log(Sales VAT)		
			Mean	St.dev	Sales FCCS > Sales VAT
March–April	2020	Under	0.054	0.645	0.520
	2019	Over	0.122	0.571	0.579
May–June	2020	Under	0.107	0.830	0.553
	2019	Over	0.169	0.696	0.614
July–August	2020	Under	0.053	0.694	0.536
	2019	Over	0.125	0.541	0.617
Jan–Feb	2020	Over	0.128	0.622	0.594
	2019	Under	0.112	0.620	0.578

Summary statistics on the difference in log turnover between applications and VAT records. The last column shows the share of positive differences

application and VAT data is 13.8%, and 60% of applications report higher turnover in the application than the registered value in the VAT registry.

The last two lines show a similar comparison for the turnover in January/February when the incentives are flipped—it is valuable to overreport in 2020 and underreport in 2019. Here, the difference between 2019 and 2020 reports is much smaller.

We make two conclusions based on the information in Table 4. First, there is a mismatch between VAT and application data that results in application turnover being systematically larger throughout. Second, this gap appears to systematically move with incentives to misreport in the direction beneficial to applicants. We will test this second notion formally next and return to the role of the mismatch between the two data sources in the next section.

The notion that gaps between VAT and application reports move systematically with incentives to misreport can be tested more formally using a simple linear regression model. We decompose the reported sales differential between the two data sources into (i) differences in means arising from the sources measuring two somewhat different turnover concepts, (ii) differences across years, (iii) differences across seasons (terms), (iv) differences in line with the incentives to misreport.

To do so, we stack the data for each application (for a given firm in a given term) in a long format such that it yields four observations corresponding to gaps in four variables relevant for calculating cash assistance, i.e. differences between turnover self-reported in the FCCS application and turnover reported in the VAT registry for (i) the application period (March/April, May/June or July/August) 2020, (ii) the same period last year, (iii) January/February 2020, (iv) January/February 2019.

We then estimate the following baseline regression model:

$$y_{fbt} = \beta_0 + \beta_1\, year_t + \beta_2\, term_b + \gamma\, K + \varepsilon_{fbt}, \tag{3}$$

where *year* and *term* capture the year and period (bi-month) the observation refers to. We have four observations for each firm period. K is a dummy variable equal to 1 for the periods where the applicant would benefit from reporting a higher turnover than what is true and 0 otherwise. Hence, $K = 1$ in January/February 2020 as well as for all months other than January/February 2019. Note also that the model can be further saturated with application fixed effects or even with fixed effects for *year* × *application* and *term* × *application*. We estimate the model for two outcomes: (1) the log difference between turnover reported in the application and VAT registry and (2) a dichotomous outcome for whether this difference is larger than zero. The results are reported in Table 5.

In column (1) we display results from estimating Eq. (3) using OLS. The coefficient for K tells us that, on average, the difference in log turnover between the FCCS application and the VAT report is 4.1% higher in periods when this benefits the applicant. Standard errors are clustered at the firm level, and the estimated coefficient is highly statistically significant. The inclusion of various forms of fixed effects related to applications has no impact on this key result—not surprisingly, because the relevant mean comparisons always rely on within rather than across applications variation.

Finally, in column (4) we include the interactions to determine any variation by term and, despite large differences in the take up of the program in different terms (see Table 8), there are no substantial differences in effects.

In columns (5)–(8), we repeat the same exercise for the dichotomous outcome. We see that the probability of a deviation between FCCS turnover and VAT registry is positive and approximately 4.5% points higher when the difference would imply increased compensation.

We take this as clear evidence of the existence of strategic misreporting. In the next section, we will investigate how this behavior translates into increased compensation in the FCCS.

4 Quantifying the scope of misreporting

In this section, we quantify the impact of misreporting on the total amount of cash compensation received by firms. To do so, we calculate the counterfactual without misreporting based on VAT data by using Eq. (1) and the four turnover items from the VAT registry. Since we do not observe counterfactual fixed costs, we rely on the information in the application.

As mentioned before, the concern about using the VAT information as the counterfactual is that it uses a different definition than the one used in the application. We illustrated in the previous section that while there is evidence that VAT vs. applications reporting gaps vary in ways consistent with misreporting incentives, the absolute levels of these gaps are skewed in the direction of turnover as reported in the application data being higher than in the VAT data.

We proceed as follows. First, we note that the formula itself is invariant to the proportional differences between different definitions. Thus, a counterfactual that's based on uncorrected VAT may still be informative. Second, we consider

Table 5 Regression results

	Log difference				Dummy var.			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Term								
2	− 0.032***	− 0.025***			− 0.028***	− 0.024***		
	(0.004)	(0.003)			(0.005)	(0.005)		
3	0.017*	− 0.022**			− 0.003	− 0.016		
	(0.011)	(0.010)			(0.008)	(0.010)		
4	− 0.031**	− 0.016			− 0.010	− 0.009		
	(0.013)	(0.013)			(0.010)	(0.014)		
Year	− 0.026***	− 0.026***			− 0.029***	− 0.029***		
	(0.003)	(0.004)			(0.004)	(0.005)		
K	0.041***	0.041***	0.041***		0.045***	0.045***	0.045***	
	(0.003)	(0.003)	(0.003)		(0.004)	(0.005)	(0.004)	
Term × K								
2 × K				0.041***				0.044***
				(0.003)				(0.004)
3 × K				0.039***				0.046***
				(0.008)				(0.009)
4 × K				0.049***				0.052***
				(0.010)				(0.011)
Constant	51.851***	51.851***	0.088***	0.088***	58.582***	58.582***	0.553***	0.553***
	(7.044)	(8.134)	(0.001)	(0.001)	(8.290)	(9.573)	(0.002)	(0.002)
Application FE	No	Yes	No	No	No	Yes	No	No
Application × Year FE	No	No	Yes	Yes	No	No	Yes	Yes
Application × Term FE	No	No	Yes	Yes	No	No	Yes	Yes
Obs	58,060	58,060	58,060	58,060	58,060	58,060	58,060	58,060

Table 5 (continued)

	Log difference				Dummy var.			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
R-squared	0.002	0.789	0.940	0.940	0.004	0.381	0.800	0.800

Standard errors in parentheses, clustered at firm level

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Estimates of Eq. (3) with (i) the difference in log sales between the FCCS and VAT registry records (Columns 1–4) and, (ii) binary variable for whether this difference is larger than zero or not (Columns 5–8) as dependent variables

correcting VAT-based reports for the systematic difference between the two sources. The results that we obtain from these two different ways of utilizing VAT data are very similar giving us confidence that despite differences in the definitions, the VAT-based counterfactuals are a reasonable approach statistically.

Our results are based on comparing the actual compensation and compensation under the constructed counterfactuals. Because both of these are constructed using different concepts, we would not expect them to be precisely identical even under perfect compliance. However, we might expect that the differences (especially after adjustments to match means of turnovers in the two datasets) should be akin to noise. Hence, we are going to investigate the shape and asymmetry of the distribution of the differences between actual and counterfactual compensation.

To understand the strengths and limitations of this approach it is helpful to inspect the properties of the FCCS and how the compensation depends on the four different turnover items as specified in Eqs. (1) and (2). Note, first, that the FCCS formula is metric invariant as a proportional increase of all four arguments has no impact on the compensation. Thus, if the VAT turnover and the "true" turnover differ only by a constant multiplicative firm-specific factor, the calculated compensation would be identical in the absence of misreporting. The same argument holds for "seasonal variation" as a proportional increase in the same-term arguments across years gives the same compensation. Proportionality is not an unreasonable assumption to consider—we are relying on multiple bi-monthly values of two different turnover concepts for the same firm, and the (proportional) gap need not necessarily be time-varying. In contrast, misreporting incentives generate a reason for the gap to vary systematically.

The remaining concern though is that the relationship between the VAT turnover and the correctly measured turnover as defined for the purpose of cash applications has in fact changed from 2019 to 2020 in ways that are heterogeneous across firms. In order to consider that possibility and test the robustness of the approach, we consider re-scaling the VAT components by a firm/term/year-specific factor μ_{fbt} defined as

$$\mu_{fbt} \equiv \frac{Y_{fbt} + Y_{f1t}}{V_{fbt} + V_{f1t}} \tag{4}$$

i.e., by re-scaling the VAT components by the ratio of reported turnover and VAT turnover for a given term b and the (always covid-unaffected) first term of the year. Note that this re-scaling factor is different for 2019 and for 2020 and therefore cash compensation is not neutral to it. While this re-scaling sets the average adjusted VAT-based turnover between January/February and term b to be identical to the self-reports, its values for specific terms are not going to match self-reports. We are thinking about it as a sensitivity check: while one might expect that this approach might shrink the difference between compensation based on application data and the counterfactual, the lack of a large effect along these lines would indicate that the counterfactual calculations based on VAT-constructed turnover are quite robust to definitional issues.

Table 6 Total compensation from FCCS

NOK 1000	Pooled	Term		
		Mar/Apr	May/Jun	Jul/Aug
Actual (application), C^Y	1,596,146	989,303	391,587	215,256
Counterfactual VAT, C^V	1,515,061	933,337	380,351	201,374
Counterfactual adjusted VAT, $C^{Vadj.}$	1,515,216	932,315	381,919	200,983
Number of applications	14,667	10,308	2754	1605

Total compensation calculated using actual and counterfactual sales. In 1000 NOK

The three total compensations: actual, VAT-based, and based on adjusted VAT are presented in Table 6.

With the (unadjusted) VAT data as counterfactual, we estimate the excess compensation from the FCCS due to misreporting to be approximately 5%. If we instead use the counterfactual based on the adjusted VAT data, the estimated misreporting is almost identical, suggesting that systematic differences in firm trends across the years are not in fact quantitatively important for calculating the compensation. This is also true for each individual bi-monthly term separately. The relative difference between the given amount and the counterfactual, the excess support, is also shown in Fig. 4.

In Fig. 5 we provide a scatter plot comparing compensation from application data with the two counterfactuals. We see that there is very little, if any, misreporting among the highest receivers, typically very large firms, but there is a substantial appearance of asymmetry in the distribution at lower compensation levels. These differences are systematically pointing toward too much compensation, regardless of which approach we use, with over 54% of observations above the diagonal and a large number of observations far away from the diagonal. "Appendix Fig. 8" shows the same graphs by term, suggesting that these discrepancies may have been especially prevalent in the initial months, although they are present throughout. We inspect whether this 'additional mass' may be the result of measurement error by creating a simulation exercise outlined in "Appendix E". We show that increasing the measurement error pushes the results toward more symmetry, i.e., acts against our findings (though it does so fairly slowly). Hence, we would not expect that going in the other direction by removing the measurement error could eliminate asymmetry, and we also don't think that quantitative bias due to the presence of the measurement error is likely to be very large.

In Fig. 6, we distill the scatterplot to the distribution of the log gap between compensation based on applications and counterfactual based on (unadjusted) VAT (censored at 0.5 for presentation purposes). Consistently with the visual evidence from the scatterplots, the difference is skewed toward positive values. The bottom panel shows the same information but superimposes the distribution's left- and right-tails on each other to demonstrate this asymmetry. Figure 7 in the appendix is based on adjusted VAT and looks very similar.

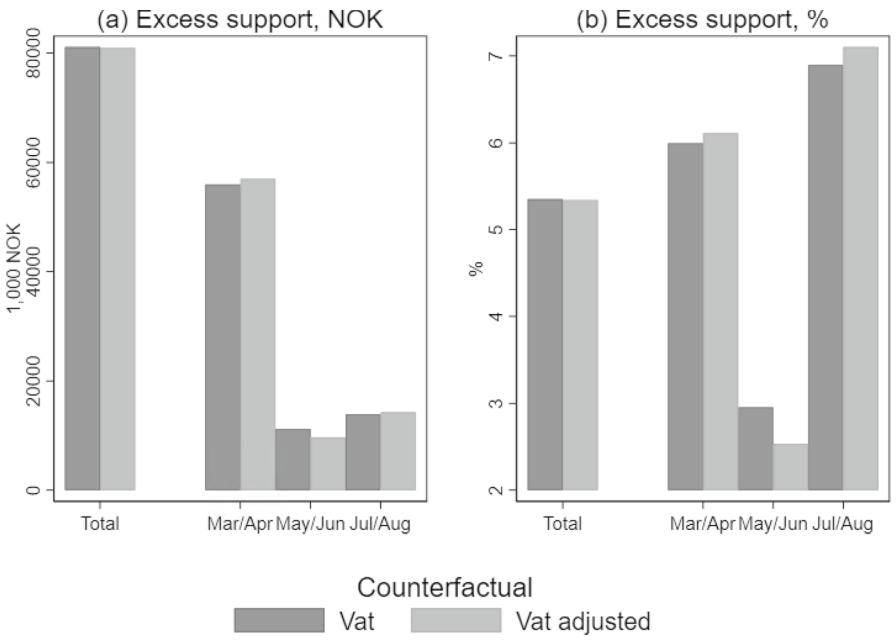


Fig. 4 Estimated excess compensation from misreporting. The difference between the given and the counterfactual compensation amount—the excess compensation—**a** 1000 NOK and **b** percent

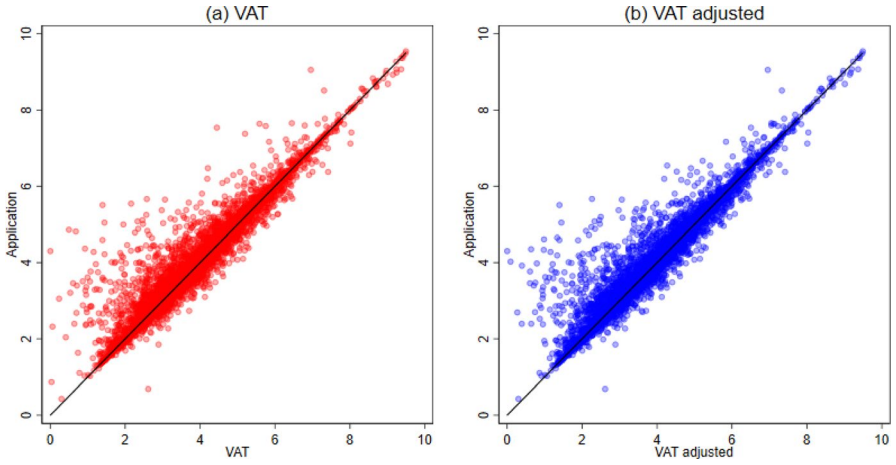


Fig. 5 Pairwise comparison of actual vs. counterfactual FCCS. Scatter plot for compensation from application and counterfactual sales (in logs). Share above the 45-degree line **a** 54.2% and **b** 54.4%. Figure 8 plots the scatter plot by term

To further inspect which firms misreport, we estimate a simple linear regression model associating misreporting with firm characteristics. The results are presented in Table 7.

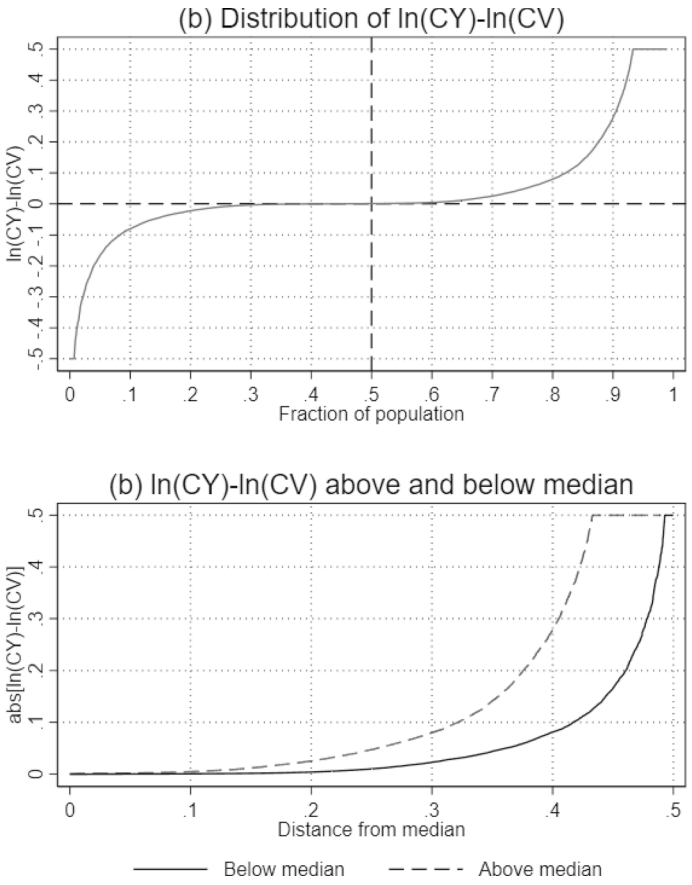


Fig. 6 Distribution of log difference in compensation from Application and (unadjusted) VAT. The distribution of the log difference in compensation from application and VAT, censored at 0.5. Distribution of the log difference in compensation from application and adjusted VAT is in Fig. 7

In the first two columns, the outcome is simply the difference between the FCCS applied for and the simple counterfactual based on VAT data, i.e., our estimate for misreporting. In the three following columns (3)–(5), the outcomes are indicator variables for whether the estimated misreporting exceeds 5, 20, and 50%, respectively.

In the upper part of the table, we have coefficients for firm size. The omitted category is micro firms (the largest category), and we estimate coefficients for three size categories: "small," "medium," and "large". There is not much evidence of an association between misreporting and firm size.

The next set of coefficients relates to firms' industries. The reference, and also the largest category, is retail. For tourism, dominated by hotels and restaurants, the industries hit the hardest by the lockdown, we find no—or very small—differences compared to retail. For some industries, we do however observe that

Table 7 Regression results

	(1) ln(CY) − ln(CV)	(2) ln(CY) − ln(CV)	(3) ln(CY) − ln(CV) > 0.05	(4) ln(CY) − ln(CV) > 0.2	(5) ln(CY) − ln(CV) > 0.5
Size					
Small	− 0.001	− 0.010	− 0.012	− 0.002	− 0.001
	(0.010)	(0.011)	(0.015)	(0.011)	(0.007)
Medium	0.081*	0.064	0.030	0.061*	0.022
	(0.046)	(0.047)	(0.043)	(0.035)	(0.027)
Large	0.007	0.010	0.029***	0.010	0.008
	(0.007)	(0.007)	(0.011)	(0.008)	(0.005)
Sector					
Tourism	− 0.003	− 0.004	− 0.008	− 0.009	− 0.009**
	(0.006)	(0.006)	(0.011)	(0.007)	(0.005)
Transportation	0.025**	0.025**	0.062***	0.045***	0.013*
	(0.012)	(0.012)	(0.017)	(0.012)	(0.008)
Admin & support services	0.052***	0.053***	0.108***	0.088***	0.043***
	(0.017)	(0.017)	(0.021)	(0.016)	(0.011)
Other services	− 0.008	− 0.003	0.007	0.004	− 0.004
	(0.007)	(0.007)	(0.012)	(0.008)	(0.005)
Construction	0.068***	0.068***	0.142***	0.117***	0.060***
	(0.017)	(0.017)	(0.023)	(0.018)	(0.013)
Manufacturing	0.038***	0.037***	0.036*	0.038***	0.028***
	(0.014)	(0.014)	(0.020)	(0.014)	(0.010)
Others	0.058***	0.058***	0.114***	0.100***	0.049***
	(0.011)	(0.011)	(0.013)	(0.010)	(0.007)
Term					
3	− 0.011	− 0.011	0.012	− 0.007	− 0.010**
	(0.007)	(0.007)	(0.009)	(0.007)	(0.005)
4	− 0.003	− 0.002	0.015	0.013	0.002
	(0.009)	(0.009)	(0.012)	(0.009)	(0.007)
ln(FC)		0.007*	0.022***	0.012***	0.010***
		(0.004)	(0.005)	(0.004)	(0.003)
Constant	0.035***	0.002	0.076***	0.009	− 0.018
	(0.005)	(0.020)	(0.026)	(0.019)	(0.013)
R-squared	0.009	0.009	0.020	0.026	0.018
Obs	14,365	14,365	14,365	14,365	14,365

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Firm size categories follow standard EU SME definition. Micro-firms are the reference group. Sector groups are based on the NACE Rev.2 classification. The sector reference group is wholesale and retail trade. The reference term group is term 2 (March–April)

Table 8 Sample selection

	Step	No. of applications	No. of firms	Frequency
1	Raw applications	78,271	34,016	Monthly
2	Non-missing sales and strictly positive compensation	77,492	33,638	Monthly
3	Keep applications for firms that applied both months within bi-month	53,280	20,967	Monthly
4	Treat 2 applications in bi-month as one, i.e. aggregate monthly data to bi-month	26,640	20,967	Bi-monthly
5	Match with bi-monthly VAT data	21,294	16,148	Bi-monthly
6	Non-missing and positive VAT records	18,914	14,613	Bi-monthly
7	Drop joint/group applications	14,667	11,752	Bi-monthly
	Final sample	14,667	11,752	Bi-monthly
	By term	10,308		Term 2
		2754		Term 3
		1605		Term 4

Steps that we undertake to transition from raw data to the sample used in the analysis

misreporting seems more frequent, in particular in construction, transportation, administrative and support services (NACE N), and industry production.

The next group of coefficients relates to calendar time. The reference is March/April, and we then test whether May/June and July/August differ from it. There is no statistically significant evidence of time differences.

Next, we include the log of firms' fixed costs (from the applications) $\ln(FC)$ to partially account for heterogeneity across applications. This variable itself is potentially endogenous because of self-reporting (although we are not attempting a causal interpretation here), but—regardless—it has almost no impact on any of the other coefficients. We find that higher fixed costs are associated with misreporting.

5 Conclusions

Our findings indicate that there were strong incentives for misreporting, but they are visible in the applications after enforcement activities only to a limited extent. There is no evidence of bunching at the 30% threshold for eligibility. There is robust evidence of monthly turnover reported to the tax authorities deviating from VAT-based measures in ways that go systematically in the direction of increasing compensation. Correspondingly, when the counterfactual compensation is compared to the claimed one, there is evidence of strong asymmetry in the direction of overclaiming support. At the same time, arguably, these effects are not very large. We find that 54% of observations in our data receive compensation above the counterfactual and 46% below it (Fig. 5), which is consistent with 96% of the population reporting on average correctly and 4% overreporting and, similarly, we estimate the 4% misreporting

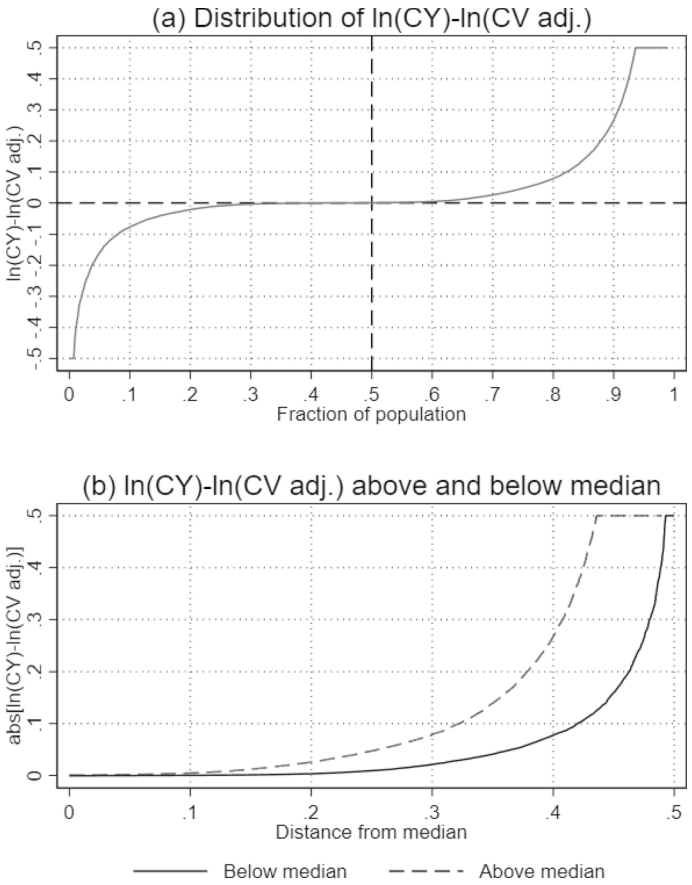


Fig. 7 Distribution of difference in compensation from Application and adjusted VAT. Distribution of the log difference in compensation from application and adjusted VAT, censored at 0.5

of individual turnover items in the direction increasing compensation (Table 5). The aggregate compensation difference (Table 6) is only about 5%.

Given the ad hoc nature of the program and hard-to-enforce criteria, we view these numbers as relatively small. In particular, the order of magnitude is comparable to the reported tax gaps overall in Scandinavian countries and much smaller than the overall tax system gap of well above 10% in the U.S. as estimated by the IRS.

There are some ways in which our estimates are an understatement. We condition on the reported level of fixed costs that were the basis for compensation, because we do not have an alternative source of information about such costs—this is the remaining important source of non-compliance that we do not account for. On the other hand, we do not have a good way of detecting imperfect take-up or false positive rejections—there may have been firms that were eligible but did not apply or firms that were truly eligible and were rejected.

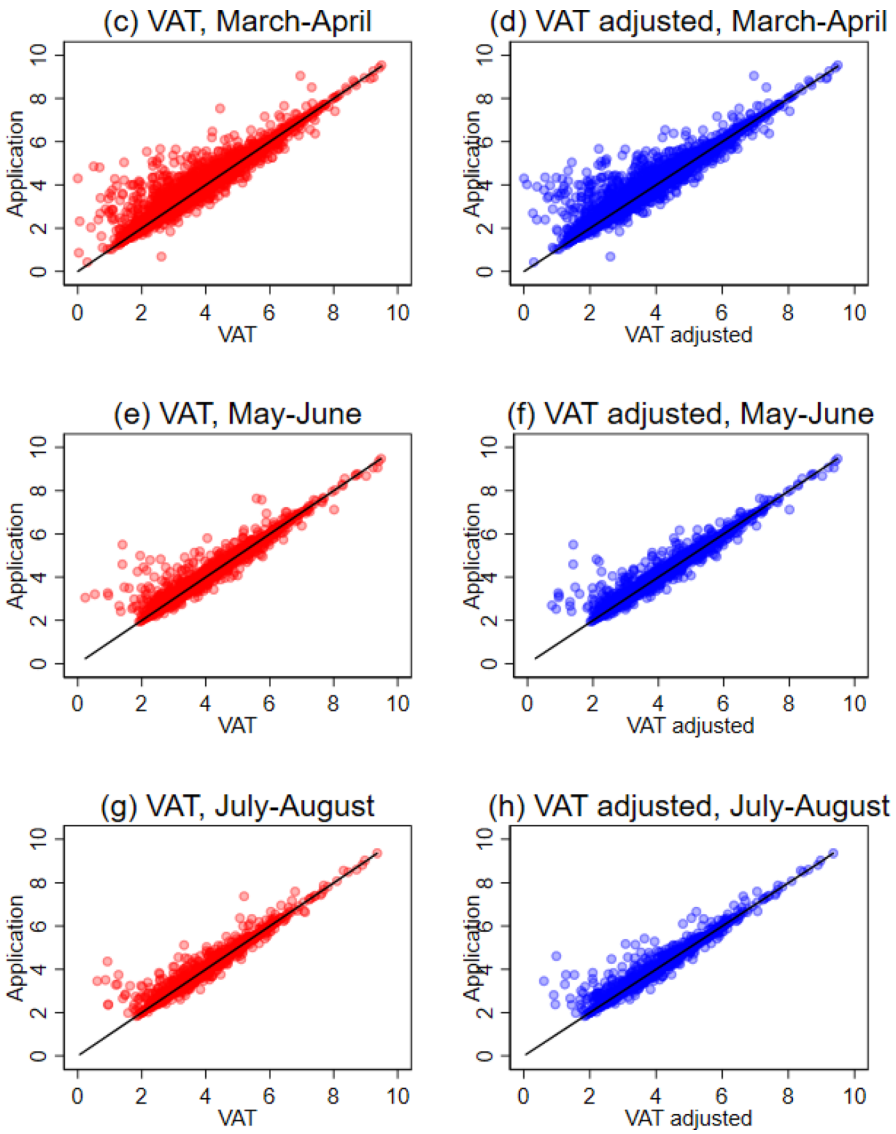


Fig. 8 Pairwise comparison of actual vs. counterfactual FCCS, by term. Scatter plot for compensation from application and counterfactual sales (in log) by term

To put all of this in context, recall that we looked at the remaining applications after an initial eligibility test and later risk-based operational audits. About one in four applications were not approved, accounting for about 1.9 Billion NOK that was not paid out (although some of those applications may have been re-submitted). The total compensation that was actually paid during the March–August period that we focus on was 6.5 Billion NOK. The 1100 operational audits led to a re-payment of

160 Million NOK and a penalty tax of 7 Million NOK. Thus, the audits disclosed that 2.5% of the overall compensation was not legitimate. Some firms also voluntarily paid back with a total re-payment of 106 Million NOK (Rønneberg and Lambrechts, 2023), presumably to avoid unfavorable reputation effects following media exposure. Treating this form of repayment as part of original noncompliance would raise the noncompliance rate before reaching our data to about 4.2%.

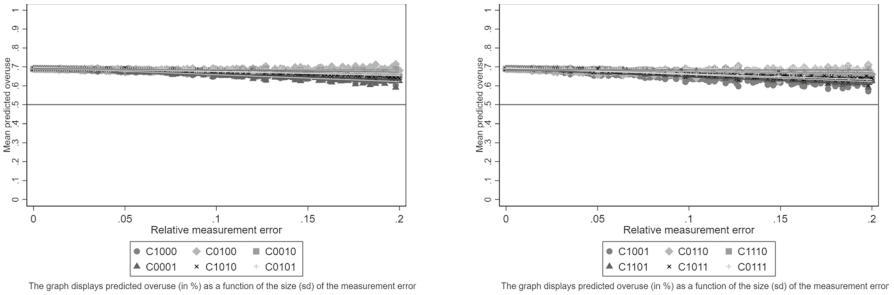
Overall, our estimates of the net gap (after enforcement activities) of about 5% should be viewed as being on top of the Tax Administration and otherwise discovered noncompliance discussed above, for the total gross (before enforcement activities) tax gap of 9–10%. One could further inflate this number by including part of the 1.9 Billion NOK rejected initially, although this is much more speculative.

While non-compliance would not be a problem in the ideal world, the 10% noncompliance rate, half of which was recovered, is not a particularly bad outcome. The design of the program created issues that did not have to exist and that added to noncompliance and administrative costs. Most importantly, by attempting to have a presumably more targeted measure of turnover drop, policymakers chose to rely on the non-standard concept of turnover that cannot be automatically verified, and that's likely complicated to verify even during audits. This is not an unusual choice—in many contexts, taxation depends on such metrics (for example, in the context of taxation of multinationals, when dealing with self-reported activities in the context of an income tax, when requesting valuation for wealth tax purposes) and they give rise to opportunities for non-compliance and costly enforcement. While sometimes such decisions may not be avoidable, in the case of this particular program using a definition that would stay closer to the VAT or accounting concepts would reduce the scope for manipulation. The same choice has likely also added to the compliance costs—when a quarter of applications is rejected (even if some of them get resubmitted and are successful then), this is revealing that much of taxpayers' effort was pure waste that, by Tax Administration's own assessment, stemmed from mistakes and confusion that likely could have been reduced by relying on less ad hoc base. At the same time though, the choice not to require accountant certification has plausibly reduced barriers to applying with ambiguous consequences—on the one hand, it may have been behind a large number of rejected applications, but on the other hand, it likely made the program more timely and quicker with—as our evidence suggests—only moderate non-compliance consequences.

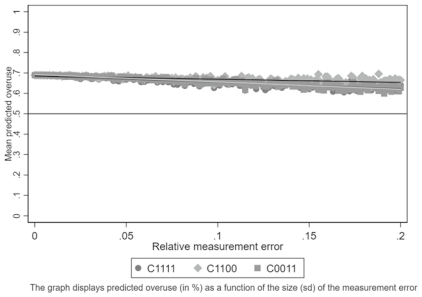
Appendix

Additional Tables and Figures

See Table 8 and Figs. 8 and 9.



(a) Perfectly correlated errors



(b) Independent errors

Fig. 9 Measurement error simulation

Conditions to qualify for the compensation scheme

C1. AS or ASA in VoF Foretak March 2020
C2. Publicly notified accounts from 2018
C3. At least one employee
C4. Positive fixed costs
C5. Positive profits in 2019
C6. Non-eligible industries:

- NACE 06 - Extraction of crude petroleum and natural gas
- NACE 64-66 - Financial and insurance activities (K)
- NACE 51 - Air transport
- NACE 35 - Electricity, gas, steam and air conditioning supply (D)
- NACE 861 - Hospital activities
- O - Public administration and defense
- P - Education
- R - Arts, entertainment, and recreation
- S - Other services activities

Marginal effects of sales and fixed costs

More specifically, analytical expressions for these incentives are provided by Eqs. (C.1a) to (C.1e): a. Turnover today - Y_{fmt}

$$\frac{\partial C}{\partial Y_{fmt}} = -\gamma \frac{(Y_{f1t-1} + Y_{f2t-1})}{(Y_{f1t} + Y_{f2t})Y_{fmt-1}}(F - \kappa) = -\gamma a^{-1} \frac{1}{Y_{fmt-1}}(F - \kappa); \tag{C.1a}$$

b. Turnover last year - Y_{fmt-1}

$$\frac{\partial C}{\partial Y_{fmt-1}} = \gamma \frac{Y_{fmt}(Y_{f1t-1} + Y_{f2t-1})}{Y_{fmt-1}^2(Y_{f1t} + Y_{f2t})}(F - \kappa) = \gamma a^{-1} \frac{Y_{fmt}}{Y_{fmt-1}^2}(F - \kappa); \tag{C.1b}$$

c. Turnover Jan–Feb 2020 - $(Y_{f1t} + Y_{f2t})$

$$\frac{\partial C}{\partial (Y_{f1t} + Y_{f2t})} = \gamma \frac{Y_{fmt}(Y_{f1t-1} + Y_{f2t-1})}{(Y_{f1t} + Y_{f2t})^2 Y_{fmt-1}}(F - \kappa) = \gamma a^{-1} \frac{1}{(Y_{f1t} + Y_{f2t})} \frac{Y_{fmt}}{Y_{fmt-1}}(F - \kappa); \tag{C.1c}$$

d. Turnover Jan–Feb 2019 - $(Y_{f1t-1} + Y_{f2t-1})$

$$\frac{\partial C}{\partial (Y_{f1t-1} + \mathbf{Y}_{f2t-1})} = -\gamma \frac{Y_{fmt}}{(Y_{f1t} + Y_{f2t})Y_{fmt-1}}(F - \kappa) = -\gamma \frac{1}{(Y_{f1t} + Y_{f2t})} \frac{Y_{fmt}}{Y_{fmt-1}}(F - \kappa); \tag{C.1d}$$

e. Fixed costs - F_{fmt}

$$\frac{\partial C}{\partial F_{fmt}} = \gamma \frac{E(Y_{fmt}) - Y_{fmt}}{E(Y_{fmt})} = \gamma \frac{a_f Y_{fmt-1} - Y_{fmt}}{a_f Y_{fmt-1}}. \tag{C.1e}$$

Fixed costs

In the regulations, fixed, unavoidable costs are defined as qualifying for compensation to the extent that they can be attributed to 10 specified items in the business statement. Typical costs for these items are; rental of premises, light and heating, renovation, water, drainage, cleaning, rental of machinery, equipment and means of transport, accounting and audit fees, electronic communication, insurance and tax on means of transport, dues and insurance premium. In addition, net interest costs are compensated.

In practice, this applies to the following items: 6300 (rental of premises), 6310 (lease of a car), 6340 (light, heating), 6395 (renovation, water, drainage, cleaning), 6400 (rental of machines), 6700 (limited for audit and accounting costs), 6995 (electronic communication), 7040 (insurance and transport fees), 7490 (quotations), 7500 (insurance), 8150-8050 (the net amount of interest expenses and interest income).

Limitations in coverage:

1. Only costs for business premises are included. This means that renting other types of property/premises is excluded. The part of the rental cost that is turnover-based is also excluded. Income from the subletting of premises is deductible.
2. Only public fees related to waste disposal, water, drainage and cleaning are included. When this type of cost is included in joint costs for renting business premises, it is assumed that only the part of the cost that applies to fees to the public sector can be included. Property tax cannot be included, although it is not unusual for the farm owner to pass this on as part of the joint cost.
3. Only costs for accounting and auditing, including assistance with the preparation of reports to the public sector, are included. Other extraneous benefits, such as guard duty, are not counted.
4. Only quotas that are tax-deductible are taken into account.
5. Insurance premiums are included, but not personnel insurance or occupational injury insurance.
6. License costs for software are included, but only the fixed basic price. If the license cost has a variable part (unit price per transaction, manufactured unit or similar variable cost), this shall not be included. Licensing costs related to rights to production and sales are not included.
7. Only interest costs on debt to banks and credit institutions and bond loans are counted. If you have other interest-bearing loans, the interest cost only counts for the part of the interest cost that has a counterpart in an equally large interest cost with the lender.
8. Only costs entered into by agreement before 1. March 2020 can be included. Exceptions to this are costs for accounting and auditing.
9. Price adjustments of agreements beyond the normal price adjustment after 1 March cannot be included
10. Costs related to time-limited assignments or deliveries cannot be included.
11. Costs which, according to the company's accounting standard, must be entered in the balance sheet, cannot be included. An example here could be building loan interest.
12. Costs that are refunded after being waived are not included. Discounts and price reductions are therefore deducted. This means, for example, that if the rent is reduced in the relevant period, it is the reduced rent that must be used.

As can be seen from the points above, there are significant limitations in which costs are compensated in relation to what many would immediately assume. Another factor is that, as mentioned at the outset, it may happen that costs that can actually be compensated are entered in other accounts than the 10 stated above in the accounts. It must then be ensured that these costs are identified and included.

Simulation exercise

Recall that we have five self-reported items that determine the compensation amount; turnover in the application-relevant terms of 2020 and 2019, pre-program (January–February) term turnover of 2020 and 2019, and fixed costs. We quantify the extent of misreporting conditional on fixed costs reports. So, we have four reporting items in the FCCS formula that we are interested in:

$$FCCS = f(Y_{f,b,2020}, Y_{f,b,2019}, Y_{f,1,2020}, Y_{f,1,2019}),$$

where b denotes the term and the precise formula is given by Eqs. 1 and 2.

In Fig. 5, we provide a scatterplot comparing the compensation from the application data and the two counterfactuals (VAT and adjusted VAT). We observe substantial asymmetry in the distribution at lower levels of compensation. We interpret this 'additional mass' on the application side (above the diagonal) of the graph as misreporting.

We are interested in testing whether this asymmetry may be the result of the measurement error rather than misreporting. To shed some light on it, given that we observe the data that already includes the measurement error, we pursue a simulation exercise that further increases the error to see how sensitive the finding of asymmetry is to it.

To inspect this, we introduce a multiplicative measurement error to the different components of the VAT number used to calculate the counterfactual support. We consider a multiplicative measurement error (as specified below), and we repeat the simulation with measurement errors drawn from distributions with different standard deviations. For each of these simulations, we compute a metric that captures the asymmetry of the difference in the actual compensation C^Y and the one defined based on distorted VAT-based calculation $\widehat{C}^V$:

$$\theta = \frac{\sum_{f, C_f^Y - \widehat{C}_f^V > 0} \left(C_f^Y - \widehat{C}_f^V \right)}{\sum_f \left| C_f^Y - \widehat{C}_f^V \right|}.$$

Our interest is in testing whether introducing the measurement error to the VAT data can create a compensation distribution that matches the compensation distribution in the application data. We operationalize it by asking whether a realistic amount of the measurement error may result in symmetry ($\theta = 0.5$).

Before we start the exercise, we need to clarify the setting.

First, we decided to keep the application turnover as fixed and add an error to the VAT reports. Because the two compensation calculations are symmetric, the results are expected to be similar.

Second, we decided to use multiplicative error $Y(1 + e)$. The alternative that we considered, an additive error $Y + e$, is not ideal in the presence of heterogeneity in the size of the firm. We assume that term e is normally distributed and censor negative values of the distorted term.

Next, we consider two cases for correlation in error terms within a firm; (i) independent errors, and (ii) perfectly correlated errors.

Finally, we specify 15 scenarios to introduce the error. For readability, we denote the counterfactual compensation as *Cxxxx*, where *x* will identify the element with the error term.

1. In one of the elements - C1000; C0100; C0010; C0001;
2. In two of the elements - C1100; C0011; C1010; C0101; C1001; C0110;
3. In three of the elements (for completeness) - C1110; C1101; C1011; C0111;
4. In all elements - C1111.

Recall the properties of the FCCS and how compensation depends on the four elements of the turnover. First, a proportional increase in all four elements has no impact on the compensation level, i.e., constant multiplicative on all four elements does not alter the amount. The same holds for "seasonal variation", i.e., constant multiplicative error imposed in the same term of different years yields the same compensation. Therefore, for perfectly correlated errors, $C1111$, $C1100$, and $C0011$ are the same as compensation with 'pure' VAT.

In the following, we list the steps of our simulation to assess the potential impact of the measurement error.

1. Calculate compensation using the VAT data with measurement error for each of the 15 scenarios described above.
2. Calculate the sum of differences in compensations between application data and counterfactual compensation calculations, in absolute values.
3. Calculate the share of the sum of positive difference-θ. This measure captures the asymmetry of the distribution.
4. Plot the relationship between theta and the measurement error based on 200 simulations for each error term scenario (i.e., for each level of measurement error and each scenario, the graphs plot 200 individual data points).

Based on these figures, we observe that even with the very large 20% measurement error, the predicted overuse is still above 60%, and it is, perhaps unsurprisingly, falling rather than increasing with the magnitude of the error, and it is so under all of the considered scenarios. Hence, the presence of measurement error is expected to bias the results toward symmetry (rather than away from it, i.e., it acts against our findings), but we would not expect this bias to be very large.

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Declaration

Conflict of interest None.

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