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The Effect of Covid-19 on the Labor Markets in Scandinavia

An Assessment of the Longer-Term Effects of NPI Implementations on the Scandinavian Labor Markets

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Preface

This master thesis is written as a part of the MSc degree at the Norwegian School of Economics. The thesis constitutes 30 ECTS to each of the authors, who are majoring in Business Analytics and Economics. Through the process of writing this thesis, we have further developed our theoretical knowledge while acquiring relevant experience in writing an academic paper. During this semester we have been encouraged to apply the knowledge we have acquired at NHH to the research field of the thesis. Writing a master thesis is time-consuming and has presented us with several challenges, in which we have supported each other through. Nevertheless, it has been a rewarding and educational process.

We genuinely express our gratitude towards our supervisor, Associate Professor Floris Tobias Zoutman, for his belief in our abilities, constructive views, and availability. Throughout the semester, he has both encouraged and challenged us. His knowledge of and proficiency in our field of study has provided us with valuable insights and advice. This has been a fundamental contribution to the results. Finally, we want to acknowledge the pivotal support of our friends and family throughout the arduous process it is to write a master' thesis.

Abstract

This thesis' aim is to analyze the longer-term effect of implementations of Non-Pharmaceutical Interventions (NPIs) on the Scandinavian labor markets. We do so by estimating how such implementations affect the number of unemployed and furloughed workers. Finally, we analyze how the impact of NPIs differ from early periods in the pandemic to later periods. The findings may serve as useful insights when passing measures that contains the pandemic while minimizing negative economic effects.

Existing literature focuses on one event, thereby studying the short-term effects of NPIs. Additionally, these studies are conducted at the start of the pandemic and analyze a single country. This thesis includes several events and studies the effect of Covid-19 over a longer period. Thus, we are able to investigate how the effect on labor markets change over time and across countries.

We start our analyses by performing an OLS regression to estimate the general effect of NPI implementations in the weeks surrounding such implementations. We then fit a regression model to estimate the individual effect of each individual NPI implementation. The results indicate how such implementations affect unemployment and furlough spells. By including the estimated effect of implementations, we further estimate the effect of different NPI categories. Finally, we estimate the effect of events in the "first wave" of the pandemic versus those in later waves.

The findings suggest that NPI implementations have a significant, increasing effect on furlough spells, but not on unemployment. We find it reasonable to assume that companies choose to furlough workers instead of laying them off. All events but one result in a statistically significant increase in the number of furlough spells per 100 000 inhabitants, as long as furlough programs were available. The effects of different NPI groups vary, and not all NPI groups alone contribute to an increased effect. The events in the "first wave" of the pandemic resulted in larger increases in furlough spells than those occurring in later stages.

***Keywords*:** Covid-19, NPIs, Unemployment, Furlough Spells, Event Study,

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1. Introduction

1.1 Motivation and Purpose

What started as a cluster of pneumonia cases in Wuhan, China in December 2019, has later evolved into a global pandemic with a total of 271 963 258 confirmed cases, including 5 331 019 deaths[1] (World Health Organization [WHO], 2020). A novel coronavirus called SARS-Cov-2[2], causing the disease known as Covid-19, was identified in January 2020 (Folkehelseinstituttet [FHI], 2020). The virus has since spread rapidly throughout all parts of the world, causing not only a public health emergency, but also a global economy crisis.

As a response to the emerging public health threat, many governments imposed stringent non-pharmaceutical interventions (NPIs). The aim was to reduce the spread of the virus and thereby limit the pressure on health care capacities. Such response measures naturally have a direct effect on the economy, through closings of businesses in the hospitality industry, personal services, entertainment, and retail (Juranek et al., 2020). They can also have an indirect impact through reduced investments, disrupted supply chains both locally and globally, and unstable financial markets. Additionally, behavioral responses to the pandemic itself may also affect the economy indirectly. This may happen through loss of production due to increased sick leave or reduced social mobility caused by risk of infection. The indirect effects may decrease consumer demand and income, which may translate into reduced labor demand (Holden et al., 2020). Thus, Covid-19 has had an immense influence on peoples' lives in several areas: economic activity, unemployment and furloughs, climate change, digitalization and use of technology, as well as reduced returns on savings, to mention some. The economic consequences have been dire, leading to the worst recession since the Great Depression (International Monetary Fund, 2020).

Several studies have examined the effectiveness of NPIs in reducing the spread of the virus and maintaining adequate health care capacities in a short-term time frame (Conyon et al., 2020; Glogowsky et al., 2020; Huber & Langen, 2020; Juranek & Zoutman, 2020). However, other

[1] Numbers retrieved at the time of writing (20.12.21) from World Health Organization - https://covid19.who.int

[2] Severe Acute Respiratory Syndrome Coronavirus 2

studies point out concerns of the negative consequences these stringent measures may impose on the economy and labor markets (Andersen et al., 2020; Juranek et al., 2020; Kong & Prinz, 2020). The decision problem of many governments has thus been viewed as a trade-off between saving peoples' lives and saving the economy (Holden et al., 2020).

Being more than 1.5 years into the Covid-19 pandemic, knowledge about NPI effectiveness and economic consequences is becoming increasingly available. The emerging body of literature on the subject is contributing greatly to this. Since the onset of the pandemic, many countries have experienced re-openings of their societies, with NPIs being more targeted or lifted completely. This allows for new studies on the effects of NPIs to be performed for a longer time frame than previously conducted.

Juranek et al. (2020) examines the Labor Market Effects of Covid-19 in four of the Nordic[3] countries. To do so, they apply an event study methodology. The time frame of their study is the first 21 weeks of 2020, and their event is week 11. The aim of our study is to extend this analysis over a longer period of time. This allows us to investigate the longer-term effects on the labor markets. While Juranek et al. (2020) includes a single event in their study, this thesis aims to include several events across countries included in our study. Consequently, our analysis can examine the general effect of NPI implementations across events, the individual effect of such events, and the effect of different NPI groups. By including several events, we can also assess whether NPI implementations in early stages of the pandemic has a greater impact on labor markets than those in later stages. This will provide valuable, quantitative insight on how NPIs affect the Nordic labor markets in a much broader perspective than what has previously been studied.

As mentioned, Juranek et al. (2020) studies the labor market effects of Covid-19 in four of the Nordic countries. However, this thesis excludes Finland from the study. The relevant data on unemployment and furlough spells for this country is unavailable to us. Therefore, the analysis will estimate the longer-term effects of NPIs on the labor markets of Norway, Sweden, and Denmark – the three Scandinavian countries. By doing so, we are able to evaluate the effect of NPIs on Scandinavian labor markets, and whether different NPI categories have different impacts.

1.2 Research Question and Hypotheses

For many reasons, the three Scandinavian countries are ideal when studying how NPIs have affected the labor markets of the respective countries. Their economic environments are similar in terms of GDP per capita and trade openness. All three countries are small, open, and developed economies. In addition, their institutional background, labor markets, and health care sectors and capacities are also comparable. Another significant similarity is their level of exposure at the beginning of the pandemic. The 100th confirmed case occurred in all three countries within 5 days, due to geographical proximity and economic relations[3] (Juranek et al., 2020). Based on these similarities and the need for further insight on the effects of Covid-19 on labor markets, the research question of the thesis is as follows:

How does the implementation of NPIs affect the labor markets of the Scandinavian countries in the longer run?

The results of our analysis will contribute with valuable insights that can serve as decision support for authorities when passing response measures. These measures should contain the pandemic while minimizing the negative effects on labor markets. In order to pass such measures, knowledge about the effect of NPIs on labor markets is essential. To examine the research question and analyze our data, we have written up three main hypotheses:

Hypothesis 1: *Events of NPI implementation or an increase in NPI strength will result in an increase in the number of unemployed or furloughed workers.*

Hypothesis 2: *Not all categories of implemented NPIs will result in an increase in the number of unemployed or furloughed workers.*

Hypothesis 3: *NPI implementations or increases in NPI strength in the "first wave" of the pandemic will have a greater effect on unemployment or furlough spells than those in later waves.*

[3] 100th case: Norway 4th, Sweden 6th and Denmark 9th of March

In order to investigate these hypotheses, this thesis applies an event study methodology and estimate the effects of interest through OLS regression. By using pre- versus post-treatment comparison, the study evaluates the general and individual effect of events on unemployment and furlough spells. Then, we will estimate the effects of different NPI groups present in the events and examine how effects differ between early and later stages of the pandemic.

1.3 Structure

The thesis is structured as followed:

Chapter 2 presents the background for the thesis. This includes a brief introduction to the Covid-19 pandemic, NPIs and NPI Strategies of the Scandinavian countries, as well as the unemployment benefits and short-time work programs of these countries. Chapter 3 presents a review of existing, relevant literature. Chapter 4 describes our data collection process for labor market and response measures data, as well as the structure of our final data samples. Chapter 5 thoroughly explains the empirical methods applied in our analysis. Here we elaborate on the event study methodology, identification of events, and pre- versus post-treatment comparison. Additionally, we describe all our regression equations and analyses. Chapter 6 presents the empirical results together with a discussion of the findings. Robustness tests used to evaluate how different adjustments affects our findings is presented in chapter 7. The limitations of our study can be found in chapter 8. Finally, chapter 9 presents a brief summary of the results and concludes whether the findings support our hypotheses and answer our research question. Chapter 9 is followed by a reference list and the supporting appendix.

2. Background

For the purpose of placing our study into a broader context, this chapter provides some background information and explanations of fundamental concepts. By doing so, one will be better equipped when interpreting the results from our analyses. A general understanding of the Covid-19 pandemic and its background is relevant to better understand the motivation behind this thesis. Awareness of how NPIs may affect economies also enlightens this motivation, as they indirectly or directly influence labor markets. Knowledge about the unemployment benefits and short-time work retention programs of the countries is essential to interpret the results of the analyses. This is particularly relevant when analyzing the effect on unemployment versus furlough spells. Enhanced understanding of relevant topics and the context of our study will help us in evaluating the hypotheses and answering research question.

2.1 The Covid-19 Pandemic

The novel coronavirus SARS-Cov-2, identified in January 2020, is the cause of the current global Covid-19 pandemic. The virus causes a respiratory disease with symptoms ranging from mild to serious, and in rare cases deaths, while some infections are asymptomatic. Most people infected will experience a mild course of infection and recover without the need of treatment. Common symptoms include coughing, fever, sore throat, headache and loss of smell and taste. Some will however experience a more serious course of infection and require medical attention. This is more common among elderly people and people with certain underlying medical conditions. Severe symptoms include shortness of breath, chest pain and confusion (FHI, 2020).

Transmission of the virus mainly happens through close contact with infected people, by exposure to small liquid particles from infected people's airways that contains the virus (FHI, 2021). Such particles include both larger respiratory droplets and smaller aerosols. Infected people excrete these liquid particles through their mouth and nose, and pass them on by activities like coughing, sneezing, speaking, singing, working out or breathing (WHO, n.d.). Droplet size and distance to the source of infection are therefore of importance for disease transmission (FHI, 2021).

2.2 Waves of Infection in Scandinavia

While there is no formal definition of a wave of infection, the term is commonly used to describe different stages of a pandemic. For wave to start, there need to be a sustained increase in infections. Similarly, for a wave to end, a sustained decrease in infections has to follow. This implies that the spread of the virus has been brough under control when a wave ends (Gallagher, 2020). Thus, the beginning and end of a wave is often a question of interpretation. A pattern of peaks and valleys can be identified by looking at visualizations of confirmed cases, making it possible to detect waves of infections. Such visualizations for the Scandinavian countries are presented in the appendix, figure A1. These visualizations are based on data from the Oxford Covid-19 Government Response Tracker project (Hale et al., 2021). Together with referenced sources, they form the basis for what we in this thesis regard as the waves of infection in Scandinavia.

For Norway, the first wave is commonly considered as the spring of 2020, the second wave as the winter of 2020/2021, and the third wave as the spring of 2021. There has also been a rise and fall in infections during the late summer/early fall of 2021, which has been referred to as the fourth wave (Johansen, 2021; Tjernshaugen, 2021). This was followed by a record high surge in infections in the fall of 2021, which is still ongoing by the time of writing (see figure A1). When studying the visualizations of confirmed cases, we observe the same trend for both Denmark and Sweden, although the number of cases naturally varies between the countries.

2.3 Non-Pharmaceutical Interventions

As a response to the emerging pandemic, many governments decided to impose non-pharmaceutical interventions (hereafter NPIs). The aim is to reduce the rate of transmission, prevent infection and thereby relieve the pressure on health care services. NPIs are strategies and policies carried out by people and communities, aimed to slow the spread of infection during an outbreak, like the Covid-19 pandemic (Centers for Disease Control and Prevention [CDC], 2020).

Personal NPIs include the prevention measures such as keeping a 1-meter distance to others, wearing a face mask, washing hands or using alcohol-based rub often, practicing respiratory etiquette like coughing and sneezing into an elbow, and staying home when feeling unwell (WHO, n.d.). Additionally, it is recommended to reduce the number of close contacts when possible, especially in times of high infection rates (FHI, 2021; Regjeringen, 2021).

Community NPIs are measures put in place by communities and organizations to decelerate an infectious outbreak. The two most common are social distancing and closures. Social distancing measures aim to facilitate increased distance between people in situations where they would otherwise have close contact. This can be at schools, workplaces, public transportation, childcare, stores, etcetera. Closures involve temporarily closings of places where people normally gather, such as those mentioned above, in addition to places of worship, sport arenas, festivals, conferences, libraries, etcetera (CDC, 2019). According to European Centre for Disease Prevention and Control, "NPIS are the most effective public health interventions against COVID-19" when a safe and effective vaccine is yet to be available (European Centre for Disease Prevention and Control, 2020).

NPIs have both direct and indirect consequences on the economy, as mentioned in chapter 1.1. This is particularly true for visitation-based businesses, which may have to operate under reduced capacity or close down to be able to follow the implemented measures. An example of this are gyms and fitness clubs, which have been severely affected by strict NPIs. For example, the Nordic gym chain "SATS" had to shut down during the first wave. Thus, they had to send a furlough notice to 4000 employees in Norway in March 2020 (Christensen, 2020). The aircraft industry has also been heavily affected by restrictions aimed to prevent the spread of the virus. As people are advised to stay at home, avoid unnecessary travels and work digitally, consumer demand in this industry naturally came to an almost complete halt. In April 2020, the Scandinavian airline company SAS had almost 11.000 furloughed workers in Scandinavia (Randen & Giæver, 2020). Such events caused by NPIs, either directly or indirectly, are very likely to have an impact on labor market statistics. This is exactly what we intend to investigate further in our study.

2.4 NPI Strategies of the Scandinavian Countries

Despite being similarly exposed to the pandemic at its onset, Sweden differed considerably from its two Scandinavian neighbors when first implementing NPI strategies to contain the spread of Covid-19. Denmark and Norway implemented a strict NPI strategy in week 11 and 12 of 2020, respectively. The aim was to reduce social mobility and interaction through stringent restrictions. Sweden, however, decided on a more trust based NPI strategy, with the restrictions imposed being much lighter (Juranek et al., 2020). Throughout the period of our study, we observe that Norway and Denmark more often opt for required measures, while Sweden more often opt for recommendations or no measures.

As a measure of the strictness of government policies, we use the Oxford Government Covid-19 Government Response Tracker (OxCGRT) project's calculated Stringency Index. This project is further explained in chapter 4.3. The interval of the Stringency Index ranges from 0 to 100, with 0 being the least strict response and 100 being the strictest response (Hale et al., 2021). By the end of week 11 of 2020, Denmark, Norway, and Sweden had a Stringency Index value of 65.74, 51.85 and 30.56, respectively. This value kept rising for all three countries the following month, before it dropped substantially for Norway during the summer of 2020. Denmark's Stringency Index value gradually reduced during the summer. Meanwhile, Sweden's value remained the highest of the three countries throughout the summer months. During 2021, the countries' Stringency Index values have been more similar. This implies a reduced difference in the strictness of the NPI strategies between the countries as the pandemic progressed. The three countries' Stringency Index values are illustrated in Figure 2.1 below, based on data from OxCGRT (Hale et al., 2021).

Figure 2.1

Stringency Index for the Scandinavian Countries

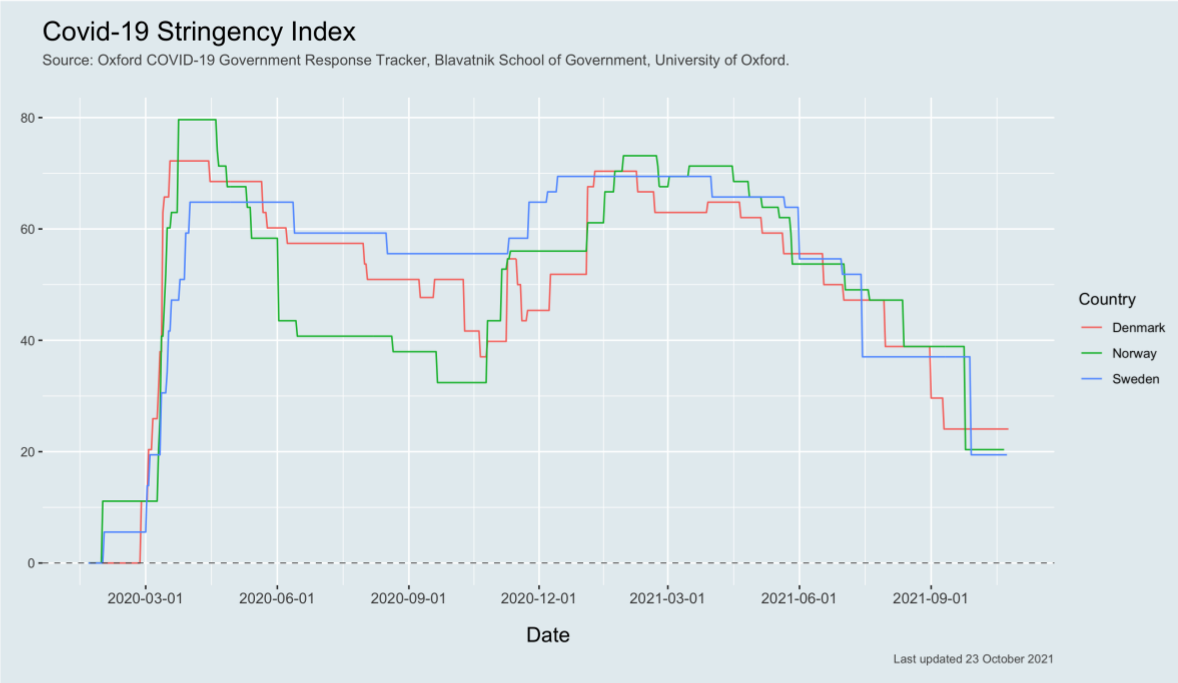

Figure 2.1

2.5 Short-Time Work Retention Programs

Short-time work programs and job retention schemes have been important during the pandemic. Their purpose is to retain employment and avoid large mass-layoffs of employees, as well as supporting the incomes of unemployed and furloughed workers (Scarpetta et al., 2020).

2.5.1 Denmark

On March 15th, 2020, Denmark announced the implementation of a novel short-time job retention scheme, available from March 9th, 2020, to July 8th, 2020. Companies that would otherwise need to lay off 30% of its workforce or more than 50 employees, caused by Covid-19 related economic challenges, are eligible to partake in the program. Partaking companies can retain employees by putting them on furlough, as an alternative to dismissals. 75% of employee salaries and 90% of salaries of workers paid by the hour (both up to a cap of 30.000 DKK) will be refunded by the government. The remaining 25% or 10% of salaries will be covered by the companies themselves. Furloughed employees must use five days of annual leave and reduce their working hours by 100%. However, they will still receive their full salary and keep their jobs (Finansministeriet, 2020).

2.5.2 Sweden

Like Denmark, Sweden also introduced a new short-time work allowance program, available from March 16th, 2020. In February 2021, the program was replaced with an updated but similar one, available (in retrospect) from December 1st, 2020 to June 30th, 2021. To be eligible to receive state aid, partaking companies must have experienced temporary and serious financial challenges that are beyond their control. In the original program employers were able to reduce the working hours of employees by 20, 40 or 60%. From May to July 2020 the upper limit of reduced working hours was increased to 80%. This limit was also included in the updated program introduced in 2021, from January to June 2021. The state will fund up to 60% of the employees' salaries, while the salaries will be reduced by up to 12%, depending on the percentage of working hours reduced. This can reduce the partaking companies' personnel costs by up to 72%. See Table 2.1 for all reduction levels and the respective funding. For all levels, the cap for financial aid is 44.000 SEK. The program allows companies financially affected by Covid-19 to retain their employees by reducing work hours instead of having to lay them off. At the same time, the central government bears most of the costs (Tillvaxtverket, 2021).

Table 2.1

Short-time Work Allowance Program in Sweden

Working time reduction	Employee salary reduction	Employer personnel costs reduction	Central government funding
20%	4%	19%	15%
40%	6%	36%	30%
60%	7.5%	53%	45%
80% [a]	12%	72%	60%

Table 2.1

[a] *Only available during May-July 2020 and January-June 2021*

2.5.3 Norway

Norway has an already existing short-time and unemployment benefit program. Companies experiencing unforeseen financial or production-related difficulties beyond their control, like Covid-19, can put their employees on full or part-time furlough. Furloughed employees are entitled to the same benefits as unemployed workers. However, temporary changes were introduced in accordance with the progression of the pandemic. These changes regarded relaxations of eligibility and enhanced benefits. Originally, employers were obligated to uphold wage payment the first 15 days of furlough. This was followed by state funded compensation for lost income of the employee on furlough. The employers' obligation was reduced to 2 days with effect from March 20th, 2020, and then increased to 10 days as of September 1st, 2020. Originally, the state funded compensation would cover 62.4% of the lost income for employees with reduced working hours by minimum 50%, with a cap at about NOK 600 000 (annual wage). For furloughs in effect between March 20th, 2020, and September 1st, 2020, the compensation would cover 100% of lost income on day 3 to 20 of the furlough spell. From day 21 and onward, the state compensated 80% of income below approximately NOK 300 000 and 62.4% of income between NOK 300 000-600 000. Income above this level was not compensated by the government. In addition, the required reduction in working hours was temporarily reduced to a minimum of 40% (Deloitte, 2021).

2.5.4 Other Considerations

Denmark offers unemployment funds that employees can be member of and receive aid from if eligible, when becoming unemployed or furloughed[4]. For the different programs, temporary changes have been made for level of compensations, requirements of eligibility or reduction in working hours, level of income allowed to combine with benefits, etcetera. To cover all such changes in detail is regarded as outside the scope of the thesis. However, the summaries provided above for the three countries' respective programs give a sufficient understanding of the most relevant parts of the different policy programs for furloughs and unemployment.

4 https://star.dk/tilsyn-kontrol-og-klager-over-a-kassernes-afgoerelser/tilsyn-og-kontrol-med-a-kasser/oversigt-over-a-kasserne/, https://www.tyj.fi/en/join-a-fund/funds-contact-information/?profession=

3. Literature Review

This chapter presents a review of relevant literature from previously conducted research in our field of study. The Covid-19 pandemic has made its impact on the world for almost two years now. Consequently, many researchers have aimed to study its effect on labor markets as well. As long as the pandemic is still ongoing, this body of literature will keep growing and be continuously updated with new findings. Hence, this review will only be able to present the current status of the literature. Much of the previously conducted studies focus on short-term effects and analyze countries outside of Scandinavia. For these studies, the time period that was possible to study was limited, as the pandemic was a relatively new phenomenon at the time of writing. Nevertheless, the existing literature provides several findings that are relevant to our thesis. This section will therefore briefly review these findings in the following sections. The main focus of existing literature is on the short-time effects of a single event, and for countries outside of Scandinavia. To the best of our knowledge, there are no studies focusing on the effects of NPIs on Scandinavian labor markets in a longer-term perspective. This thesis will contribute to the existing body of literature by filling this gap in the current research.

Juranek et al. (2020) studies the short time labor market effects of NPIs in the Nordic countries. They apply an event study methodology to examine the effect of one single event on unemployment and furlough spells. This event is week 11 in 2020, which is the week were Norway and Denmark introduced strict NPIs and lockdown measures. The total time period of their study is the first 21 weeks of 2020, compared to the same period of 2019. They find that the labor markets of all four Nordic countries were severely hit by the pandemic. However, they find that it had the largest impact on Norway and Denmark, while Sweden experience slightly fewer negative effects than its neighbors. This study is a large contribution to our research, as we intend to extend their analyses by examining a longer time period with more events included. This way we can investigate both the general effect of events, as well as the individual effect of each event, on labor markets. Additionally, it allows us to compare the effects of events early in the pandemic to those later on, as NPIs strategies change over time. Finally, this thesis also aims to investigate how different NPIs have varying effects.

In their study, Alstadsæter et al. (2020) focus on the effects of Covid-19 on the Norwegian labor market in the first weeks of the pandemic. Their study examines what parts of the population and industry was most affected by the crisis in its beginning. They do so by looking at both temporary and permanent layoffs. They also look at the risk of exposure to infection for workers with socially critical functions. One of their findings is that layoffs first occurred in industries directly affected by restrictions, but that other industries rapidly became indirectly affected. Thus, there was a large spillover effect of measures implemented, onto businesses not directly targeted. Other findings are that almost 90% of layoffs were temporary at this time, and that the layoffs that were permanent led to a monthly unprecedented change in unemployment. This research is relevant to our study as it provides us with an essential understanding of the early effects of Covid-19 on the Norwegian labor market. Unlike Alstadsæter et al. (2020), we have the possibility to study the effects of Covid-19 on labor markets over a longer period. Additionally, this thesis includes the other Scandinavian countries, as they were on similar trajectories at the onset of the pandemic. Thus, it is possible to evaluate how the effects may change over time and between otherwise similar countries.

The IZA Institute of Labor Economics is an economic research institute conducting research in the field of labor economics (Eichhorst et al., 2021). They provide a research report series, IZA COVID-19 Crisis Response Monitoring, in which Hensvik and Skans (2020) studies the labor market impacts of Covid-19 in Sweden. Their focus is on the early stage of the pandemic. In the report, they discuss how Sweden has had less restrictive measures relative to other countries. The measures have mainly been recommendations, relying on voluntarily compliance. However, their study finds that even milder recommendations have had a large impact on economic outcomes and the labor market. Their study uses data from the Public Employment Service (PES). One of their results is that the number of new vacancies at the PES were reduced by one third. They also find that the number of layoff notices increased from 24.000 to 84.000, implying that the crisis led to 1% of the labor force being notified of a layoff. Additionally, they elaborate on the short-time work scheme introduced as a response to the crisis. Like Alstadsæter et al. (2020), Hensvik and Skans (2020) focus on the impact of Covid-19 on only one of the Scandinavian countries. However, their study does provide us with a greater understanding of the effects on the Swedish labor market as well as their work retention programs. We intend to include Sweden in our study, and thus this understanding is relevant for our analyses and interpretations.

In the Journal of Public Economics, Kong and Prinz (2020) analyze how NPIs affected unemployment in the U.S. during the Covid-19 pandemic. They apply an event-study framework and Google search data as a proxy for unemployment insurance claims. Their aim is to investigate and disentangle the effects of several NPIs on unemployment. Their main finding is that the NPIs considered in the study was not causing the observed increase in unemployment. State-level limitations on bars and restaurants, closures of non-essential businesses, ban on large gatherings, closures of schools and emergency declarations caused less than 13% of the increase in UI claims in the first weeks of the pandemic. While limitations on bars and restaurants and closures of non-essential businesses had some effect on UI claims, the other NPI groups had no significant effects. Their study presents some interesting insights on the effects of NPIs on unemployment. In a similar fashion, Bauer and Weber (2021) investigate how Germany's Covid-19 containment measures affects the labor market in the short-term. Through a difference-in-difference estimation, they examine the treatment effect on unemployment. The focus of their study is on one single event, based on the time of introduction of measures. Their main finding is that 60% of increased inflows to unemployment in April 2020 was caused by lockdown measures. In short, the measures caused a short-term increase in unemployment of 117.000 people. Our thesis extends the ideas of Kong and Prinz (2020) and Bauer and Weber (2021) to the Scandinavian context and several events.

This thesis will contribute to the already existing literature on the effect of Covid-19 and NPIs on labor markets. Existing studies, presented above, provide us with essential understanding and relevant findings. However, these studies focus on the effects of one single event at the start of the pandemic, and thereby their findings highlight the short-term effects. Except from (Juranek et al., 2020), the studies also focus on one single country. This thesis intends to estimate the effect of several events across the Scandinavian countries. By doing so, the thesis will fill this gap in the existing literature. Our findings can highlight the longer-term effects of NPIs and evaluate whether these effects change over time.

4. Data Collection and Sample Description

In this thesis, we study a data sample that consists of data on unemployment and furlough spells from Norway, Denmark, and Sweden. Further, it includes data on the NPIs implemented and their level of strength for these countries in the same period. This chapter elaborates on our data collection process, the challenges that have arisen, and how we have solved them.

4.1 Panel Data

The data sample used in our main analyses is structured as a panel data set. Panel data consist of time series for each cross-sectional unit in the data set. Such units can for instance be geographical (Wooldridge, 2018a). This is the case for our data set, as we collect repeated observations across both regions and countries, over a given period of time. There are several advantages of using a panel data set. They contain more information and variability, and allow for increased efficiency, compared to pure time-series or cross-sectional data (Eric, 2021). With multiple observations of the same units, one has the possibility to control for certain unobserved characteristics (Wooldridge, 2018a). In our study, we intend to analyze the change in unemployment and furlough spells over time. Panel data serves this purpose better than pure time-series or cross-sectional data.

4.2 Data on Furlough Spells and Unemployment

4.2.1 Norway

The collection of the Norwegian data on unemployment and furlough spells has been a complex and time-consuming task. We have gathered data from *The Norwegian Labor and Welfare Administration* (hereafter NAV), through weekly reports published on their website. The reports containing the weekly data are published as separate excel files, one for each week. Monthly reports are also available, with separate excel files for each month. The data on unemployment and on furlough spells are also published in different excel files. Thus, we had to download a large number of excel files and move all the data we needed into our own panel structured data set. Additionally, there are many weeks where the reports are simply not published, due to capacity issues (NAV, 2021d). See appendix A2 for a complete overview of these weeks. We have seen the unpublished data being used in other studies and news articles, so we do know that it is registered, just not published. NAV does not answer requests regarding data or assist master students (NAV, 2021c). Thus, this data unfortunately remains unavailable to us. The final data sample for Norway contains data on furlough spells and unemployment from week 13 in 2020 to week 43 in 2021, with some weeks being estimations.

For the weeks where data is not published, we calculate different estimations. The analyses rely on the assumptions that these estimates present an accurate trend in unemployment and furlough spells. If the weeks with missing data is the last week of a month, we calculate estimations based on the end-of-month statistics on unemployment and furlough spells. This is gathered from the monthly reports (NAV, 2021a). For the remaining weeks we calculate an average number of unemployment and furlough spells. This is based on weeks with reliable data. In many cases, there are several subsequent weeks where data is missing. We then calculate the average for the first missing week based on the values of the last week with reliable data and the next available week with reliable data. For the next missing week, we use the calculated average of the first missing week and the value of the next week with reliable data to estimate an average. In chapter 7.1 we present an alternative estimation method, used as a robustness test of our analyses.

NAV registers unemployment and furlough spells in several ways. One can either be registered as (1) full-time unemployed, (2) partially unemployed, or (3) jobseeker participating in labor market measures. The same goes for furlough spells. In Norway, gross unemployment is defined as full-time unemployment plus jobseekers participating in labor market measures (Statistisk Sentralbyrå, 2015). This thesis will use the same definition for furlough spells. Thus, gross furloughs equal full-time furloughs plus furloughed jobseekers participating in labor market measures.

Additionally, the weekly data in the published reports cannot be used as it is. Several calculations must be performed before including it into our own data set. People who are full-time furloughed are included in the number of full-time unemployed (NAV, 2021b). Likewise, people who are partially furloughed are included in the number of partially unemployed. Finally, the number of furloughed people participating in labor market measures are included in the number of jobseekers participating in these measures. Thus, to obtain the number of unemployed not including furloughs, one needs to subtract the number of full-time furloughed from the number of full-time unemployed. The same logic applies to partially unemployed and jobseekers participating in labor market measures.

4.2.2 Denmark

Collecting data on unemployment and furlough spells from Denmark has been a relatively straightforward process. We downloaded experimental data on full-time unemployment on a national level from Statistics Denmark (Statistics Denmark, 2021). The source of this data is The Danish Agency of Labor Market and Recruitment (Styrelsen for Arbejdsmarked og Rekruttering, n.d.). The data on unemployment is structured on a daily level, while our data set is structured at a weekly level. Thus, we use the number of unemployed at the last day of a week as the weekly number of unemployed in our data sample. We received data on full-time furlough spells per e-mail from *Erhvervsstyrelsen* on a weekly, regional level (personal communication, 15. October 2021). The final data sample for Denmark contains data on unemployment from week 1 in 2020 to week 42 in 2021, and on furlough spells from week 11 in 2020 to week 36 in 2021.

4.2.3 Sweden

Collecting data on unemployment and furlough spells in Sweden has also been relatively easy. We received data on full-time unemployment on a weekly, national level per e-mail from *Arbetsförmedlingen* (personal communication, 4. October 2021). *Tillväxtverket* provided us with data on furlough spells on a weekly, regional level (personal communication, 6. October 2021). As discussed in chapter 2.5.2, employees can only be partially furloughed in Sweden. Hence, the data consist of part-time furlough spells. For the Swedish data, there was no need to make any adjustments or calculations before including it into our sample. The final data sample for Sweden contains data on unemployment from week 1 in 2020 to week 44 in 2021, and on furlough spells from week 12 in 2020 to week 26 in 2021.

4.2.4 Other Considerations

Initially, we indented to analyze both unemployment and furlough spells on a regional level. However, data on unemployment for Sweden and Denmark is available to us on a national level only. Hence, we decide to analyze unemployment on a national level for all countries. The data on furlough spells, however, is available to us on a regional level for all three countries. Thus, we proceed with these analyses on a regional level, as planned. As mentioned in chapter 4.2.1, there are several ways to register unemployment and furlough spells in Norway. We have data on gross unemployment for Sweden and Denmark. Thus, we decide to use gross unemployment for the Norwegian data as well. Thereby, the definition of unemployment for our analysis is full-time unemployed plus jobseekers participating in labor market measures. In Denmark, one can be only on full-time furlough, while in Sweden one can only be partially furloughed. In Norway, both are possible, but 72% of furlough spells are actually full-time (Juranek et al., 2020). Based on this, and because all Danish furloughs are full-time, we decide to use full-time furlough spells for Norway as well.

4.3 Response Measures

We download data on different NPI measures for the three countries from *Our World in Data* (Ritchie et al., 2020). The source of this data is the OxCGRT project, which systematically collects live data on government policy measures during the Covid-19 pandemic (Hale et al., 2021). They gather this data in a panel database, containing pandemic policies of more than 180 countries. The database consists of 23 indicators that are categorized into containment and closure policies, economic policies, health system policies, and vaccine policies. This data has been collected on a daily level since the 1st of January 2020 and is updated continuously. Interactive visualizations and CSV data sets based on the OxCGRT data are available for download through *Our World in Data*[5].

[5] https://ourworldindata.org/coronavirus

We download daily data on the level of strength of 12 indicators during the period of our study, for each of the three countries included. We also download two measures of policy response strictness: the Government Stringency Index and the Health and Containment Index. An overview of the 12 indicators and the two indexes is presented in appendix A3. As our data set is structured on a weekly level, we had to make some adjustments to the data on NPIs before adding it to our sample. To aggregate the data on a weekly level, we merged the days of each week. If there was a change in an NPIs level of strength during a week, we kept the level of strength present in most of the week. In other words, when merging the daily data, we use the average level of strength of NPIs during a week.

We have also considered to use the European Center for Disease Prevention and Control's (hereafter ECDC) data on country policy measures. However, the ECDC are only publishing data on NPIs on a national level, regardless of regional measures. Hence, local measures that are stricter than national measures are not included in their data (ECDC, 2021). In the OxCGRT data however, the strength of NPIs registered for a country is based on the strictest policies on regional levels. National measures are often less strict than regional measures. For example, Oslo has for longer periods of the pandemic had stricter NPIs implemented than Norway has had on a national level. We believe that it is important to include NPIs at the strictest level in order to capture the effects on labor markets. Therefore, we choose to proceed with the data from OxCGRT, and not from ECDC.

5. Empirical Methods

This thesis aims to investigate the effect of NPIs on unemployment and furlough spells in the three Scandinavian countries. To do so, we apply an event study methodology to study the general and individual effects of NPIs, as well as the effects of different NPIs. We also study the effect of NPIs implemented in the first wave of the pandemic, compared to those in later waves. This chapter describes the main features of event study methodology. It also elaborates on our use of pre- versus post-treatment comparison together with linear regression. These methods are used to estimate the effects of interest, as mentioned above.

5.1 Event Study Methodology

An event study is an econometric analysis of the effect of an event, such as a change in government regulation or economic policy, on an outcome variable. The method is more common in finance, with focus on stock returns after an announcement of a significant market event (Wooldridge, 2018b). Nevertheless, the method is becoming more common in other research areas as well. For instance, Juranek and Zoutman (2020) use an event study to estimate The Effect of Social Distancing Measures on Intensive Care Occupancy. Lamdin (1999) uses the methodology to investigate the Effect of the Cigarette Advertising Ban, introduced in America in 1971. An event study consists of an (1) estimation window, (2) event date and (3) event window. These three components will be further discussed in subchapters below.

This thesis applies an event study methodology to investigate the effect of NPI implementations on unemployment and furlough spells in the Scandinavian countries. We examine whether such implementations cause a change in unemployment and furlough spells, relative to the period before implementations. Coefficients related to the period before implementations are estimated in the estimation window of our study. They serve as a counterfactual for the number of unemployment and furlough spells, in the absence of NPI implementation. Coefficients related to the period after implementations are estimated in the event window of our study. They serve as the variable of interest when estimating the effects of different NPIs.

5.1.1 Event Study Timeline

In order to perform our event study on NPI implementations, we first define the events of interest. We then establish the periods before and after the events that will serve as estimation and event windows, respectively. The time unit of our study is calendar weeks. This implies that an event date will be a calendar week and not a regular date. We therefore define an event as the calendar week in which NPIs are expected to have an effect on unemployment or furlough spells. As implementations of NPIs are often announced in advance, an event could be the week of such announcements. This reasoning is often common in event studies within finance. In such studies the date of a shock creating announcement or happening serves as the event date. However, we find it reasonable to assume that workers are not laid off or put on furlough until the week NPIs become active. This is because unemployment and furlough spells often are a direct or indirect consequence of active NPIs, as discussed in subchapter 1.1 and 2.3.

In the body of literature on Covid-19, one can find several studies on the epidemiological effect of NPIs. This can for instance be the effect on Covid-19 related hospitalizations or deaths, or the spread of the virus (Grjotheim & Kjeldstad, 2021; Juranek & Zoutman, 2020). In such studies, implementation of NPIs is commonly expected to influence the variables of interest 2-3 weeks after implementation. This is due to the nature of the virus and the incubation period that follows when infected. However, we do not see the disease progression of the infected as relevant to the variables of interest in our study. Therefore, we do not find it necessary account for any similar time lag when defining the event date.

Based on the reasoning above, we expect NPI implementations to have an immediate effect on unemployment and furlough spells. Therefore, we set the event date (in our study; week) to be the calendar week of NPI implementation. For the estimation and event window, we include the four weeks before and after the event date, respectively. Figure 5.1 illustrates the event study timeline as described. In the following subsections we elaborate on the identification of different events, as well as the selection of estimation and event windows.

Figure 5.1

Event Study Timeline

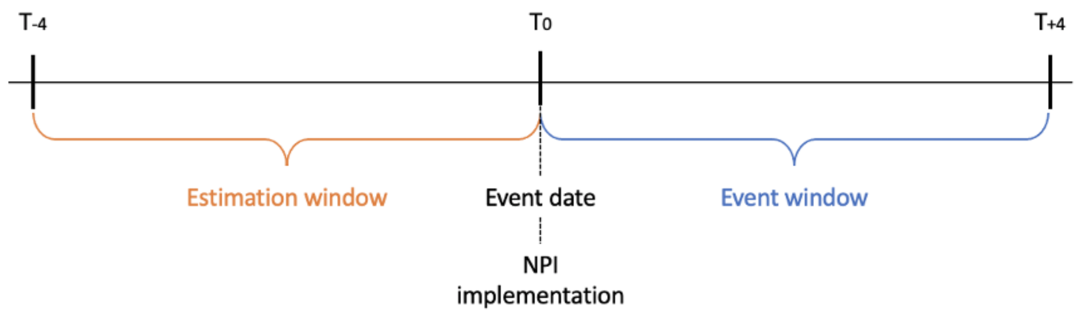

Figure 5.1

5.1.2 Identifying the Events

Our NPI data sample consists of 12 different NPI categories, as well as the "Stringency index" and a "Health and Containment Index". Observations are registered on a weekly level for each region in Norway, Denmark and Sweden, and the data is structured as a panel data set. Thus, our complete sample contains 289 rows. Each row represents a calendar week with corresponding levels of each NPI category and the two indexes. The NPIs are reported on a national level. Therefore, the same levels of NPIs are observed in the same calendar week for each region. The NPI categories can take on values based on their level of strictness, as defined in appendix A3. The minimum value of any NPI level is 0, meaning no measure was in place. A value of 1 generally means that the NPI level of strength was equal to a recommendation. The maximum value of a level varies between NPIs, but any number higher than 1 most often implies requirements on different levels.

To be able to identify the events (weeks) of interest, we define several criteria for event identification. The first criterion is that an NPI has to be implemented or increase in strength in a given calendar week. We choose this identification criterion to identify all weeks were any NPI became active or more stringent. The second criterion is that the implementations or increases in strength have to happen in weeks where the "Stringency index" or the "Health and containment index" is increasing. These indexes are a measure of total NPI stringency. Therefore, we choose this criterion to make sure that weeks where some NPIs are implemented as a relaxation to others are not identified as events. An example of this would be to increase the stringency of testing and tracing NPIs, in order to relax other NPIs such as restrictions on gatherings and public events, closures, etcetera. The third criterion is that more than one NPI category has to be implemented or increase in strength in the given calendar week. Implementations of NPIs or changes in level of strictness often varies over the period of our data set. We do not expect weeks where only one NPI category is implemented or increased in strength to have a large enough impact to be identified as an event. Furthermore, “face covering policy” is often the NPI that changes in the weeks with only one implementation or increase in strength. We do not expect this NPI alone to influence unemployment or furlough spells greatly. These criteria and the steps taken to identify the events in our data sample are illustrated in Table 5.1. Finally, if several events are identified in subsequent weeks, we choose the median week as event date.

Table 5.1

Criteria and Steps to Identify Events

Criteria no.	Action taken	No. of events
1	Identify events where NPIs were implemented/increased	49
2	Identify events where “Stringency” or “Health and containment” indexes are increasing	43
3	Identify events where 2 or more NPIs implemented/increased	17
4	Merge events in subsequent weeks to the median week	10

Table 5.1

5.1.3 The Estimation Window

The next step in an event study is to choose the time interval for the estimation window. Its purpose is to estimate how the observations in our sample are expected to develop. Thus, the estimation window needs to be prior to the event window in order to not be influenced by the event itself. How to set the length of the time interval is not given, and it is really a trade-off between precision and indication of current situation. A longer interval increases precision in estimation of expected progress of the observations in the sample. On the other hand, a shorter interval provides a stronger indicator of the current state (Atkas et al., 2007).

As mentioned in subchapter 5.1.1, the estimation window for our study equals the 4 weeks prior to the event date. The coefficients estimated for this interval serves as counterfactuals. They help us estimate the expected progression of unemployment and furlough spells, had the NPIs not been implemented or increased in strength. We choose the 4 weeks prior to NPI implementation as estimation window to capture the situation of the labor market in the weeks leading up to implementation. Any larger time interval could be confounded by seasonal trends, as unemployment and furlough often vary seasonally. In addition to this, our NPI data sample limits the possibility of longer intervals. Several of the identified events are close to each other in time. Thus, in some cases, a larger interval makes the estimation window overlap with prior events. Also, we do not have data on furlough spells in the weeks prior to the first event in Norway and Denmark. This was not registered before the time of the first event. With an already small sample of events, we do not wish to reduce it even further by having a larger estimation window that excludes these two events.

5.1.4 The Event Window

The next time interval to establish is the one for the event window. Its purpose is to unveil the effects of the events in the study. To choose the length of the event window is, like choosing the length of the estimation window, about balance and trade-off. It can be challenging to estimate the time interval after an event where its effect is most present. While a shorter interval can leave out periods where the effect is still present, a longer interval can include periods where the effect is no longer present. With a shorter interval one could also risk to not account for unrelated effects such as random changes in unemployment and furlough spells. On the other hand, a longer interval could include changes not caused by the event, such as seasonal fluctuations in unemployment and furlough spells.

The event window is set to the 4 weeks after the event, as described subchapter 5.1.1. We assume that the effects of the events will be more or less immediate. Therefore, we choose the weeks immediately following the event. As for the estimation window, the data set also limits our possibilities of choosing longer intervals for the event window. This is due to events being close to each other in time, causing longer event windows to overlap following events or estimation windows.

5.1.5 Overlapping Windows

We have briefly mentioned that some events are close to each other in time. This causes the event window of a previous event to overlap with the estimation window of the next event. This is true for week 46 and 50 in 2020 and week 1 in 2021 in Denmark, and for week 6 and 12 in 2020 in Sweden. For these events we have decided to prioritize the event window, as it is most important to capture the effect of the events in the following weeks. We also assume that the estimates for the weeks prior to the event will show that nothing significant happens in this period. If this is the case, it is more important to prioritize the length of the event window.

5.2 Estimating the Effect of Events and Different NPIs

For the first part of our analyses, we will use a pre- versus post-treatment comparison. The purpose of this comparison is to estimate the effect of the events, i.e., implementation or increased strength of NPIs. The following subchapters briefly discuss the methodology used for the estimation of effects. It also elaborates on the use of these results in further analyses, to estimate the effects of different NPIs. Additionally, it presents our hypotheses and the regression equations used to estimate the effects of events and different NPIs.

5.2.1 Pre- Versus Post-Treatment Comparison

Pre- versus post-treatment comparison is a form of difference evaluation (Pomeranz, 2017). We will apply this comparison to estimate the effects of interest to our study. To identify causal effects of treatment, one would ideally compare the same unit both in the case of receiving treatment and in the case of not receiving treatment. Then one would evaluate the difference in their outcomes. This is of course not practically possible, as the unit either receives treatment or not. A second-to-best option can be to compare two groups that are, on average, similar and follow the same trend. One group then serves as treatment group receiving treatment and the other as control group not receiving treatment. Under the assumption that the two groups would have otherwise developed identically, the causal effect of treatment is observable in the difference in the respective outcomes (Angrist & Pischke, 2014).

For the purpose of this study, we would therefore ideally like to identify a similar country that did not implement any NPIs. We could then compare the labor market situation of this country with the labor market situation of the countries that did implement NPIs. This was also our initial plan, as we assumed that Sweden did not implement as many and as strict NPIs as the other two countries. However, after examining the NPI data we found that Sweden implemented all the same NPIs as Norway and Denmark, though with generally less stricter levels. Out of the three countries, Sweden also had the highest values on the "Stringency Index" and "Health and Containment Index" for longer periods of time. Furthermore, there are multiple treatments in multiple time periods, varying between the countries. Hence, we proceed with a comparison of each country to itself, prior to and after NPI implementations and increases in their strength. This equals a pre- versus post-treatment comparison, where the estimation window serves as pre-treatment, the event is the treatment, and the event window serves as the post-treatment. By doing so, we can analyze how unemployment and furlough spells changes throughout the period of the study. We can then evaluate whether these changes are related to NPI implementations or increases in their strength.

A pre- versus post-treatment comparison could generate biased estimates due to unobserved differences. However, in some settings it can still provide credible estimates when studying only one group (Pomeranz, 2017). One important assumption is that nothing but NPI implementations or increases in their strength affected the changes in unemployment and furlough spells during the period studied. This assumption implies that the labor markets would have developed in the same fashion as before NPI implementation or increases in their strength. Naturally, there are a number of things that could affect unemployment and furlough spells over time, other than the level and number of active NPIs. The labor market is known to have seasonal fluctuations, with periods of higher unemployment and furlough rates during a normal year. Yet, we need to assume that for the period of our study, the NPI implementations and increases in their strength are what influenced changes in the labor market. We make this assumption in order to proceed with our comparison.

To strengthen this assumption, we observe several events during the period of our study, identified by the same criteria. In addition, all events and their corresponding labor market situations are observed in all regions for each country. Studying data collected over time makes it possible to eliminate the variability in the response variable due to effects that are constant over time and identical for all the countries (Finseraas & Kotsadam, 2013). Furthermore, the intervals of the estimation and event windows are relatively short. We assume that they do not contain changes related to factors other than the event. In other words, we assume that unemployment and furlough spells would have remained stable in these intervals without active NPIs. Based on this, we attribute changes in unemployment and furlough spells to the NPI implementations and increases in their strength.

5.3 Regression Analyses

For our regression analyses we fit linear models with the possibility of including time- or unit-fixed effects in the regression equations. We use ordinary least squares (OLS) to fit the models and estimate the regression coefficients. In this method the coefficient estimates are obtained by minimizing the sum of squared residuals. The residuals are equal to the differences between observed response values and response values predicted by the model (Gareth et al., 2021). We fit different linear models by using the OLS method in order to explore each of our hypotheses.

Hypothesis 1: *Events of NPI implementation or an increase in NPIs strength will result in an increase in the number of unemployed or furloughed workers.*

To investigate this hypothesis, we examine the full data set. This contains weekly, regional data on unemployment, furlough spells, and the strength of NPI groups for the three countries. We also include an "event-time" factor variable, which represents the time to or since an event. In other words, the value of this variable represents the estimation window, the event date, and the event window. We also include a variable for unemployment and furlough spells per 100 000 inhabitants in the different regions. This makes the numbers for the different regions comparable. Standard errors are clustered on regions for furloughs and on country for unemployment. The regression model is as follows:

$$Y_t = \gamma_0 + \gamma_\tau + e_{i,t} \qquad (5.1)$$

Equation 5.1

The dependent variable, Y_t, represents the number of unemployed or furloughed people per 100 000 inhabitants in week *t*. We fit this model twice, once with unemployment per 100 000 as the dependent variable and once with furloughs per 100 000 as the dependent variable. The "event-time" variable, γ_τ, is a factor variable which takes on the value -4 four weeks prior to the event date, -3 three weeks prior to the event date, and so on. The event date takes on the value 0, while the week after the event takes on the value 1. Two weeks after the event takes on the value 2, and so on up until four weeks after the event date.

The intercept, γ_0, represents the reference category among the nine levels of the factor variable γ_τ. In this regression, the reference category is the week prior to implementation (i.e., event-time equal to -1). The intercept can be interpreted as the average number of unemployed or furloughed people per 100 000 in the week prior to the event. The other estimated coefficients represent the average effect for each of the eight remaining levels (i.e., weeks) of the "event-time" variable γ_τ, relative to the reference category. Comparing the effect across all events allows us to evaluate how the number of unemployed or furlough per 100 000 changes in the weeks surrounding the event date and on the event date itself. These changes are relative to the number of unemployed or furloughed per 100 000 for the week prior to the event. This gives us an indication of the general effect of events, i.e., the average effect of implementing NPIs or increasing their strength.

However, the regression above does not imply how each individual event has affected the number of unemployed or furloughed people per 100 000 inhabitants. To evaluate this, we fit a new linear model using OLS in order to estimate coefficients for each event. We consider each event as a treatment. Then, we apply a pre- versus post-treatment event study to evaluate the effect of each event. For this regression analysis we use a different data set, containing observations for the 4 weeks prior to each event, the week of the event, and the 4 weeks after the event. Observations for weeks not included in either the estimation window, the event date or the event window are removed. The data set also contains the corresponding weekly number of furloughed people per 100 000 inhabitants, on a regional level for each country. Finally, we include a dummy variable for each of the individual events. Standard errors are clustered on regions, as we only examine furlough spells in this analysis. The effect of each event is estimated by fitting the model below:

$$Y_{i,t} = \beta_0 + \sum \beta_i * event_{i,t} + e_{i,t} \qquad (5.2)$$

Equation 5.2

The dependent variable, $Y_{i,t}$, represents the number of furloughed people per 100 000 inhabitants for event *i* in week *t*. For this model we compare two periods: pre-treatment versus post-treatment. The variable $event_t$ is a dummy variable that indicates whether week *t* is in the pre- or post-treatment period for event *i*. The variable takes on a value of 0 for the four weeks prior to event *i*. It takes on a value of 1 for the week of event *i*, as well as the four weeks after event i. In other words, the variable equals 1 in the weeks we expect the event to have an effect. Thus, β_i is the coefficient of interest and represents the effect of event i relative to the period before the event. This period is defined by the intercept β_0, serving as the reference category. Therefore, the estimated value of the intercept represents the average number of furloughed per 100 000 in the weeks prior to NPI implementations or increases in their strength.

Using the estimated effects of each event, we can analyze how the different NPI categories influence unemployment and furlough spells. This is based on an identification of NPIs active in the events and estimation of their corresponding effects. This allows us to investigate the following hypothesis:

Hypothesis 2: *Not all categories of implemented NPIs will result in an increase in the number of unemployed or furloughed workers.*

To evaluate hypothesis 2, we include the coefficient estimates for each event, obtained by β_i, in a new data set. This data set contains all the events, together with the estimated effect of each event. It also includes variables for the different NPI groups and their level of strength in the weeks of the events. Our goal is to identify how different NPI groups affect the number of furloughed people per 100 000 inhabitants. To examine this, we fit the following model using OLS regression:

$$E_i = \delta_0 + \sum \delta_j * NPI_j + e_{i,j} \qquad (5.3)$$

Equation 5.3

The variable NPI_j represents the different NPI groups. An overview of these is found in appendix A3. We recode the variables of the groups so that they take on a value of 1 if the NPI group is active as a requirement. It takes on a value of 0 if it is active as a recommendation only, or not active at all.

The original NPI grouping consists of 12 different NPI groups. As our sample only contains a total of 10 events, including all 12 NPI groups in the regression will lead to low explanatory power. We therefore sort the 12 groups into even broader categories to reduce the number of variables. “Restrictions on public gatherings” and “cancellation of public events” are merged into one variable. If one of the two original variables had a value of 1, the new variable will take on a value of 1 as well. “Face covering policies”, “testing policies” and “contact tracing” are also merged into one variable. If more than one of the original variables had a value of 1, the new variable will take on a value of 1.

In addition, we decide to leave out some of the NPI groups completely. The variable “public information campaigns” has a value of 1 in all events. However, we do not expect it to have a real effect on the labor markets and we therefore leave it out. As “public transport closures” has a value of 0 in all events, we leave this out as well. Finally, “restrictions on domestic travel” has a value of 1 in week 12 in 2020 in Norway only. This is the event with the largest effect. Because of this, we find it likely that the estimated effect of this NPI group will be much larger than its real effect and that it will contaminate the results. We therefore decide to leave it out and include it in a robustness test instead. After the recoding and sorting of NPI groups, all the variables have a value of 0 for the event in week 6 in 2020 in Sweden. The rest of the events have different combinations of 0’s and 1’s for the different NPI groups. In general, Norway and Denmark have more 1’s and Sweden has more 0’s, compliant with their respective NPI strategies, mentioned in subchapter 2.4.

We are left with a total of 6 binary variables for different NPI groups to be included in the regression. The coefficient of interest is therefore δ_j, representing the average effect of NPI group j, if active as a requirement. The dependent variable, E_i, is given by β_i from Equation 5.2 and equals the estimated effect of each event. The intercept, δ_0, represents the average effect of an event if no NPI is active as a requirement.

By estimating Equation 5.2 and Equation 5.3, we are also able to investigate our third and final hypothesis:

Hypothesis 3: *NPI implementations or increases in NPI strength in the “first wave” of the pandemic will have a greater effect on unemployment or furlough spells than those in later waves.*

Equation 5.2 estimates the effect of each individual event. We know the dates of all the events and are thus able to determine whether they occurred in the “first wave” of the pandemic. The estimated coefficients allow us to compare the different effects of each event on unemployment and furlough spells. Consequently, we are able to evaluate which events had a greater impact and in what stage of the pandemic they occurred. To further investigate this, we include a dummy variable that serves as a first wave indicator to the data set used to estimate Equation

5.3. This allows us to confirm whether our conclusion based on the results of Equation 5.2 still holds. We do so by fitting the following model using OLS regression:

$$E_i = \alpha_0 + \alpha_f * first_{wave_i} + e_{i,f} \qquad (5.4)$$

Equation 5.4

The dependent variable, E_i, is still given by β_i from Equation 5.2 and is the estimated effect of each event. The variable $first\_wave_i$ is a binary variable that takes on a value of 1 if event i occurred in the "first wave", and 0 otherwise. Its coefficient, α_f, estimates the effect of an event occurring in the "first wave", relative to events occurring in later waves of the pandemic. In other words, it is the difference between events in the "first wave" and events in later stages. The average estimated total effect of a "first wave"-event is thus equal to $\alpha_0 + \alpha_f$. The intercept, α_0, represents the average effect of an event if it did not occur in the first wave, i.e., when $first\_wave_i$ equals 0.

6. Empirical Results

This chapter presents the empirical findings based on our regression results. The aim is to provide answers to our three hypotheses and our research question: *How does the implementation of NPIs affect the labor markets of the Scandinavian countries in the longer run?* To answer this, we have applied an event study methodology along with a pre- versus post-treatment comparison. We have estimated the different coefficients of interest by fitting linear models with an OLS-method. First, this chapter will present the results from the event regressions, found by estimating both the average and individual effect of events. Then, it presents the results from the NPI regression, obtained by estimating the effects of different NPIs. Finally, it evaluates whether events in the "first wave" of the pandemic had a greater impact on labor markets than those in later waves. In this chapter, we refer to implementations of NPIs or an increase in their strength as an event.

6.1 Effects of Events

The following sections focus on the results of the event regression analyses. The aim is to answer our first hypothesis. It regards the effect of implementations of NPIs or increases in their strength on unemployment and furlough spells. We analyze the results to determine both the general and individual effect of events. In order to do so, we first estimate Equation 5.1 to determine the general effect of events. The independent variable of the equation is "event-time". This is a factor variable identifying the 4 weeks before and after the event week, plus the event week itself. First, we fit the model with the number of unemployed people per

100 000 inhabitants in each country as the dependent variable. Then we fit the same model with the number of furloughed people per 100 000 inhabitants in each region as the dependent variable. The estimates are presented in Table 6.1 and Table 6.2 (respectively) below, together with an interpretation of the results.

Table 6.1

General Effect of Events on Unemployment

Coefficients	Estimate	Cluster s.e.	95% CI		p
			LL	*UL*	
Intercept [a]	2876.46	703.36	1497.87	4355.05	<.001
event_time-2 [b]	-108.06	56.63	-219.05	2.93	.061
event_time-3 [b]	-126.90	51.50	-227.84	-25.96	.016
event_time-4 [b]	-130.39	64.72	-257.24	-3.54	.048
event_time0 [b]	-12.91	151.54	-309.93	284.11	.932
event_time1 [b]	-18.14	187.88	-386.38	350.10	.923
event_time2 [b]	17.63	190.45	-355.65	390.91	.927
event_time3 [b]	50.41	206.18	-353.70	454.52	.808
event_time4 [b]	101.83	207.13	-304.14	507.80	.623

Table 6.1

Note. CI = confidence interval; *LL* = lower limit; *UL* = upper limit.

[a] Event time = -1. [b] Number at the end indicates weeks since (negative: until) event.

The intercept is the omitted category, also known as the reference group. In our analysis, this is event-time equal to -1, i.e., the week prior to the event date. The value of 2876.46 is the average number of unemployed people per 100 000 inhabitants in the week prior to an event. This coefficient estimate is statistically significant on a 0.0-level. However, the clustered standard error is relatively big, which leads to quite large confidence intervals (± 1384,47). The remaining weeks in the estimation window has a lower level of statistical significance (0.1 and 0.05). The estimations for the event week and weeks in the event window, which aims to capture the effect of events, does not have any statistical significance.

The estimated coefficients show the difference in the average number of unemployed per 100 000 in a given week, relative to the week prior to the event. To find the average total number in a given week, we therefore add the coefficient for that week to the value of the intercept. By doing this for all weeks, we get the estimated total number of unemployed per 100 000 in each week. The results show that, on average, there is a slight increase in unemployment during the weeks studied. The average increase from four weeks prior to an event to four weeks after an event is about 232. As the numbers are per 100 000 inhabitants, this is a relatively small increase. There is no statistically significant effect of events on unemployment in the weeks we are most interested in, i.e., the event window. Results are further illustrated in Figure 6.1 below:

Figure 6.1

Total Average Number of Unemployed per 100 000

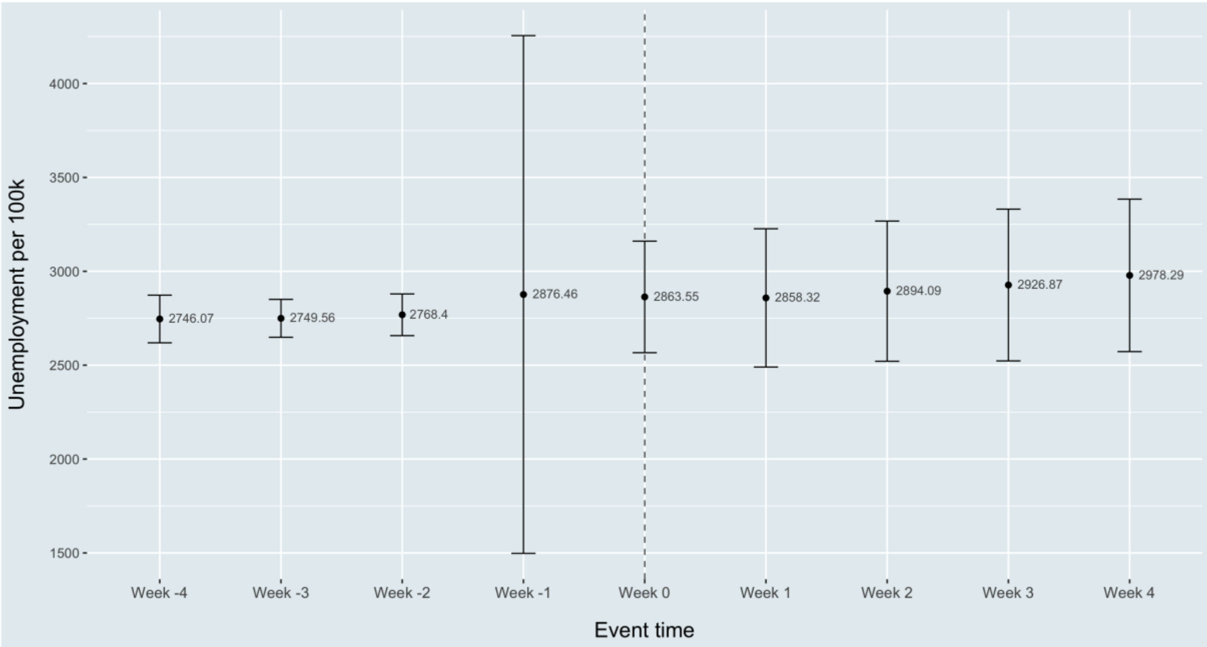

Figure 6.1

Figure 6.1 shows the average, total number of unemployed per 100 000 inhabitants in the weeks surrounding an event. The figure illustrates how there is no particular increase in unemployment in the weeks following an event. A figure illustrating the average change, relative to one week prior to an event, can be found in appendix A4.

Table 6.2

General Effect of Events on Furlough Spells

Coefficients	Estimate	Cluster s.e.	95% CI		p
			LL	*UL*	
Intercept [a]	466.00	52.45	363.20	568.80	<.001
event_time-2 [b]	119.68	19.29	81.87	157.49	<.001
event_time-3 [b]	117.32	19.56	78.98	155.66	<.001
event_time-4 [b]	137.76	22.68	93.31	182.21	<.001
event_time0 [b]	263.07	79.35	107.54	418.60	<.001
event_time1 [b]	847.63	104.61	642.59	1052.67	<.001
event_time2 [b]	1123.79	102.51	922.87	1324.71	<.001
event_time3 [b]	1237.00	111.87	1017.73	1456.27	<.001
event_time4 [b]	1365.28	127.39	1115.60	1614.96	<.001

Table 6.2

Note. CI = confidence interval; *LL* = lower limit; *UL* = upper limit.

[a] Event time = -1. [b] Number at the end indicates weeks since (negative: until) event.

When estimating the general effect of events on furloughs, however, all weeks are of statistical significance. Additionally, they all have a p-value of less than 0.001. The interpretation of the results, presented in Table 6.2 above, is the same as that for unemployment. The intercept of 466 represents the average number of furloughed people per 100 000 in the week prior to an event. The coefficients for the different event times represents the average change in furlough spells per 100 000, relative to the week prior to the event (event time -1). For instance, in the week of an event the number of furloughed increases on average by 263.07, relative to the week prior to an event. Thus, the average total number of furloughed people per 100 000 in the week of an event is equal to 729.07 (466+263.07).

If we calculate this number for all the weeks studied, we see that there is, on average, a relatively large increase in furlough spells during the period. From four weeks prior to an event to four weeks after an event, the number of furloughed people per 100 000 increases on average by 1227.52. Additionally, we can see from the results that this increase is largest in the weeks following an event. This is further illustrated in Figure 6.2 below:

Figure 6.2

Total Average Number of Furlough Spells per 100 000

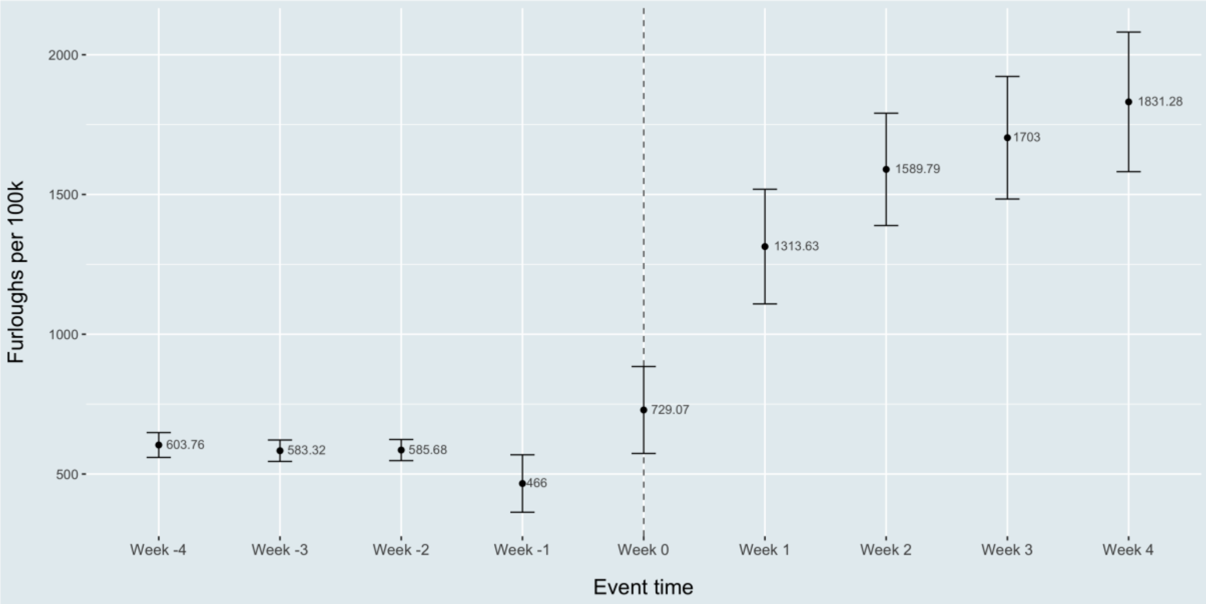

Figure 6.2

Figure 6.2 shows the average, total number of furlough spells per 100 000 inhabitants in the weeks surrounding an event. A figure illustrating the average change, relative to one week prior to an event, is presented in appendix A4. From Figure 6.2 we can easily see a large increase in furlough spells in the weeks following an event. Thus, there seems to be a significant, general effect of events on furlough spells. However, the plot indicates a relatively large increase from the event week to one week after an event. This implies that an event does not necessarily have an immediate effect, and that it might have a lag of week. This could suggest that furlough spells start in the week after NPI implementations or increases in their strength, instead of the same week. In the following weeks the increase is more stable. This suggests that the effect of events lasts for weeks, possibly even beyond our event window.

A likely interpretation of the results is that companies choose to furlough their workers rather than firing them in the weeks following an event. This seems reasonable, as it is generally a very high threshold for firing workers in the countries of our study (Langseth, n.d.). As described in chapter 2.5, the three countries also offered job retention programs. The aim of these programs is to help businesses manage challenges caused by the pandemic, while retaining employment contracts. It is reasonable to assume that the availability of these programs influenced companies to choose furlough spells over dismissals, when possible.

Our results indicates that any event in general results in an increase in the number of furlough spells in the weeks following the event. However, the results suggest no such effect on unemployment. We therefore decide to focus on furlough spells for the rest of our analyses. We continue our analyses by investigating the effect of each individual event on the number of furloughed people per 100 000 inhabitants. In order to do so, we estimate regression Equation 5.2 from chapter 5.3. Table 6.3 below presents the results of the regression.

As we see from the regression results in Table 6.3, all events in our sample are statistically significant. They all have a p-value of less than 0.001, except for week 50 in 2020 in Denmark which has a p-value of .004. The intercept represents the reference group. For this regression, this is the weeks corresponding to pre-treatment. In other words, it is the four weeks prior to an event, defined as the estimation window. Its value of 566.80 equals the average number of furloughed workers per 100 000 in the weeks leading up to an event. Each of the estimated coefficients represents the average change in the number of furloughed people per 100 000 in the event window. This change is relative to the estimation window. In other words, it shows the average change in furlough spells post-treatment, relative to pre-treatment, for each event. Thus, the coefficients show the relative effect of NPI implementations or increases in their strength for each individual event, relative to the preceding weeks.

Table 6.3

Effect of Each Event on Furlough Spells

Coefficients	Estimate	Cluster s.e.	95% CI		p
			LL	*UL*	
Intercept [a]	566.80	55.46	458.10	675.50	<.001
nor_12_20 [b]	4190.81	169.92	3857.77	4523.85	<.001
nor_45_20 [b]	393.93	95.44	206.87	580.99	<.001
nor_4_21 [b]	592.04	120.70	355.47	828.61	<.001
dnk_11_20 [b]	2466.09	714.10	1066.45	3865.73	<.001
dnk_46_20 [b]	-479.45	68.12	-612.97	-345.93	<.001
dnk_50_20 [b]	872.35	303.92	276.67	1468.03	.004
dnk_1_21 [b]	1534.09	454.75	642.78	2425.40	<.001
swe_6_20 [b]	-566.80	55.46	-675.50	-458.10	<.001
swe_12_20 [b]	1364.12	147.83	1074.37	1653.87	<.001
swe_48_20 [b]	493.16	67.47	360.92	625.40	<.001

Table 6.3

Note. CI = confidence interval; *LL* = lower limit; *UL* = upper limit.

[a] Pre-treatment. [b] First number = calendar week; last number = year.

To find the total number of furloughed people per 100 000 following an event, one adds the value of the coefficient for the event to the value of the intercept. For example, the estimated average change in furloughed people per 100 000 after the event in week 12 in 2020 in Norway is 4190.81. The estimated total number of furloughed people per 100 000 after this event is therefore equal to 4757.61 (566.80+4190.81). Figure 6.3 below illustrates the average, total effect of each event. The change relative to pre-treatment can be found in in appendix A4.

Figure 6.3

Total Average Effect of Each Event

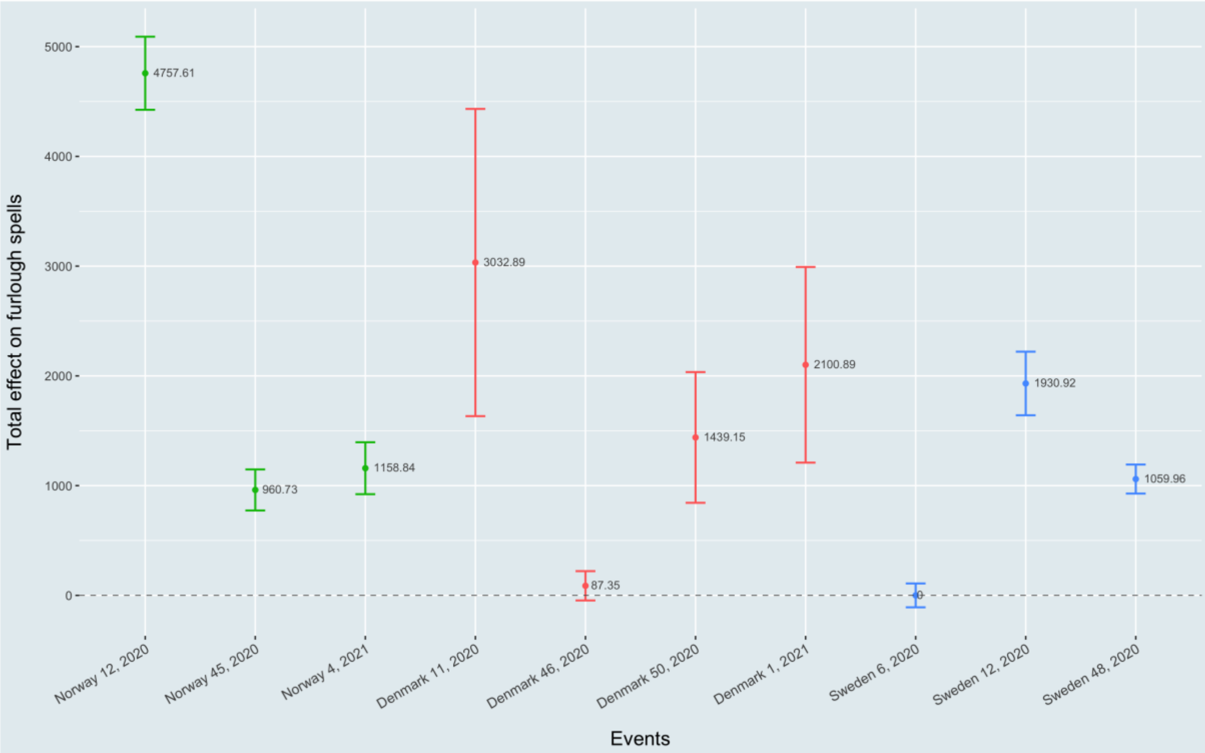

Figure 6.3

In Figure 6.3, we see that two of the events in our sample has a negative estimated coefficient. In week 6 in Sweden the estimated average effect on furlough spells is -566.80. Given an intercept of 566.80, the estimated total number of furloughed people in this week equals 0, as seen in Figure 6.3. This may seem strange at first, but from chapter 2.5.2 we know that Sweden did not introduce its furlough program until week 12 in 2020. Thus, there was in fact no furloughed people in week 6, as the furlough program did not exist at the time.

In week 46 in 2020 in Denmark, the estimated average effect on furlough spells is -479.45. Thus, the estimated total number of furloughed people per 100 000 after this event is 87.31, as seen in Figure 6.3. During this time of the year, there were multiple NPI implementations or changes in their level of strength in Denmark. Three of the events in our sample is from this time period, and their overlap estimation and event windows overlap. Week 46 is the first of these three events, and it is shortly followed by two events (week 50 and 1). Both of these have estimated coefficients that implies an increase in the estimated effect of furlough spells.

The time period preceding week 46 (the late summer and early autumn of 2020) was characterized by relatively low infection rates in all three countries. The following weeks were then characterized by increasing infection rates (figure A1). As mentioned in chapter 2.2, this period is regarded as the start of the second wave. Although it is an assumption only, it is reasonable to assume that Danish authorities first tried to contain the virus by implementing less strict measures in week 46. Then, it is possible that they saw the need to increase their strength in the following weeks to succeed. This would explain the increase in the estimated coefficients of week 50 and 1, that follows the negative coefficient in week 46. This suggests that companies may not have felt the need to furlough workers at first, until measures became stricter in the following weeks.

This assumption is supported by a relatively low Stringency Index prior to week 46, followed by an increasing Stringency Index during the weeks following this event. This is true for both Denmark and Norway. We can also see from the regression results that week 45 in Norway and 48 in Sweden, both in 2020, have relatively low effects on furlough spells. We do not have any events after this week in Sweden. However, for week 4 in 2021 in Norway there is an increase in the estimated coefficient, compared to week 45 in 2020. We therefore find it likely that the assumption discussed in previous sections may be true for all countries.

The effects of each event on the number of furloughed people per 100 000 inhabitants forms the basis for out further analysis. Our second hypothesis concerns the effects of the different categories of NPIs implemented during the period of the study. We intend to answer this by identifying how each of the different NPI groups influences the effect of the events. We elaborate on this analysis in the following subchapter.

6.2 Effects of NPI groups

In this subchapter, we investigate our second hypothesis. The aim is to evaluate whether different categories of NPIs affect the labor markets differently. In order to analyze the effect of different NPI groups, we estimate Equation 5.3 from chapter 5.3. We use the estimated effect of each event as the dependent variable. Dummy variables for different NPI groups serve as independent variables. The regression results are presented in Table 6.4 below.

Firstly, we should mention that our event sample consists of only 10 events. There are many possible categories of NPIs, based on how they are grouped. This leads to a large number of potential predictors. Thus, a sample of 10 events represents a relatively small number of observations. For a regression analysis on the effect of NPI groups to be more accurate, it would be beneficial to have even more events in our sample. We would then be able to include more NPI groups as predictors as well. Data on NPI measures on a regional level, instead of on a national level, would also improve the accuracy. However, we do believe that our analysis can give an indication of the different effects of the NPI groups.

Our initial assumption was that some of the NPI groups would lead to a larger increase in furlough spells, such as workplace closures, cancellations of or restrictions on public events, and travel restrictions. We had this assumption because we believed such restrictions would likely lead to changes in labor demand, either directly or indirectly. This is discussed in more detail in chapter 1.1 and 2.3. Initially, we also assumed that not all NPI groups would lead to an increase in furlough spells, like school closures. Digital education was introduced when schools closed, so it is reasonable to assume that teachers still had full time working hours.

Table 6.4

Effect of Different NPI Groups

Coefficients	Estimate	s.e.	95% CI		p
			LL	*UL*	
Intercept [a]	100.2	135.9	-166.16	366.56	.463
school closure	-565.2	322.7	-1197.69	67.29	.083
workplace closure	2036.7	209.1	1626.86	2446.54	<.001
stay-at-home	702.9	422.8	-125.79	1531.59	.099
international travel	1663.0	273.1	1127.72	2198.28	<.001
tisk strategy [b]	2327.0	463.8	1417.95	3236.05	<.001
public gatherings [c]	-4009.6	403.5	-4800.46	-3218.74	<.001

Table 6.4

Note. CI = confidence interval; *LL* = lower limit; *UL* = upper limit.

[a] Average effect when all NPIs equals 0. [b] Testing, tracing and face covering policies.

[c] Restrictions on public gatherings and cancellation of public events.

Our regression results confirm that different NPI groups had different effects, and that not all NPI groups contributed to an increase. For example, the NPI group for school closures has a negative estimated coefficient, with a value of -565.2. This number is the average, relative effect of implementing school closures as a requirement, all else equal. The total estimated effect is found by adding this value to the value of the intercept. Doing so, gives a total effect of -465, if no other NPI group is active. Consequently, school closures alone are not contributing with an increase to the effect of events. Cancellations of or restrictions on public events also has a large, negative estimated coefficient. This is opposed to our initial assumption. However, this NPI group was only active in the events with smaller or negative estimated effects, which may explain the negative value. Workplace closures, however, has an estimated coefficient of 2036.7. If this NPI group becomes implemented as a requirement in an event, the average effect of the event increases by 2036.7, all else equal. This corresponds with our initial assumption. Figure 6.4 below illustrates the total, average effects of each NPI group:

Figure 6.4

Total Average Effect of Each NPI Group

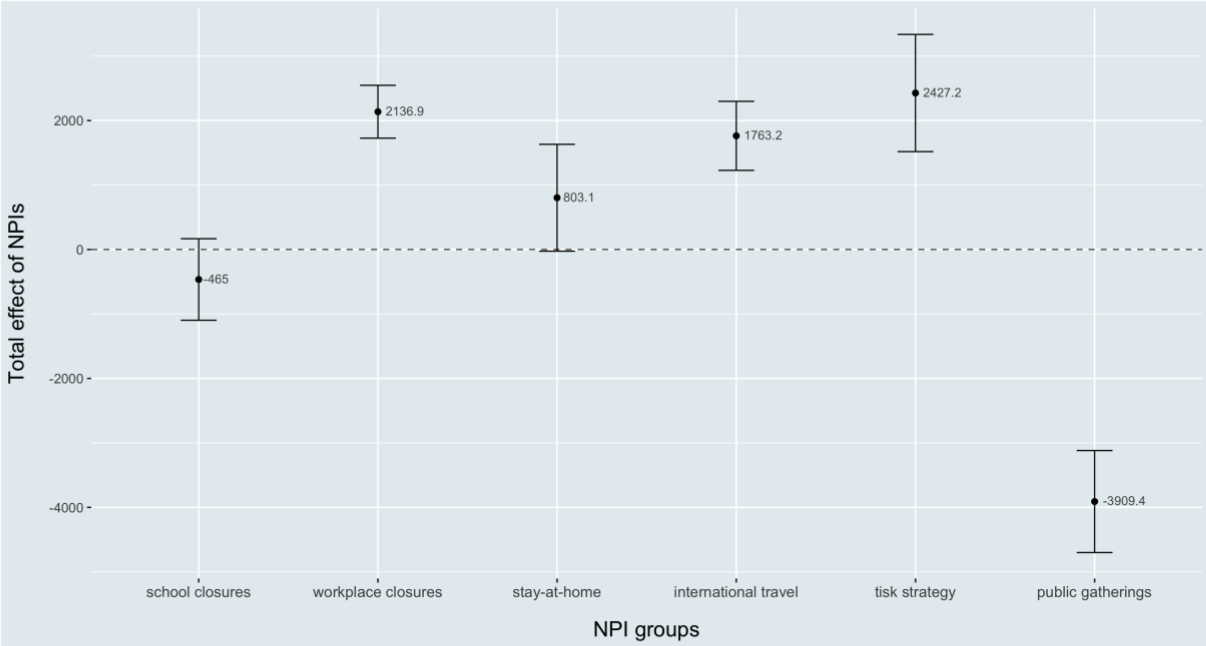

Figure 6.4

The regression results suggests that workplace closures, restrictions on international travel, and testing, contact tracing and face covering policies contributes to the largest increases in the effect of events. These are all significant with a p-value of less than 0.001. Stay-at-home requirements contributes to a somewhat smaller increase, but on a somewhat lower level of significance. School closures, together with cancellations of and restrictions on public events, contributes to a decrease in the estimated effect of events, according to the regression results. These findings support our hypothesis, but not in the same ways as we assumed for the different NPI groups.

To predict the total average effect of an event, one adds the value of all the coefficients of the NPI groups active in the event to the value of the intercept. When doing this for our 10 events, we can predict the average estimated effect of three of them accurately. These three events are week 4 in 2020 in Norway, week 11 in 2020 in Denmark, and week 48 in 2020 in Sweden. For the event in week 12 in 2020 in Sweden, the prediction of the average effect of the event is fairly accurate. This prediction is just above the lower limit of the confidence interval for the estimated effect of this event. The predictions of effects of the remaining 6 events are not as accurate. For the event in week 1 in 2021 in Denmark, the prediction is just below the upper limit of the confidence interval for the estimated effect of the event. Predictions for the other 5 events is outside of the confidence intervals for the estimated effects of these events. The predictions of all 10 events can be found in appendix A5.

However, the estimated effects of events and of NPI groups are just that – estimated. Effects of events are based on estimations from regression Equation 5.2. Additionally, the standard errors of the coefficients of the NPI groups are also relatively large. This results in large confidence intervals. Therefore, one could use quite different values for the effect of NPIs, still within these intervals, when making the predictions discussed above. Then, it is possible that the predictions for all of the events would be withing the confidence intervals of the estimated effects of each event, obtained by regression Equation 5.2.

6.3 Early Stages Versus Later Stages

Our third and final hypothesis concerns the effect of events in early stages of the pandemic versus the effect of events in later stages. Our assumption is that events happening in the "first wave" results in a larger increase in furlough spells, than those happening in later waves. To investigate this, we start by looking at the effects of each event from Equation 5.2. The results of the regression are presented in Table 6.3, and thoroughly discussed in subchapter 6.1. We know the date of each individual event and are therefore able to determine whether it happened in the "first wave" of the pandemic. As discussed in subchapter 2.2, the spring of 2020 is commonly regarded as the "first wave" in our three countries.

There are three events in our sample that can be defined as occurring in the "first wave", one in each country: week 12 in 2020 in Norway and Sweden, and week 11 in 2020 in Denmark. These are also the weeks or events with the largest estimated effect on furlough spells, for each country. The events in these three weeks are also statistically significant on a 0.0-level. The remaining weeks or events in each country has a notably smaller effect on furlough spells. Thus, based on the regression results from Equation 5.2, we conclude that events occurring in the "first wave" of the pandemic leads to a larger increase in furlough spells than those occurring in later waves. To investigate this even further, we estimate Equation 5.4 from subchapter 5.3. The results are presented in Table 6.5 below.

Table 6.5

Effect of "First Wave" Events

Coefficients	Estimate	s.e.	95% CI		p
			LL	*UL*	
Intercept [a]	239.67	98.49	46.63	432.71	.017
first wave [b]	2113.73	174.38	1771.95	2455.51	<.001

Table 6.5

Note. CI = confidence interval; *LL* = lower limit; *UL* = upper limit.

[a] Average effect of events in later waves. [b] Average difference "first wave" and later waves.

The equation includes a dummy variable that indicates whether the given event occurred in the "first wave" of the pandemic. The intercept is the average effect of events that did not occur in the "first wave". The value of the estimated coefficient, 2133.73, represents the increase in the average effect if an event happened in the "first wave", relative to later waves. The results imply a relatively large increase in the estimated average effect of "first wave" events, that are statistically significant with a p-value of less than 0.001.

The results further support our hypothesis that events occurring in the early stages of the pandemic has a larger effect on furlough spells, than those in later stages. This can be explained by several reasonable assumptions. As the pandemic progresses, people might get tired of adhering to strict measures and thus experience a sort of "NPI fatigue", leading to lower compliance. In the "first wave", authorities had very little insight to use as decision support when passing measures. The NPIs introduced in later stages have the possibility to be based on more credible knowledge and experience than those in the "first wave". Thus, they may be more targeted and have less negative "side effects". Finally, the effect of an individual NPI group being implemented at a later point might be smaller if there are already several other NPIs groups active at the time.

Figure 6.5

Total Average Effect of "First Wave" Events

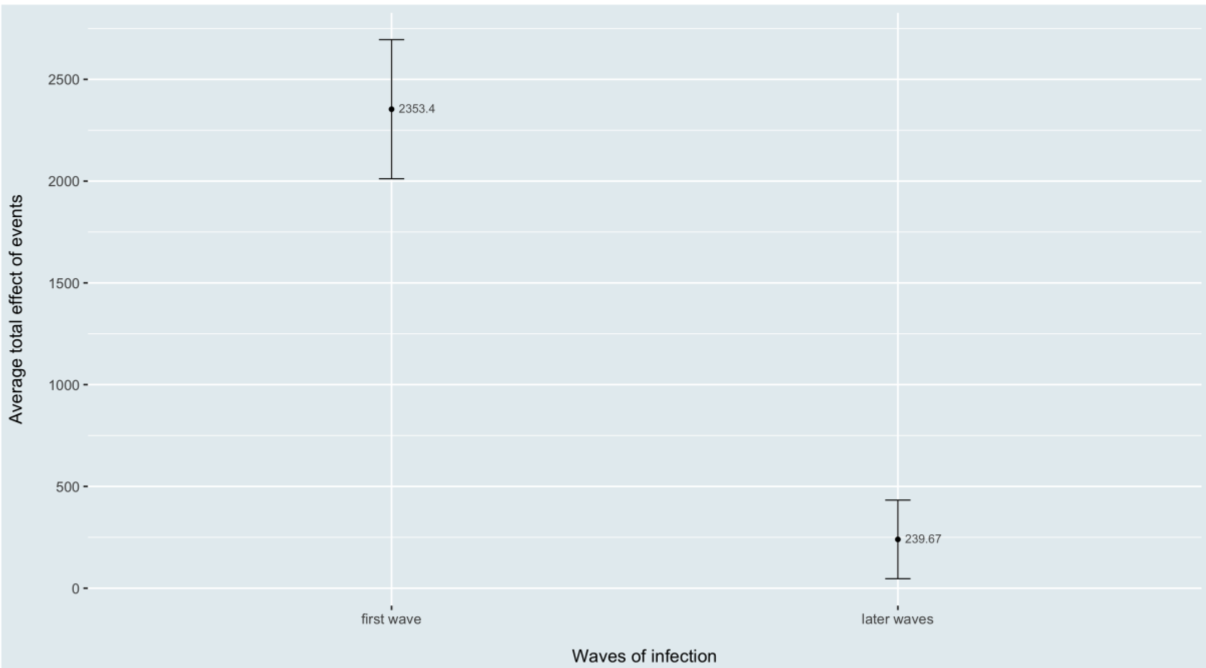

Figure 6.5

7. Robustness Tests

Our data analyses depend on different assumptions as well as estimates for missing data. Therefore, we conduct several robustness tests to evaluate the validity of our analyses and results. This way we will be able to observe how our results respond to changes in our assumptions and model specifications. First, we will run all the regression analyses with a different set of estimates for data in weeks where this is missing. As discussed in chapter 4.2.1., there are several weeks in our data set where we had to estimate the number of furloughed and unemployed. By controlling our results when using a different estimate, we can evaluate how robust our original analyses are. Then, we will control for fixed effects in our event regression analyses. We will use this test to evaluate if and how such controls change the estimated effect of events. Finally, we will include a dummy variable for the NPI group of restrictions on domestic travel in our NPI regression analysis. We have left this variable out of our main analysis, as we suspect it would be assigned a larger effect than it really has. We will use this test to examine how including this variable may change the effect of different NPIs.

It would also be interesting to experiment with a different event date. In our original analyses we assume that the NPIs have an immediate effect on the labor market. We have therefore set the event date to the week of implementation. From our event regression, we observe a relatively big change in the effect of an event from the week of implementation to one week later. In addition, the effect of the implementation may last longer than the following four weeks. An option would thus be to set the event date to one week after implementation while keeping the event window at four weeks. This would allow us to evaluate our assumption of immediate effect versus an assumption of effect with a week's lag. We would also be able to observe the effect five weeks after implementation. This would allow us to examine our assumption that a four week-long event window captures the effect of events. However, as previously discussed, we have overlapping estimation and event widows. This affects the possible length of the windows. In some cases, setting the event date to one week after implementation would lead to an event window of just three weeks, as the next event occurs then. Based on the limitations presented by the data, we decide to keep our event date, as well as estimation and event windows, unchanged.

7.1 Different Estimation of Missing Data

For our first robustness test we perform the same regression analyses as described in chapter 5 and 6. However, we use a different set of estimation for furlough and unemployment data in weeks where this is missing. The estimation used in the original analyses is described in chapter 4.2.1. In this robustness test we refer to the new set of estimations as “estimation type 2”. These are obtained by subtracting the value of the first week with reliable data from the value of the last week with reliable data. This difference is then divided by the number of weeks between the two reliable data points. This value is the average weekly change in furlough spells or unemployment between the weeks with reliable data. The weekly average change is used to calculate the number of furloughed or unemployed in the weeks with missing data. This is done by adding the average weekly change to the value of the previous week, starting with the first week of reliable data. We repeat this until all missing weeks have estimated values. The change in furlough spells or unemployment between weeks with estimated data will thus be the same.[6] The results obtained by using estimation type 2 in the regression analyses are presented in the next subchapters. In the following sections, an event is considered as implementations of NPIs or increases in their strength.

[6] A mathematical formulation of the estimation can be found in the appendix A6.

7.1.1 General Effect of Events

Table 7.1

General Effect on Furloughs Using Estimation Type 2 in Equation 5.1

Coefficients	Original analysis	Estimation type 2
Intercept	466.00 *** (52.45)	923.07 *** (65.83)
event_time-2	119.68 *** (19.29)	-20.15 * (8.70)
event_time-3	117.32 *** (19.56)	-23.79 * (9.48)
event_time-4	137.76 *** (22.68)	7.72 (13.90)
event_time0	263.07 *** (79.35)	-44.01 (79.11)
event_time1	847.63 *** (104.61)	680.94 *** (122.36)
event_time2	1123.79 *** (102.51)	1011.59 *** (111.29)
event_time3	1237.00 *** (111.87)	1149.83 *** (121.09)
event_time4	1365.28 *** (127.39)	1356.98 *** (142.16)

Table 7.1 *Equation 5.1*

*** *p <.001; * p <.05*

The coefficients in the table above represents the average effect of events on furlough spells per 100 000 inhabitants for the weeks included in the study. Using estimation type 2 for the weeks with missing data changes the estimated coefficients. It also leads to smaller standard errors in event time -4 to -2, yet larger standard errors for the event window. The changes in coefficients are greater for those representing the weeks before the event and the week of the event. However, these coefficients have a reduced statistical significance, compared to the original analysis. For the coefficients of the four weeks after the event, the change between the original analysis and the one using estimation type 2 is relatively small. These changes are also decreasing in size for each of the four weeks following an event. The coefficients obtained by estimation type 2 are for these weeks statistically significant on the same level as the ones in the original analysis.

Based on the results, it is reasonable to assume that the first type of estimated data, used in our original analysis, is reliable. The effect of an event is captured in the event window, which is the four weeks after an event in this study. Thus, it seems safe to assume that there are significant effects on furlough spells, independent of estimation method. Additionally, this effect seems to be relatively stable in the event window for both methods.

Table 7.2

General Effect of Events on Unemployment Using Estimation Type 2 in Equation 5.1

Coefficients	Original analysis	Estimation type 2
Intercept	2876.46 *** (706.36)	2978.76 *** (786.81)
event_time-2	-108.06 . (56.63)	-123.69 * (60.98)
event_time-3	-126.90 * (51.50)	-140.57 . (70.79)
event_time-4	-130.39 * (64.72)	-149.60 . (79.70)
event_time0	12.91 (151.54)	-49.69 (191.74)
event_time1	-18.14 (187.88)	-120.45 (256.86)
event_time2	17.63 (190.45)	-80.16 (245.87)
event_time3	50.41 (206.18)	-47.38 (239.73)
event_time4	101.83 (207.13)	2.34 (127.45)

Table 7.2 *Equation 5.1*

*** $p <.001$; * $p <.05$; . $p <.1$

The coefficients in Table 7.2 above represents the average effect of events on unemployment per 100 000 inhabitants for the weeks included in the study. As with furlough spells, using estimation type 2 for the weeks with missing data changes in the estimated coefficients. It also leads to larger standard errors in all weeks except week four after an event. The changes in coefficients are generally larger in the event week and the weeks of the event window, compared to the original analysis. Neither of these coefficients are statistically significant in any of the analyses. The change in the coefficients for the intercept and the weeks in the estimation window is relatively smaller. The intercept is statistically significant on the highest level in both analyses. There is some significance on lower levels of the weeks prior to the event in both analyses. However, we are more concerned about the effect of the event, captured in the weeks after the event date. The conclusion of our original analysis is that implementations of NPIs or increases in their strength has an effect on furlough spells, but not necessarily on unemployment. In other words, companies may choose to put their workers on furlough instead of laying them off. Based on this robustness test, we argue that the assumption still holds, even when using a different estimation method for weeks with missing data.

7.1.2 Effect of Each Event

Table 7.3

Individual Effect on Furloughs Using Estimation Type 2 in Equation 5.2

Coefficients	Original analysis	Estimation type 2
Pre-treatment	566.80 *** (55.46)	566.91 *** (55.45)
nor_12_20	4190.81 *** (169.92)	4155.39 *** (164.66)
nor_45_20	393.93 *** (95.44)	394.16 *** (95.34)
nor_4_21	592.04 *** (120.70)	591.93 *** (120.78)
dnk_11_20	2466.09 *** (714.10)	2465.97 *** (714.10)
dnk_46_20	-479.45 *** (68.12)	-479.55 *** (68.11)
dnk_50_20	872.35 ** (303.92)	872.27 ** (303.92)
dnk_1_21	1534.09 *** (454.75)	1533.99 *** (454.75)
swe_6_20	-566.80 *** (55.46)	-566.91 *** (55.45)
swe_12_20	1364.12 *** (147.83)	1364.00 *** (147.83)
swe_48_20	493.16 *** (67.47)	493.04 *** (67.47)

Table 7.3 *Equation 5.2*

*** $p < .001$; ** $p < .01$

There are minimal differences between the coefficients and standard errors obtained by fitting the model with the two types of estimation methods. The level of significance is unchanged for all events as well. For this regression, isolated, the type of estimation for missing weeks does not seem to affect the results particularly. The four weeks in the event window had relatively small differences in coefficient estimates in the previous robustness test and regression as well. Based on this, we still assume that the estimations used in our original analyses is reliable in capturing both general effect of events and the effects of individual events. The original analyses seem to be quite robust, independent of estimation methods.

The coefficients presented in Table 7.3 above, are used as the dependent variable in the NPI regression Equation 5.3. This regression estimates the effect of different NPIs. We do not find it necessary to run an additional NPI regression with the coefficients obtained by using estimation type 2. With such small differences between the coefficients of the two estimation methods, running the NPI regression with the new coefficients will yield similar results as the original one.

7.2 Controlling for Fixed Effects

In our main analyses we use OLS regression without fixed effects. However, unobserved variables may cause the estimation of coefficients to be biased. For example, there might be characteristics of regions that are time-invariant. In other words, they do not change over time, but do change between regions. As region is our unit of observation, this can be controlled for by adding a region (unit) fixed effect to the regressions. This captures differences that vary between regions but are common to all time units for a given region.

There might also be factors that change over time but are common to all units of observations. As week is the time unit of our analyses, this can be controlled for by adding a week fixed effect to the regression. The general business cycle and seasonal fluctuations in labor markets may influence furlough spells and unemployment as well. Controlling for year fixed effects is therefore also an option. Time fixed effects capture temporal changes or trends that are common to all regions but vary over time. The results of controlling for various fixed effect specifications in our event regressions is presented in the following subchapters.

7.2.1 General Effect of Events

Table 7.4

General Effect of Events Ehen Encluding Eixed Effects (FE) in Equation 5.1

Coefficients	Original analysis	Region FE	Week FE	Region + week FE	Region + year FE
Intercept	466.00 *** (52.45)				
event_time-2	119.68 ***	48.96 *	62.61 *	199.80 *	50.22 *
	(19.29)	(22.14)	(31.79)	(86.24)	(22.96)
event_time-3	117.32 ***	46.60 *	-262.70 ***	80.00	47.87 *
	(19.56)	(21.75)	(70.25)	(84.64)	(22.60)
event_time-4	137.76 ***	67.04 *	-338.05 ***	113.71	7.23
	(22.68)	(24.85)	(90.11)	(134.82)	(43.55)
event_time0	263.07 ***	243.68 ***	271.61 ***	200.97 ***	255.31***
	(79.35)	(61.23)	(80.52)	(39.41)	(60.90)
event_time1	847.63 ***	771.18 ***	643.01 ***	471.10 **	767.03 ***
	(104.61)	(92.07)	(132.76)	(133.02)	(93.27)
event_time2	1123.79 ***	1047.33 ***	650.32 ***	362.36 .	1043.18 ***
	(102.51)	(89.20)	(162.60)	(196.08)	(91.71)
event-time3	1237.00 ***	1160.55 ***	805.91 ***	519.10 **	1156.40 ***
	(111.87)	(98.13)	(151.26)	(181.94)	(101.36)
event_time4	1365.28 ***	1300.83 ***	1052.94 ***	583.57 .	1304.14 ***
	(127.39)	(129.23)	(264.29)	(341.79)	(134.16)

Table 7.4 *Equation 5.1*

*** $p < .001$; ** $p < .01$; * $p < .05$; . $p < .1$

The results of the estimated general effect of events seem to be quite robust when controlling for possible fixed effects. There are some variations in the level of statistical significance and some changes in the estimated coefficients. However, the trend still seems to be the same as in the original analysis. The different results still indicate a definitive effect of events on furlough spells in the week of the event and in the four weeks that follows.

For this robustness test, standard errors are still clustered on regions. All the fixed effects specifications lead to larger standard errors in the weeks of the estimation window. For the event week, the specifications generate similar or smaller standard errors. In the weeks of the event window, *region,* and *region + year* fixed effects lead to similar or lower standard errors. *Week* and *region + week* fixed effects generate larger standard errors.

As presented in the original analysis and the previous robustness test in chapter 7.1, there are generally no significant results from the regression analysis on the effect of events on unemployment. Controlling for fixed effects does not yield any results of statistical significance for unemployment either. Thus, our assumption that events mainly affect furlough spells, and not unemployment, seems to hold after this robustness test as well.

7.2.2 Effect of Each Event

Table 7.5

Effect of Each Event When Including Fixed Effects (FE) in Equation 5.2.

Coefficients	Original analysis	Region FE	Week FE	Region + week FE	Region + year FE
Intercept	566.80 ***				
	(55.46)				
nor_12_20	4190.81 ***	3765.14 ***	2180.50 ***	1512.45 **	3797.95 ***
	(169.92)	(97.11)	(563.05)	(542.84)	(103.59)
nor_45_20	393.93 ***	-31.74 **	-65.64	-711.76 ***	1.06
	(95.44)	(9.98)	(111.74)	(43.30)	(17.42)
nor_4_21	592.04 ***	166.37 ***	1100.58 ***	336.09 ***	111.67 ***
	(120.70)	(42.01)	(137.00)	(54.76)	(30.08)
dnk_11_20	2466.09 ***	2850.20 ***	1425.02 ***	1687.45 ***	2850.20 ***
	(714.10)	(681.90)	(323.24)	(280.13)	(681.90)
dnk_46_20	-479.45 ***	-145.44 *	-906.52 ***	-673.25 ***	-145.44 *
	(68.12)	(67.42)	(78.57)	(99.32)	(67.43)
dnk_50_20	872.35 **	1122.86 ***	388.90	847.02 **	1122.86 ***
	(303.92)	(282.64)	(292.75)	(264.48)	(282.64)
dnk_1_21	1534.09 ***	1868.11 ***	1890.35 ***	2269.54 ***	1780.62 ***
	(454.75)	(426.18)	(482.95)	(442.81)	(426.99)
swe_6_20	-566.80 ***	-460.50 ***	-45.43 **	-210.09 ***	-460.50 ***
	(55.46)	(24.84)	(14.48)	(55.15)	(24.84)
swe_12_20	1364.12 ***	1470.42 ***	-292.53	-292.53	1470.42 ***
	(147.83)	(128.62)	(535.60)	(526.86)	(128.62)
swe_48_20	493.16 ***	599.46 ***	-78.09	34.91	599.46 ***
	(67.47)	(31.53)	(65.58)	(45.49)	(31.53)

Table 7.5 *Equation 5.2*

*** $p < .001$; ** $p < .01$; * $p < .05$

We have also controlled for fixed effects in the regression that estimates the individual effect of each event. Although this leads to some changes in both estimated coefficients and standard errors, the trends seem to be the same. For Norway, the event in week 12 in 2020 still has the largest effect, while it is somewhat lower when controlling for fixed effects. The fixed effect results also suggests that week 45 in 2020 might have a smaller effect than estimated in the original analysis. It may even be a negative effect, when controlling for fixed effects. However, not all of these estimates are statistically significant. Week 4 in 2021 still seems to have an increasing effect on furlough spells, but of various sizes depending on the fixed effects specifications one chooses.

For Denmark, the event with the largest effect varies between week 11 in 2020 and 1 in 2021, depending on the specifications one chooses. Nevertheless, they both have a relatively large impact on furlough spells. Week 46 in 2020 has a negative effect no matter the specifications, but the size of the effect varies. Week 50 in 2020 has an increasing effect on furlough spells, but to which extent varies between the different specifications for this event as well.

For Sweden, week 6 in 2020 still has a negative effect when controlling for fixed effects. However, as we know that there in fact was no furlough spells at this time, our original analysis seems to estimate this accurately. For the two remaining weeks, controlling for *week* fixed effects and *region* + *week* fixed effects leads to a large decline in the estimated effects. However, these results are not statistically significant, unlike the estimates with *region* + *year* fixed effects for these weeks. Controlling for *region* and *region* + *year* fixed effects does not yield any large changes in estimated effect.

In general, the estimated effects of events, both general and individual effects, are fairly robust under various fixed effect specifications.

7.3 Additional Predictor in NPI Regression

In our original analysis of the effect of different NPIs, we decide to leave out the NPI group regarding restrictions on domestic travel. The reason for this is explained in chapter 5.3. As a final robustness test, we have performed the NPI regression with this NPI group included as a predictor. The results are presented in Table 7.6 below.

As expected, the results show that all the estimated coefficients change quite a lot when including restrictions on domestic travel. Standard errors are also smaller. Like we assumed, restrictions on domestic travel gets assigned a relatively large effect. However, we know that it was active as a requirement in only one of the 10 events. We also know that this is the event with the largest estimated effect. Thus, when including domestic travel, its effect may be overrated so that the estimated effects of the other NPI groups becomes less accurate. Workplace closures, for example, has a much smaller estimated effect on furloughs and is no longer significant in the robustness test. It is reasonable to assume that this is likely not true in reality. We conclude that leaving out restrictions on domestic travel in the NPI regression leads to more accurate estimations of the other NPI groups.

Table 7.6

Effect of NPI Groups, Including domest_travel as Predictor in Equation 5.3

Coefficients	Original analysis	Additional predictor included
Intercept	100.2 (135.9)	-220.98 * (91.08)
school closures	-565.2 . (322.7)	628.10 ** (228.46)
workplace closures	2036.7 *** (209.1)	230.22 (197.56)
stay-at-home	702.9 . (422.8)	2075.89 *** (293.21)
international travel	1663.0 *** (273.1)	611.18 ** (194.77)
tisk strategy	2327.0 *** (463.8)	81.96 (348.23)
public gatherings	-4009.6 *** (403.5)	-2203.15 *** (297.04)
domestic travel		2942.29 *** (235.77)

Table 7.6 *Equation 5.3*

*** $p < .001$; ** $p < .01$; * $p < .05$; . $p < .1$

8. Limitations

In this chapter we discuss the limitations of our study by critically assessing our data sample and other relevant factors that may influence the reliability of our findings.

8.1 Small Data Sample of Events

Our data sample of events contains observations of only 10 events across the three countries studied. Although it is sufficient to estimate a general and individual effect of events, it would still be preferable to have a larger event sample. A majority of our events have overlapping estimation and events window. With more events included, overlapping events would potentially make up a smaller part of the sample, making the event regressions more accurate. The small sample of events imposes a bigger limitation on the NPI regression. The sample size greatly affects the explanatory power of this regression model, as the number of possible predictors is relatively large. Because of our small sample size, we are forced to exclude some NPI groups from the regression. Other NPI groups have to be merged into broader categories in order to reduce the number of predictors. Consequently, the NPIs active in each event ends up being quite similar. If we had more events included in our sample, these adjustments would not have been necessary. With enough events we could also include even finer categories of NPIs. These are all factors that would likely improve the reliability and accuracy of our results significantly.

We originally planned to include Finland in our study, which would lead to an increase in observed events in the sample. However, the relevant and necessary data on Finnish labor market statistics are not available to us. The possible increase in number of events obtained by including Finland in our study would still have been limited. We could increase our sample size by including additional European countries as well. While it would give us the benefit of more events, it would also reduce the advantage of comparable countries and similar trends. This could potentially cause inconsistency in our results. As we believe the event regressions to yield relatively credible results, we find our 10-event sample to be sufficient for this purpose. If the effects of different NPIs would have been the sole focus of our study, we would likely prefer to increase our sample size.

8.2 Estimations of Unavailable Data

As discussed in chapter 4.1.1, the Norwegian statistics on unemployment and furlough spells consists of several weeks where data is missing. We have collected our Norwegian labor market data through excel sheets publicly available at nav.no. We do know that NAV has registered the data for the weeks where this is not published online, as other research articles and news publications use it. However, NAV does not assist master students with questions or data collection. This data has therefore been unavailable to us, even though it exists. Nevertheless, we also know that the registration method and definitions of furlough spells was subject to changes at the start of the pandemic (Ruff, C., personal communication, 8th November 2021). Even if we would have been able to obtain the data that has been unavailable to us, numbers from early stages may not have been comparable to those of later stages. We have used two different estimation methods to calculate values in weeks where data was missing. Further, we have run robustness tests to see whether the estimation methods affect the results. They turned out to be quite robust independent of method. However, we do not know how our estimates compare to the real numbers in weeks where the data is missing. It is therefore possible that changes in unemployment and furlough spells are different in real life. If this is the case, our estimated effects could to some degree be incorrect.

8.3 Unreliable Data

Another limitation of our study is the reliability of the data we do have. In particular, we suspect that the number of furloughed workers in Norway are underreported. Many students support themselves though part-time jobs. As a full-time student you do not have the right to claim unemployment benefits or compensation of lost income due to being furloughed (Norwegian Confederation of Trade Unions, 2021). For furloughs to be registered and to claim benefits, one has to register as a job seeker with NAV. As students does not have the right to these claims anyways, we find it reasonable to assume that many students that were furloughed from part-time jobs did not register this. Another reasonable assumption is that students with part-time jobs often work in visitation-based businesses, such as restaurants, stores, gyms and so on. As discussed in chapter 1.1 and 2.3, these are businesses commonly affected by NPIs. Thus, the real number of furloughed workers may be higher than the registered numbers and our findings may be biased as a result of this.

8.4 All Groups Received Treatment

Initially, we intended to use Sweden as a control group, while Norway and Denmark would serve as treatment groups. We assumed that the NPI strategies of the countries were significantly different throughout the pandemic. As the countries are otherwise comparable on many levels, we wanted to do a pure difference-in-difference analysis. We would then compare Sweden to Norway and Denmark, pre- and post-treatment. Our aim was to investigate whether a strict strategy had a greater impact on labor markets, than a less strict strategy. However, through the work with the thesis and by observing the collected data carefully, we realized that our initial assumption was not completely accurate. We learned that also Sweden at several times received treatment, and that the role of treatment and control groups generally switched around during the period of our study. Additionally, there was multiple treatments of the different countries during this period, making a difference-in-difference analysis even more complex. Thus, we recognized that a pure difference-in-difference analysis would be too complex of a task to undertake in the amount of time available to us. However, a pre- versus post-treatment comparison still provides some very interesting results on the effects of NPIs on labor markets of the three countries.

9. Conclusion

The aim of our thesis is to investigate the longer-term effects of NPIs on the labor markets of the Scandinavian countries. Our objective is to contribute with valuable insight to the body of literature on Covid-19, with a focus on the effects on labor markets. We extend the analyses from existing literature in the field by examining several events and their effects on unemployment and furlough spells. Much of the literature discussed in chapter 3.0 study the effect of a single event and a single country. Our results, however, provide quantitative insight on the effects of several events across countries, in a longer perspective. The existing literature on Covid-19 also consists of research on the epidemiological effect of NPIs. Combined with this knowledge, our findings can serve as decision support for policymakers when passing measures to contain the virus while minimizing negative economic effects.

In the following sections, an event is considered as NPI implementations or increases in their strength. Our first hypothesis concerns the general effect of events on unemployment and furlough spells. To examine this, we perform an OLS regression to estimate the average effect on unemployment and furlough spells in the weeks prior to, during, and after an event. The results indicate a slight increase in unemployment per 100 000 inhabitants during the weeks studied. The estimated average number of unemployed in the week prior to the event is statistically significant with a p-value of less than 0.001. However, there is only a small significant effect in the weeks prior to an event, and no significant effect in the weeks during and after an event.

On the other hand, the estimated effects on furlough spells per 100 000 inhabitants are significant with a p-value of less than 0.001 for all weeks studied. There is a small decrease in furlough spells in the weeks prior to an event and a relatively large increase in the weeks after an event. From four weeks prior to an event until four weeks after an event, the results show a total increase in furlough spells that are more than five times larger than that of unemployment. This indicates that events have a statistically significant increasing effect on furlough spells, but not on unemployment. Thus, the part of our first hypothesis regarding an increase in furlough spells is supported by the analysis. The part that concerns unemployment is not supported by the results. Based on the results, we conclude that for the period studied, companies choose to furlough their workers instead of laying them off, when possible.

For the remainder of our analyses, we examine the effect on furlough spells, as we conclude that there is no significant effect of events on unemployment. To further investigate our first hypothesis, we fit a new linear model through OLS regression to estimate the effect of each individual event. This second regression estimates Equation 5.2 and allows us to examine whether all of the events contribute to an increase in furlough spells. We apply a pre- versus post-treatment comparison to evaluate how each event affects furlough spells, relative to the average of the prior weeks. All 10 events in our sample are statistically significant with a p-value less than 0.001, except for the event in week 50 in 2020 in Denmark, which has a p-value of 0.004.

However, the impact of each event varies. For instance, the negative effect of the event in week 46 in 2020 in Denmark indicates a decrease in furlough spells per 100 000 in the weeks following the event. For the event in week 6 in 2020 in Sweden the effect is also negative, and results in 0 estimated furlough spells per 100 000 inhabitants. Yet, this is explained by the fact that there was no furlough program at the time. Thus, the event did not result in any furlough spells either. For the event in week 12 in 2020 in Norway though, the average relative change equals 4190.81. This results in 4757.61 estimated total furlough spells per 100 000 inhabitants after the event. In short, all events but one results in a statistically significant increase in the number of furlough spells when furlough programs were available. However, the effects are of varying size. This supports the part of our first hypothesis that concerns the effect of NPIs on furlough spells.

Our second hypothesis address the effect of different NPI groups. To examine this, we use the results obtained by the second regression for further analysis. We use the estimated effect of events as the dependent variable in Equation 5.3 and estimate the effect of NPI groups. The event sample consists of observations of only 10 events and a large number of possible predictors. Thus, we have to group the NPIs into broader categories and exclude some in order to preserve explanatory power. Although the accuracy of the results may be somewhat uncertain because of this, we do believe that they give good indications of the effects. The results suggests that the different NPI groups has varying effects, and that not all NPIs contributes to an increased effect. Workplace closure and restrictions on international travel, as well as testing, contact tracing and face covering policies contributes to the largest increases in estimated effects of events. Stay-at-home requirements contributes with a slightly smaller increase. On the other hand, school closures together with cancellations of and restrictions on public events contributes to a decrease in the estimated effect of events. The model does provide some accuracy when predicting the effect of the 10 events. However, it is characterized by uncertainties, due to large standard errors and estimates used as the dependent variable. The findings support our hypothesis, but not in the way we initially assumed for the different NPI groups.

The results from regression Equation 5.2 and Equation 5.3 also allow us to examine our third hypothesis. This assumes that events during the “first wave” of the pandemic had a larger effect than those in later waves. The spring of 2020 is commonly referred to as the “first wave” in our three countries, as described in chapter 2.2. As we know the date of all the events in our sample, we can identify those that occurred in this period. Hence, we can compare the effect of the events occurring in the “first wave” to the effects of those happening in later waves. Each of the three countries has one event during the “first wave”. The effect of these three events is notably larger than the effect of the events occurring in later periods, for all three countries. This result supports our third hypothesis.

To further investigate this hypothesis, we include a “first wave” indicator to the sample of events. Then we use this to estimate the difference between the effect of a “first wave” event relative to events in later waves. The result of this regression indicates that events occurring in the “first wave” contributes with a large increase in estimated effects, relative to the events in later waves. Based on our findings, we conclude that events in the early stages of the pandemic resulted in larger increases in furlough spells than those occurring in later stages. This can be explained by several reasonable assumptions, such as “NPI fatigue”, more targeted measures, or smaller effects of NPI groups introduced later if other groups are already active.

The objective of this thesis is to answer the following research questions: *How does the implementations of NPIs affect the labor markets of the Scandinavian countries in the longer run?* We have analyzed several events across the three Scandinavian countries. By doing so, we have estimated the general and individual effect of implementations of NPIs or increases in their strength on the countries’ labor markets. Our results indicate that such events have a significant, increasing effect on furlough spells, but not necessarily on unemployment. This indicates that the introduction and availability of short-time job retention programs also works as intended. Further, our results suggests that implementations of NPIs or increases in their strength happening in the early stages of the pandemic contributes with a larger effect than those in later stages. Our thesis contributes to the literature with quantitative insight on the longer-term effects of NPIs on labor markets in a broader perspective than what has previously been studied. This may serve as insight for policy makers when passing containment measures that also aims to minimize the negative effects on the economy.

Further Research

The pandemic is still ongoing, and how Covid-19 affects labor markets will thus continue to be a relevant research topic for researchers in the foreseeable future. This is especially true with the discovery of the Omicron variant. As a consequence, strict measures are currently being implemented in several countries worldwide once again. It is not unlikely that this will affect labor markets in months to come. This section elaborates on suggestions for further research on the effect of NPIs on labor markets.

Our study uses the total weekly number of furloughed or unemployed people per 100 000 inhabitants in each region as the dependent variable. This has been the only data available to us. When using the total number of unemployed or furloughed people, we can observe changes in this number over time. However, we also run the risk of not capturing all of the effects. If, for example, 1000 people were put on furlough in a given week, while 1000 people were sent back to work, the total number of furloughed people will not change. Thus, if the 1000 people was put on furlough or sent back to work because of NPI implementations or measures being lifted, our analyses would not capture this effect.

The total number of unemployment or furlough spells does give good indications of larger movements in the labor market in weeks surrounding events. However, it could be more accurate to look at weekly inflow to and outflow from unemployment and furlough spells instead. This is of particular relevance when looking at longer-term effects. It is reasonable to assume that there are more outflows in later stages of the pandemic than in early stages of the pandemic. Our results, suggesting larger increases in events in the "first wave", supports this assumption as well. There could also be increased outflows following the reopening of societies and the lifting of measures, or increased inflows caused by NPIs once again being implemented due to Omicron. *With new measures being implemented, future studies have the possibility of analyzing even more events.*

Also, we have the challenge of missing data for several weeks in Norway. Having access to data on unemployment and furlough spells with reliable observations in all weeks studied, would improve the accuracy of the results. As time goes by, the amount of available data increases, allowing for further research of greater accuracy. It would be interesting to investigate the effect of Covid-19 on labor markets by analyzing a greater amount of reliable data. As Omicron leads to implementations of measures once again, future studies have the possibility of analyzing even more events. If different countries register their labor market data in a similar fashion and make this available to researchers, one could include more events in future studies. A larger sample of events would improve the estimation of effects of both events and NPI groups. Including data on NPIs on a regional level could lead to an even more accurate estimation of the effects of different NPIs and the effect of “first wave” events.

Finally, we would like to stress the importance of viewing the results in a larger context. Naturally, insights on the effects of NPIs on labor markets are important for policy makers and authorities. It can serve as decision support in order to pass measures that have minimal negative effects on the labor market. Yet, another important concern in to pass measures that contains the spread of the virus and maintains adequate health care capacities. There is likely a trade-off here, and as the duration of the pandemic increases it becomes even more important to balance this. We therefore find it important for further research to examine how different NPIs affect the economy and public health simultaneously. It is necessary to study the effect of NPIs on both of these areas, in order to pass measures that contain the pandemic and at the same time minimize the negative effects on the economy.

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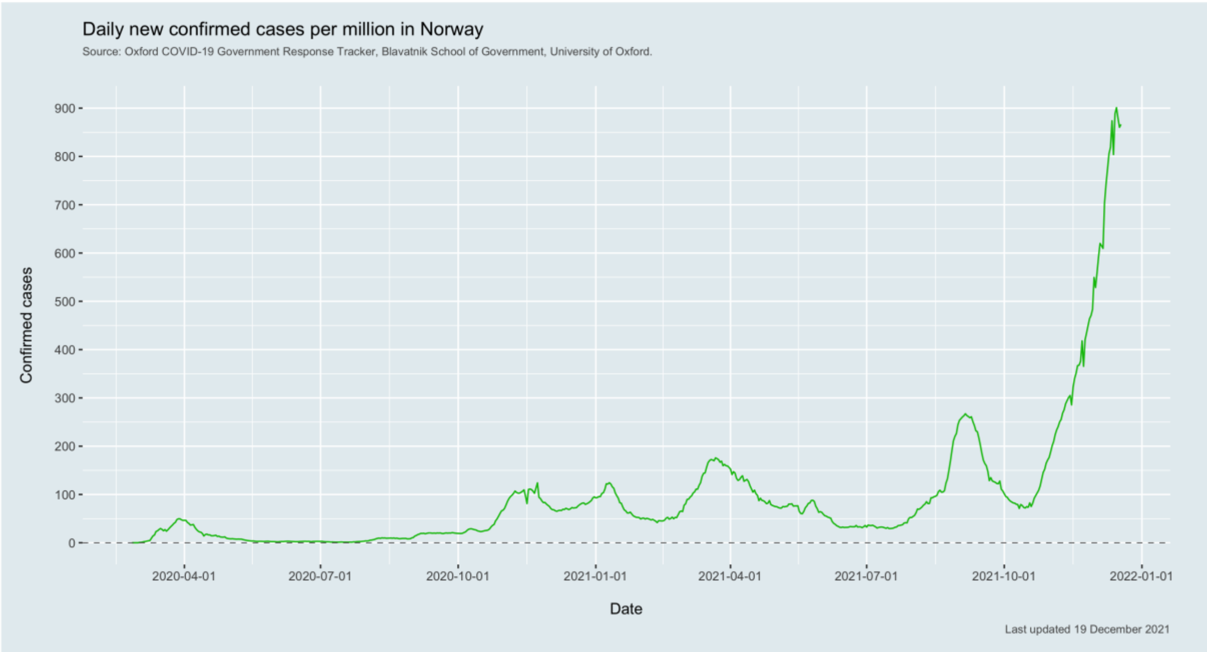
Appendix

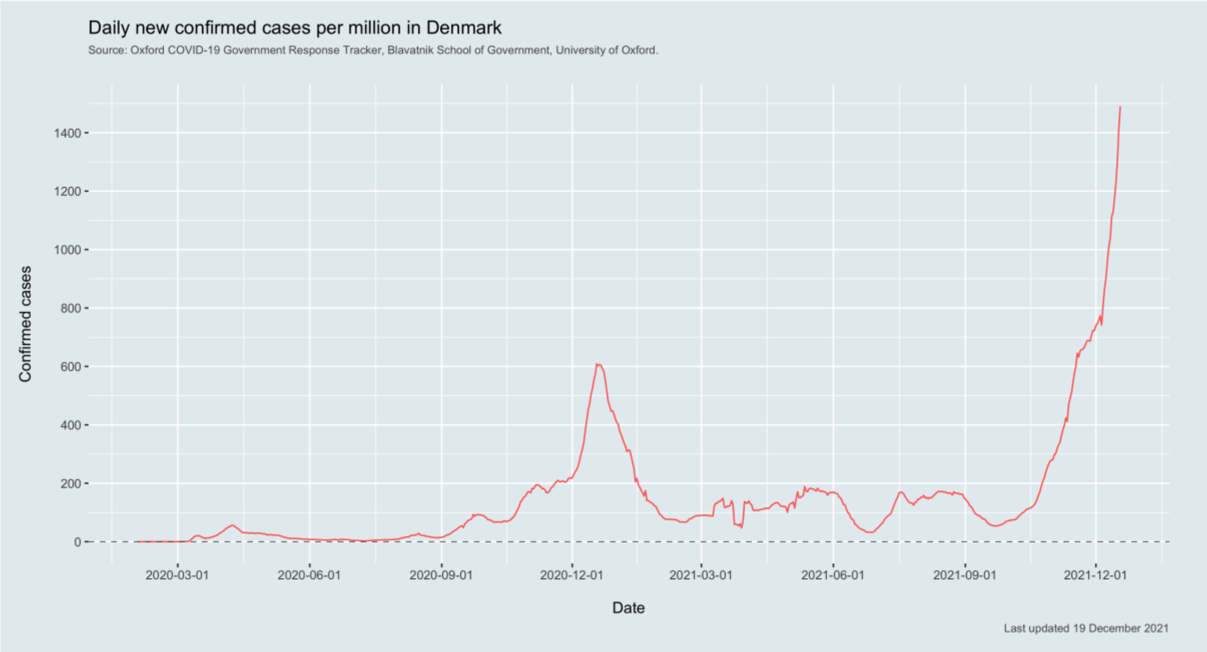
A1 Visualizations of Daily New Confirmed Covid-19 Cases

Visualizations of daily new confirmed cases in the three Scandinavian countries, based on data from *OxCGRT* (Hale et al., 2021).

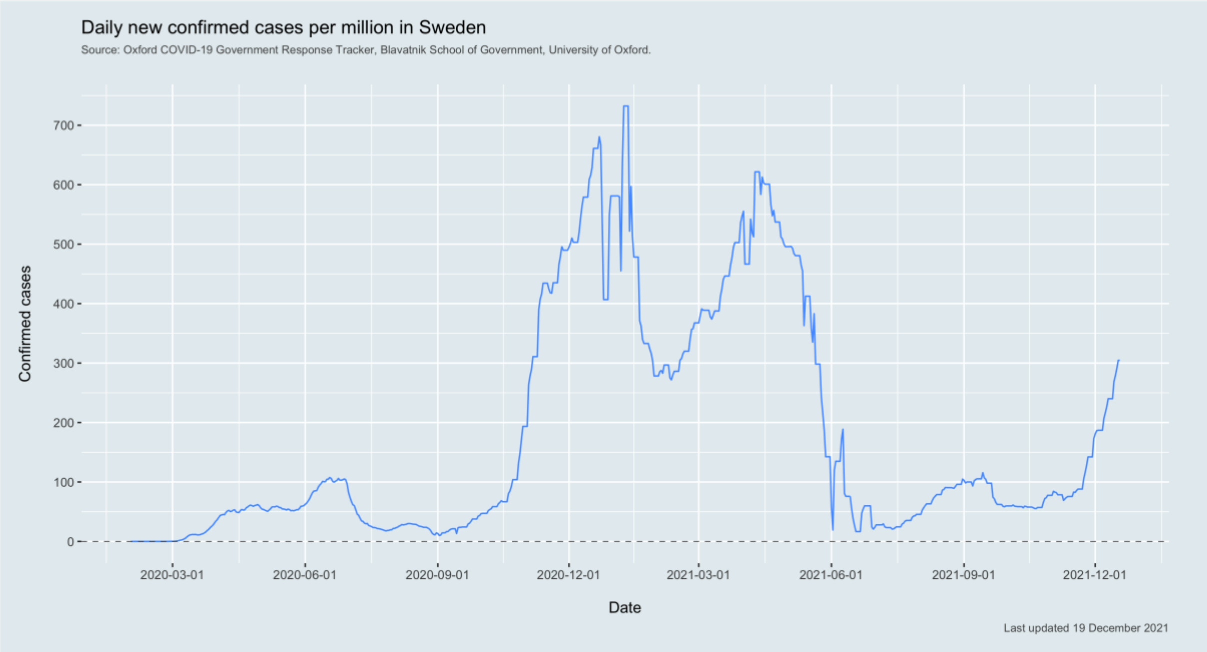
Norway



Denmark


Daily new confirmed cases per million in Denmark
Source: Oxford COVID-19 Government Response Tracker, Blavatnik School of Government, University of Oxford.
Confirmed cases
1400
1200
1000
800
600
400
200
0
2020-03-01
2020-06-01
2020-09-01
2020-12-01
2021-03-01
2021-06-01
2021-09-01
2021-12-01
Date
Last updated 19 December 2021

Sweden


Daily new confirmed cases per million in Sweden
Source: Oxford COVID-19 Government Response Tracker, Blavatnik School of Government, University of Oxford.
Confirmed cases
700
600
500
400
300
200
100
0
2020-03-01
2020-06-01
2020-09-01
2020-12-01
2021-03-01
2021-06-01
2021-09-01
2021-12-01
Date
Last updated 19 December 2021

A2 Overview of Missing Weeks in the Norwegian Data

Overview of missing weeks in the Norwegian data

End of month values used		*Estimated values used*	
Year	**Week**	**Year**	**Week**
2020	13	2020	14
2020	17	2020	15
2020	22	2020	16
2020	30	2020	18
2020	35	2020	19
2020	48	2020	20
2020	53	2020	27
2021	4	2020	28
2021	8	2020	29
2021	13	2020	31
2021	17	2020	32
2021	26	2020	33
2021	30	2020	34
2021	35	2020	40
2021	39	2020	44
2021	43	2020	45
		2020	51
		2020	52
		2021	1
		2021	12
		2021	14
		2021	18
		2021	21
		2021	27
		2021	28
		2021	29
		2021	31
		2021	32
		2021	42

***Note**: The missing weeks are almost the exact same weeks for both unemployment and furlough data. However, there are some weeks that are different for furlough spells: we used end-of-month value for furlough spells in week 21 (2021) instead of estimated values as we did for unemployment. In addition, week 18 was not missing in the furlough spells data, and thus we did not need to estimate it like we did for unemployment.*

A3 Indicators of Government Policies

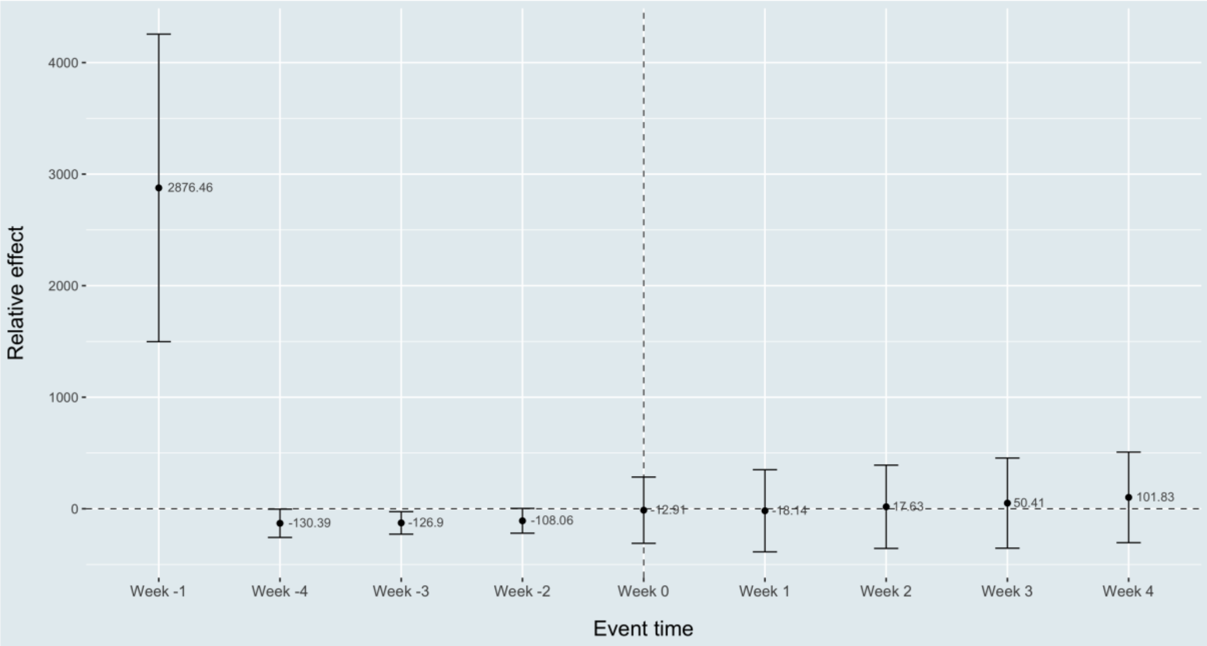
Indicators of government policies (NPIs)

Indicator	Levels of strength
School closures	No measures, recommended, required at some levels, required at all levels
Workplace closures	No measures, recommended, required at some levels, required at all levels but essential workplaces
Cancellation of public events	No measures, recommended, required
Restrictions on public gatherings	No measures, >1000 people, 100-1000 people, 10-100 people, <10 people
Stay-at-home requirements	No measures, recommended, required except for daily 'essential' trips, requires with minimal exceptions
Face covering policies	No policy, recommended, required in specified public spaces, required in all public spaces, required outside the home at all times.
Public transport closures	No measures, recommended, required
Restrictions on internal movement	No measures, recommend movement restrictions, restrict movement
International travel controls	No measures, screening, quarantine from high-risk regions, ban on high-risk regions, total border closure
Testing policies	No policy, only those who both have symptoms **and** meet specific criteria, testing of anyone with symptoms, open public testing
Contact tracing	No contact tracing, limited - not for all cases, comprehensive - for all cases
Public information campaigns	No COVID-19 public information campaign, public officials urging caution about COVID-19, coordinated campaigns
Stringency Index	0-100
Containment Index	0-100

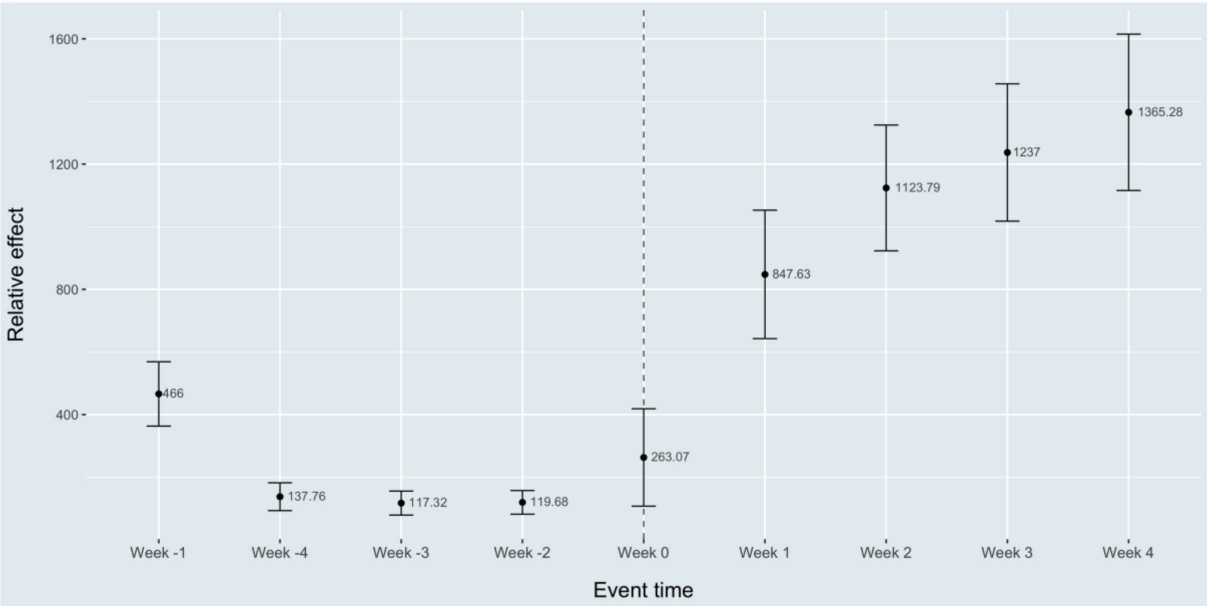
Source: (Hale et al., 2021).

A4 Figures Illustrating Relative Effects

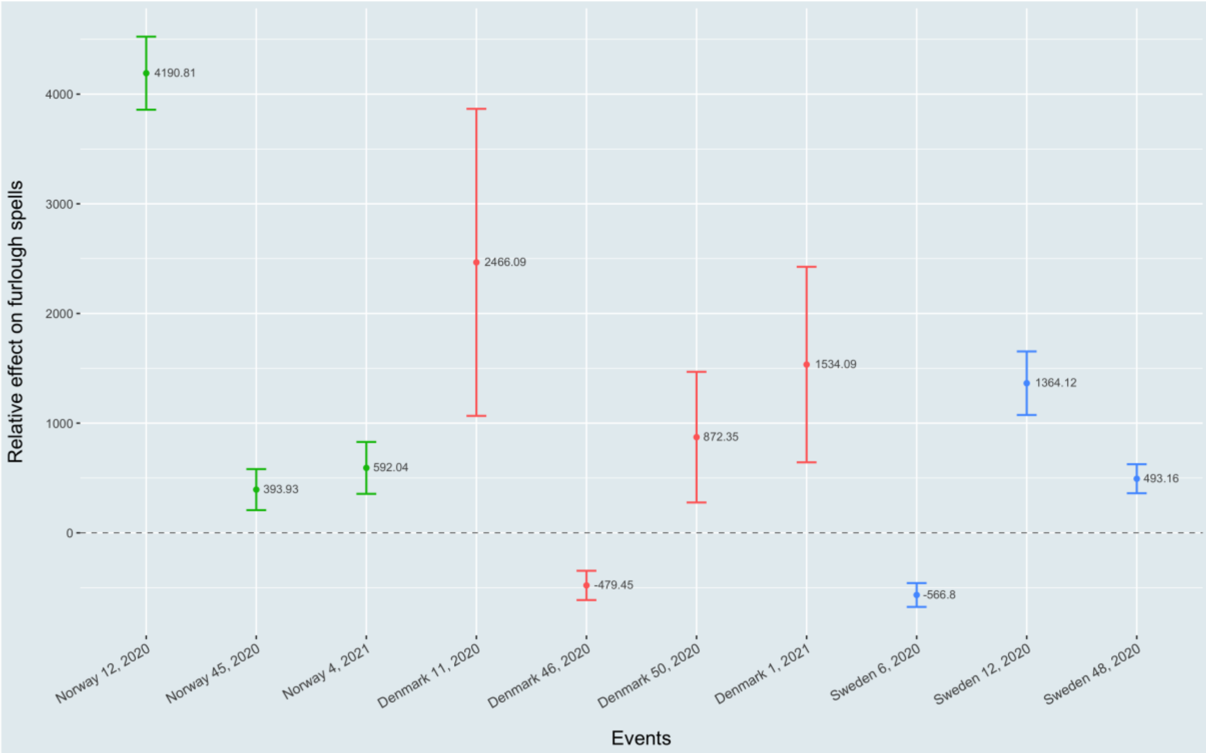
Average change in unemployment, relative to the week prior to an event:



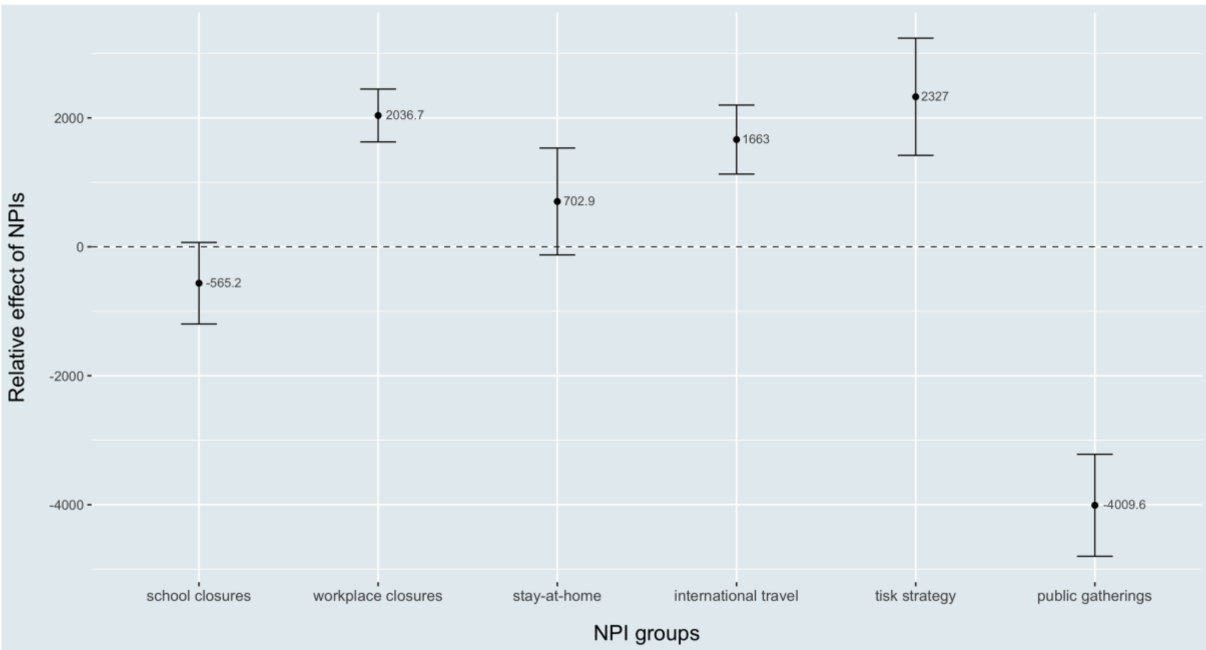
Average change in furlough spells, relative to the week prior to an event:



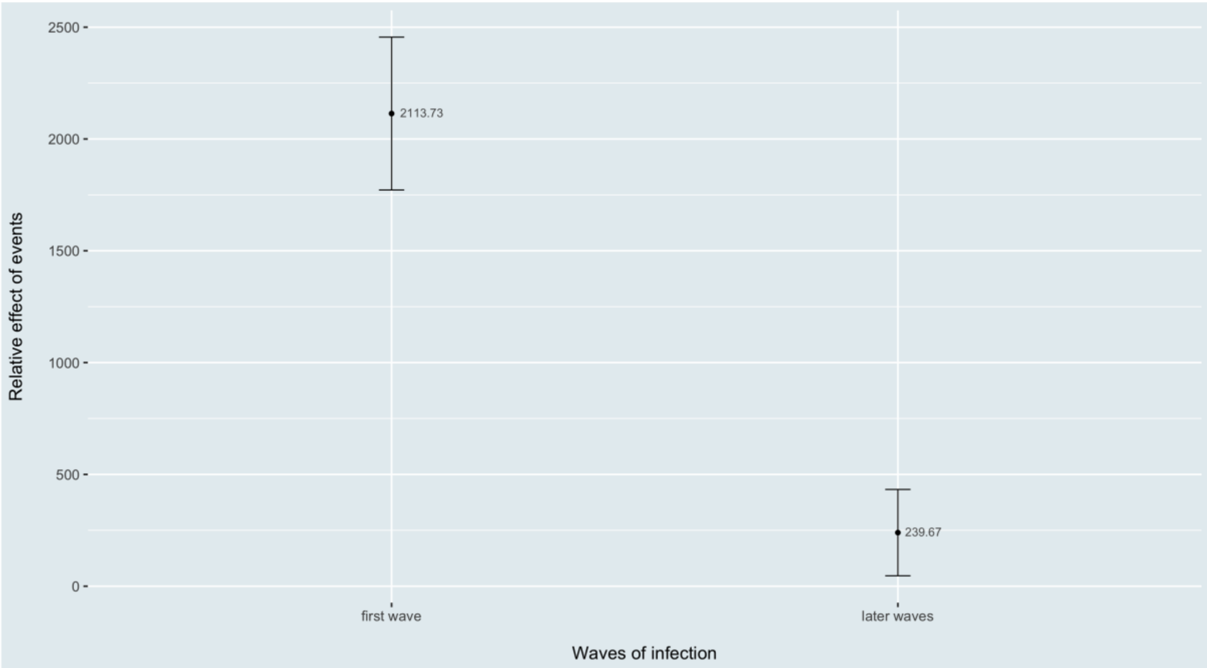
Average change in effect on furlough spells, relative to the week pre-treatment:



Average change in effect of events for NPI groups, relative all NPIs being zero:



Average change in effect of events in "first wave", relative to events in later waves:



A5 Predictions of effect of events based on estimated effects of NPI

Table A5

Predictions of effect of events based on estimated effects of NPI

Coefficients	Estimate	Prediction	Residual
Intercept	566.80		
nor_12_20	4190.81	3234.70	956,11
nor_45_20	393.93	-879.50	1273,43
nor_4_21	592.04	592.00	0,04
dnk_11_20	2466.09	2466.10	-0,01
dnk_46_20	-479.45	218.30	-697,75
dnk_50_20	872.35	2255.00	-1382,65
dnk_1_21	1534.09	2255.00	-720,91
swe_6_20	-566.80	100.20	-667,00
swe_12_20	1364.12	1198.00	166,12
swe_48_20	493.16	493.20	-0,04

A6 Mathematical Formulation of Estimation Type 2

Equations for estimate type 2:

Weekly average change = x = $\frac{\text{value of last week with certain data – value of first week with certain data}}{number\ of\ weeks\ between\ certain\ data\ points}$

Number of furloughed or unemployed in first week with certain data $= t_0$

Missing week number 1$= t_1 = t_0 + x$

Missing week number 2 $= t_2 = t_1 + x$

Missing week number n $= t_0 = t_{n-1} + x$