# ERASMUS UNIVERSITY ROTTERDAM

Erasmus School of Economics


Bachelor Thesis (FEB13200)

*Where governments fail, crime rates flourish:*

A theft analysis during the post-pandemic cost-of-living-crisis


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Date final version: Thursday 13 June 2024

**Abstract**

The cost-of-living crisis in The Netherlands that arose after the Covid-19 pandemic has hampered the poorest in society in looking after themselves. Minimum wages stagnated and fell behind yearly inflation. Some resorted to petty crime in response, with Dutch supermarkets in particular experiencing unparallelled levels of shoplifting activity.

This paper focuses on the forecasting value of minimum wage levels, in conjunction with consumer price indices, on the future number of monthly shoplifting incidents throughout The Netherlands. Using the models in this paper, companies and governments can more accurately predict which months will experience the most shoplifting activity, and in response they can allocate their means and security services accordingly.

Using monthly Dutch minimum wage, CPI and shoplifting data between 2012 and 2024, I set up an *autoregressive distributed lag* (ARDL) model with year and month dummies to account for seasonality and idiosyncrasies. I find that, on average, an increase of real minimum wages with €1 in one month leads to a 13.3% reduction in shoplifting incidents in the next month. This is, however, merely a forecasting effect; this paper does not establish any causal links.

*Keywords:* Crime, minimum wage, inflation, shoplifting, forecasting

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## I. Introduction

"It's the economy, stupid!" When former president Bill Clinton coined this phrase in August of 1992, the United States had just fallen into a recession after the 1991 Persian Gulf crisis under his predecessor, president George H.W. Bush. Clinton and his campaign team were convinced that a path toward economic growth would be the single most important factor in deciding the upcoming presidential election – and they were right (Levy, 2009).

It appears this expression has withstood the test of time since it has remained all too relevant in the 21st century. Especially since the end of the Covid-19 epidemic and during the first two years after the Russian invasion of Ukraine, economic instability has once more taken centre stage in daily news coverage and the minds of the voting populace throughout the Western world. Due to sharp inflation increases, for instance, during the last Dutch parliamentary elections in November 2023 the cost-of-living crisis proved to be the decisive matter at hand for the landslide victory of the far-right and anti-establishment Freedom Party (PVV) of Geert Wilders (Ipsos, 2023).

In late 2022, the Dutch government announced an unprecedented increase in the national minimum wage to mitigate the harshest effects of the country's crippling inflation rates that same year – averaging around 10% when measured year-on-year (Centraal Bureau voor de Statistiek, 2023). Thereto, the national monthly minimum wage would be increasing from €1,756 to €1,934 as of January 2023, equal to an adjustment of 10.15% (Ministerie SZW, 2022).

In The Netherlands, the national government sets new minimum wage rates on a biannual basis, namely per 1 January and 1 July each year, which are based on a monthly salary earned in a standard 40-hour workweek.[1] This minimum wage applies to every Dutch employee who is at least 21 years of age. Moreover, the minimum wage level is also linked to national social welfare schemes, such as the social assistance benefit (in Dutch referred to as the *bijstandsuitkering*), state pensions (*AOW*), and unemployment and illness benefits (*WW-uitkering* and *ZW-uitkering*, respectively). Therefore, a percentage increase in minimum wages also results in the same percentage increase in nearly all – and including the most important and widespread – social benefit schemes (Ministerie SZW, 2024).

One aspect relating to minimum wage increases that is often omitted from national news coverage, at least in The Netherlands, is its possible link to crime rates. It stands to reason, for instance, that an increased minimum wage generates more income for the nation's poorest, therefore reducing the need for petty crime such as – or especially – supermarket shoplifting. However, as of 2024, no noteworthy research into the relationship between minimum wage increases and shoplifting incidents in The Netherlands has been conducted.

[1] As imposed by articles 8 and 14 of the Dutch Law on minimum wage and minimum holiday allowance.

This paper, therefore, seeks to investigate whether such a link between the biannual minimum wage increases in The Netherlands and shoplifting incidents exists, and to what extent a minimum wage increase can accurately forecast the number of crime rates in the country. Thus, the main research question reads as follows:

*“In what way does a national minimum wage increase forecast future shoplifting rates?”*

All in all, this research fits neatly into the social debate regarding the most efficient methods to combat and understand petty crime – not only in The Netherlands, but also in other countries that experience an uptick in crime rates and a cost-of-living crisis. Establishing that a minimum wage increase creates a downward expectation for shoplifting rates may, for instance, help Dutch entrepreneurs in understanding why they have been on the receiving end of increased shoplifting rates (NOS, 2024), and direct government attention toward properly addressing that a minimum wage increase need not necessarily solely inflate wage costs for employers, but could just as well save them the troubles associated with shoplifting in general.

Moreover, since the minimum wage increase as mentioned above merely pertains to adults over the age of 21, meaning that adolescent employees seldom benefit from wage increases because the youth minimum wage system does not employ the government to re-examine wages biannually, the rising minimum wage gap between adults and minors may explain the recent uptick in the relative number of shoplifting incidents attributed to teenagers (NJi, 2024). If it were to be found that minimum wage increases do, in fact, decrease shoplifting rates, then there would be a solid case for simultaneously lifting the youth minimum wage as well – assuming similar theft effects and forecasts, and notwithstanding any negative labour demand and social welfare expenditure effects associated with increased minimum wages.

In answering this question, this research uses monthly measurements of the number of shoplifting incidents in 342 Dutch municipalities between January 2012 and April 2024, including the 25 biannual minimum wage and social benefit increases that occurred during that same period. The data is then combined into national shoplifting rate figures. This information is thereafter analysed using an *autoregressive distributed lag* (ARDL) time series model with year and month dummies.

Using this estimation approach, it is established that nominal minimum wage increases in one month, using real minimum wages as a proxy, forecast a 13.3% reduction in shoplifting rates for every €1 increase. Additional tests then confirm that real minimum wages are of significant and useful forecasting power when estimating future shoplifting rates in The Netherlands.

This paper, therefore, provides new insight that the positive minimum wage effects associated with wage increases outweigh any possible negative unemployment effects. However, this research merely focuses on forecasting ability; it does not claim any form of causality.

## II. Related literature

This paper is a strong complement to existing eminent literature on the relationship between minimum wages and crime rates. Most analyses related to this subject follow three general empirical strategies, albeit with minor deviations from their standard formats. These methodologies will be discussed sequentially using concrete and pertinent literature.

Incipiently, Braun (2019) focuses on the effect of a minimum wage increase on the average individual crime rate and willingness to commit crimes. Thereto, she has designed a theoretical model that outlines an individual's *utility functions* as being partly dependent on employment and productivity costs of employment, as well as committing crimes and the probability of being sent to prison because of those illegal activities. Then, utility maximisation implies marginal utility equating the marginal benefit of employment, i.e., net wages. The individual utility functions are thereafter combined to form aggregate relationships between minimum wages and tendencies to commit crime.

Using individual panel data from the US National Longitudinal Survey of Youth between 1997 and 2010, Braun (2019) finds that the effect of an increase in the minimum wage on the average individual crime rate is U-shaped. This implies that, when the minimum wage is initially low, any increase will have a substantially negative effect on the average crime rates. Then, when the minimum wage increases from an originally average starting point, the marginal negative effect on crime rates is greatly diminished. Finally, it appears that, when the initial wage was already above par, a minimum wage increase has a positive marginal effect on the average individual crime rate. This occurs because, thenceforth, the negative employment effects of a minimum wage increase surpass the positive wage effects.

Instead, three other papers opt for *linear regression* formats and individual panel data. For instance, Beauchamp and Chan (2014) investigate whether crime rates respond to changes, either positive or negative, in the minimum wage level. Their research focuses on youth crime incidents after a rise in the US federal minimum wage, as well as some individual states' wage policies. Beauchamp and Chan reason that these effects could be double-sided since minimum wage rises hamper employment opportunities and therefore causes employees to substitute into committing crimes as compensation for joblessness. On the other hand, higher wages raise crime opportunity costs for the employed.

They approach the issue using a linear probability ordinary least squares (OLS) model with common vector controls to estimate the effects of a minimum wage increase on the probability of committing a crime, and a logit model to allow for non-linear probability increases to see whether the outcomes would differ significantly. Using the same dataset as Braun (2019), the researchers find that those working at an hourly wage that is at most 36 cents above the binding minimum wage become 1.9 percentage points more likely to commit crimes when the minimum wage is raised, with a base crime rate of 12.1%, thus a relatively large increase. Thefts increase with around 0.2 percentage points. Moreover, it is found that employment effects far outweigh the wage effects of an increased minimum wage. In sum, only employees who are directly bound by the minimum wage increase are influenced in their decisions to commit crimes, whereas employees with a larger-than-minimum wage remain unaffected.

Subsequently, Agan and Makowsky (2023) apply a multiple linear regression containing various individual vector controls. This strategy measures the effect of a US state minimum wage increase and individual income tax breaks on whether an individual was reincarcerated within three years after their initial release from prison. They use individual data from around 6 million released prisoners in the US between 2000 and 2014, pertaining to income, employment, and personal demographics.

Thereto, the researchers find that a minimum wage increase of 50 cents reduces the probability of property crime recidivism by 2.2 percentage points. This statistic especially benefits prisoners from African American descent. In this case, it seems the positive wage effects associated with minimum wage increases outweigh the negative employment effects. A similar effect is measured when the government implements income relief legislation, such as tax breaks and deductions, albeit that only women are significantly affected in their willingness to commit crime because of those fiscal policies. These results once more highlight the opportunity cost effects of fiscal and minimum wage policy on criminal activity.

In addition, Hansen and Machin (2002) investigate the effect of the introduction of a national minimum wage in the UK in 1999 on local crime rates. They developed a log-linear OLS regression that estimates the percentage change in the proportion of workers being paid less than the minimum wage that committed crimes because of the introduction of the minimum wage, and another log-linear OLS regression estimating the percentage change in employment rates because its enactment.

Using local panel data on monthly crime rates in England and Wales between 1998 and 2000, they conclude that the introduction of a minimum wage in a previously fully liberalised labour market – such as in the UK – on average reduces crime throughout all neighbourhoods regardless of economic and demographic backgrounds, yet that the decrease in crime rates is skewedly beneficial for areas with low household income. The authors find that a minimum wage is, therefore, an effective remedy in combatting crime in poorer neighbourhoods.

Moving on, both Gould *et al.* (2002) and Fone *et al.* (2023) use a *US county fixed effects* approach. The first paper intends to analyse whether wage or unemployment effects are the dominant economic drivers caused by a minimum wage increase, whereas the second one investigates minimum wage effects on crime reports, crime elasticities, and unemployment effects among adolescents aged between 16 and 24.

Gould *et al.* (2002) gathered panel data for 705 US counties with yearly observations between 1979 and 1997 on numerous aggregate demographic and socioeconomic variables. Their analysis contains a fixed effects approach and is extended to include *instrumental variables* for initial county industrial composition and demographic idiosyncrasies to eliminate any remaining endogeneity. The researchers find that both wages and employment have a causal link with crime rates, but that the positive wage effects have consistently played a larger role than negative unemployment effects. These results are sufficiently robust to alterations in the sample ethnic and socioeconomic composition.

Ultimately, Fone *et al.* (2023) use panel data from 1997 to 2016 on US federal, state, and local crime reports sorted by the offender's age at the time of the crime. They apply a two-way OLS fixed effects strategy with demographic and economic county vectors as controls, measuring the effect of a percentage increase in the county minimum wage on the county number of arrests of adolescents per 1000 inhabitants. This method is used to test the parallel test assumption. Then, they use a *difference-in-difference* model to estimate the exact causal effect of minimum wage increases on adolescent arrests and crime rates.

Their results indicates that property crimes committed by adolescents – the vast majority constituting larceny – show a 0.3 percentage point increase for every 1 percentage point increase in minimum wages. Thus, minimum wage policy would incentivise thefts and is, therefore, counterproductive in solving crime issues. Again, this effect occurs because unemployment effects overshadow any wage effects. Moreover, a minimum wage increase is not shown to dilute overall youth crime incidents significantly.

Considering the mechanical effects as elaborated upon by Beauchamp and Chan (2014), for the purposes of this paper one might expect that a higher minimum wage increases employee opportunity costs associated with committing crimes, such as job layoffs and penal measures. This would then create a larger crime deterrence effect, leading to fewer crime incidents and thus, as is pertinent to this paper, lower shoplifting rates. On the other hand, as per the research of Fone *et al.* (2023), higher minimum wages lead to falling labour demand and thus increase structural unemployment. This, in turn, results in lower spendable income for the unemployed, thus lowering opportunity costs for criminal activity. This mechanism may induce higher crime rates, among which an increased number of shoplifting incidents.

In light of this, my research is intended to analyse whether, as a net sum, the positive income effects outweigh the negative unemployment effects of a minimum wage increase on Dutch shoplifting rates, or vice versa. Thereto, this research may provide supporting evidence for either the conclusions of Beauchamp and Chan (2014) or Gould *et al.* (2002) regarding which of the effects is more dominant, at least when used as a forecasting power.

## III. Data

For the purposes of this paper, I have created a custom dataset containing information on three socioeconomic variables that was retrieved from two key institutions. The custom dataset pertains to the main data that will be used during the regression further on. Below, I will discuss its origins and structure, as well as their core definitions.

First and foremost, the data on the variable of interest for this research, namely the national average number of reported shoplifting incidents, has been retrieved from documentation gathered by the Dutch National Police Authority (NP, in Dutch referred to as the *Nationale Politie*). Since its establishment in 2012, the NP has been monitoring monthly crime incidents for all crimes that are punishable according to the Dutch Penal Lawbook and various other, more specific penal laws, irrespective of their gravity.[2]

In its various measurements, the NP follows consistent guidelines on the definition of certain crimes. As is central to this paper and its research question, the act of *shoplifting* is defined as "any theft – as mentioned in article 310 of the Dutch Penal Lawbook – of displayed goods that are intended for market sale and are property of a store or vendor, either with or without an accompanying use of violence or threats against any person, and during store opening or vending hours."[3]

The *shoplifting* variable in the dataset from the NP contains monthly measurements of the number of reported shoplifting cases per individual municipality, evaluated between January 2012 and April 2024. They include records of 342 municipalities in the European part of the Kingdom of the Netherlands, i.e., excluding the Caribbean parts of the country. The municipality subdivisions and measurements are based on the 2023 municipality borders, which have not been altered since. The municipal data was subsequently combined to obtain aggregate, national shoplifting incident figures during the same period.

As previously mentioned, each year the Dutch national minimum wage is reassessed on every 1 January and 1 July. The custom dataset used for the purposes of this research also contains the corresponding nominal minimum wage increases during January 2012 and April 2024 if applicable. It must be noted that this biannual increase affects the aforementioned national social welfare programs with the same percentage increase.

This information was derived from a panel dataset that was published by the Netherlands Central Statistics Agency (CBS, *Centraal Bureau voor de Statistiek*). As a government institute, according to Dutch law CBS has been responsible for collecting information on nearly all facets of daily life since the final years of the nineteenth century, such as year-on-year inflation, gas

[2] These specific penal laws mostly pertain to arms and drugs offences.
[3] As per paragraphs A50 and B50 of definition 2.5.2 of the *National Police Crime handbook*.

price changes, the net migration rate into a municipality, unemployment rates, and criminal activity. The measurement frequency of these factors ranges from daily to yearly analyses.

Thereto, the CBS dataset sets out the biannual changes in nominal minimum wages as a percentage change compared to the minimum wage level before the increase. One must note that this is a national measurement which, accordingly, is not bound by municipality idiosyncrasies. The records took place between January 2012 and January 2024.

Furthermore, CBS also keeps record of month-on-month inflation rates using the *consumer price index* (CPI). This variable measures the average price increases for the "standard basket" of consumer goods for the median Dutch household. It entails a weighted average of inflation rates on, for instance, groceries, rent, and petrol prices. This CPI was combined with the nominal minimum wage rates during January 2012 and April 2024 to create the *real* minimum wage. The *biannual* CPI is calculated as the real price difference between the two relevant periods of minimum wage increases, and it illustrates the difference between the nominal minimum wage increases and the real average price increases within the same period. These figures can be found in Table 3.1.

*Table 3.1: Biannual percentage changes in Dutch minimum wages and inflation, 2012-2024*

| **1 January** | **Min. wage** | **CPI** | **1 July** | **Min. wage** | **CPI** |
|---|---|---|---|---|---|
| 2012 | 0.79 | 0.82 | 2012 | 0.66 | 2.31 |
| 2013 | 0.90 | 0.60 | 2013 | 0.57 | 2.53 |
| 2014 | 0.53 | -1.06 | 2014 | 0.65 | 2.03 |
| 2015 | 0.44 | -1.95 | 2015 | 0.40 | 2.94 |
| 2016 | 0.83 | -2.38 | 2016 | 0.83 | 2.12 |
| 2017 | 0.94 | -0.33 | 2017 | 3.09 | 1.80 |
| 2018 | 0.80 | -0.34 | 2018 | 1.03 | 2.60 |
| 2019 | 1.34 | -0.16 | 2019 | 1.23 | 3.01 |
| 2020 | 1.10 | -0.97 | 2020 | 1.60 | 2.81 |
| 2021 | 0.29 | -1.12 | 2021 | 0.96 | 2.78 |
| 2022 | 1.41 | 4.64 | 2022 | 1.81 | 7.45 |
| 2023 | 10.15 | 1.82 | 2023 | 3.13 | 4.15 |
| 2024 | 3.75 | -0.07 | 2024 | 3.10* | - |

*Notes:* Table 3.1 indicates the biannual percentage (%) changes in the Dutch nominal national minimum wage and consumer price index (CPI) between January 2012 and April 2024. The increases have been rounded to two decimals. *The upcoming minimum wage increase in July 2024 has officially been determined by the Dutch government but, given the available shoplifting data, this information is unusable for this research. Furthermore, the biannual CPI until July 2024 is yet unknown since the data only extends to April 2024.

Table 3.1 shows that, on average, the minimum wage increases follow a consistent trend at a one-percent biannual raise. A great outlier is the previously addressed 10.15% increase as of 1 January 2023. This had to do with an inflationary correction caused by hiking food, rent and petrol prices in 2022 – as can be seen above – which were mainly driven by market uncertainty because of the Russian invasion of Ukraine from February of that year onwards. July 2023 and January 2024 have also seen a relatively large minimum wage increase, again as a result of the persisting above average inflation rates, albeit that inflation has been diminishing since then (Ministerie SZW, 2024), which can also be seen in Table 3.1.

The combination of both the NP and CBS datasets provides month-on-month information on the number of nationally reported shoplifting incidents, as well as information on the height of the national minimum wage, both nominal and CPI-adjusted, for those aged 21 years and older, all measured between January 2012 and April 2024. A visualisation of this data may provide a preliminary indication of any relationship between these variables.

The figure below contains a graphical analysis of the developments in the number of national shoplifting incidents between January 2012 and April 2024, displayed jointly with the developments in the real national minimum wage over that same timespan.

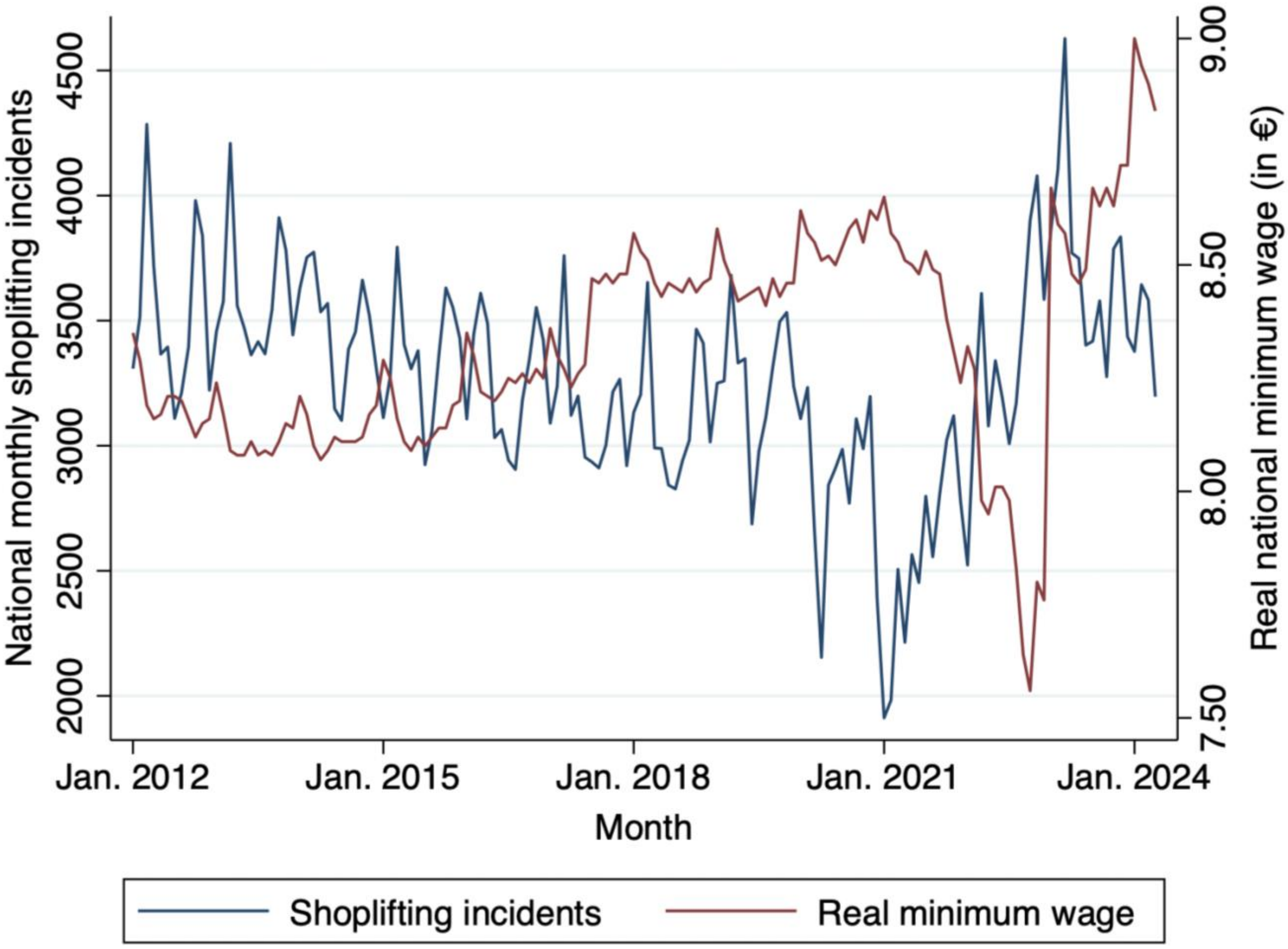


*Figure 3.2: Distribution of the number of monthly shoplifting incidents in The Netherlands and corresponding monthly real minimum wage levels, January 2012 to April 2024*

Figure 3.2 illustrates the chronological developments in the number of shoplifting incidents at the Dutch national level between January 2012 and April 2024, as well as the real minimum wage level per month. The rounded baseline minimum wage for those aged 21 years and older working 40 hours a week was nominally €8.35 on 1 January 2012. Each following minimum wage hike is the result of the percentage increases in January and July in a certain year, according to the information in Table 3.1. Again, it must be noted that this percentage increase is duplicated for the Dutch national social welfare benefit schemes as explained earlier on.

In general, as one can clearly see from the graph above, up until approximately March 2020 the national total number of reported shoplifting incidents showed a consistently downwards sloping trend – with fluctuations not exceeding around 300 incidents from the mean trend. Between early 2020 and mid-2023, this trend was abruptly altered. In plain sight, there are two plausible explanations for this dramatic fall – a decrease of around 30% within one year - in shoplifting incidents and following shoplifting hike – a 50% increase in around a nine-month period – during that timeframe.

From March 2020 until February 2022, The Netherlands, along with many more countries worldwide, had experienced the Covid-19 pandemic and the accompanying movement and economic restrictions because of national public health legislation. The sudden drop in the number of shoplifting incidents might have been caused by the closure of many stores and supermarkets during the pandemic, by virtue of which there were fewer instances for shoplifts to occur. Since these measures affected the entire country, their effects are not limited to merely a few individual municipalities.

Then, as the pandemic concluded, the country went through a cost-of-living crisis because of rapid inflation and supply shortages caused by, among other factors, the Russian invasion of Ukraine and the subsequent trade embargos of many European nations against Russia. Lower average real wages and social welfare benefits might, therefore, have nudged recipients thereof into committing petty crimes such as shoplifting as a last resort. This relationship may also be viewed in Figure 3.2 since the real minimum wage fell drastically between 2020 and 2022, whilst simultaneously the number of shoplifting incidents skyrocketed.

Another aspect that is noticeable in Figure 3.2 is the overall cyclical pattern of the number of shoplifting incidents in The Netherlands. Whereas this pattern is magnified because of the break in the y-axis between 0 and 2000 incidents, the effect might still be relevant as, for instance, the difference between a relatively high-incident month such as March 2012 and a low-incident month like August 2012, in absolute terms, is around 30%. It is possible that this cyclical pattern is related to, or even caused by, the similarly cyclical nature of the biannual minimum wage increases and therefore real minimum wage corrections.

Ultimately, it appears that the rapid increase in the reported number of shoplifting incidents until around July 2023 has greatly diminished and has, in the period between July 2023 and April 2024, returned to a similar downward trend as the one before the Covid-19 pandemic and the cost-of-living crisis. One explanation for that might be the relatively large minimum wage correction in January 2023 containing the aforementioned 10.15% increase as a response to these two consecutive economic crises. Namely, as real minimum wages and social welfare benefits increased, the necessity for shoplifting might have been rescinded, thus leading to fewer recorded incidents. Again, this relationship is displayed in Figure 3.2.

Given the fact that a relationship between the relatively low minimum wage increases and national shoplifting incidents until 2020 – in light of the information in Figure 3.2 – at first glance is not unequivocally evident, the combination of a relatively high increase in minimum wages and significant drop in shoplifting reports might indicate that there *is* a relationship between these variables, but only *insofar as* the increase has a large enough magnitude. It remains to be seen whether a minimum wage increase is a good forecasting variable when determining the future national monthly number of shoplifting incidents.

## IV. Empirical strategy

In contrast to the strategies set out in the literature as elaborated on before, this research seeks to utilise a *time series* modelling approach. In general, time series models can investigate whether a dependent variable outcome is not only related to the value of a certain independent variable in the same period of measurement, but they can also analyse the potential effect of earlier measurements on later variable outcomes. This fits well into the purpose of this paper's analysis, which aims to evaluate whether a change in the national minimum wage has a forecasting effect – be it immediate or with a certain degree of delay – on the number of national shoplifting incidents.

Time series models are, thereto, an effective empirical strategy when considering *lagging effects* and persistence of variable outcomes. These lagging effects may pertain to an exogenous variable, for instance when one variable is dependent on consecutive measurements of another variable, or to earlier measurements of the variable itself, i.e., when one observation is partially dependent on a previous value of the same variable.

Contemporary economic and financial studies have applied a wide variety of different time series analyses. Therefore, it is necessary to ascertain which specific model is sufficiently applicable to the data and purpose at hand. In the spirit of Shrestha and Bhatta (2018), one can follow a general guideline to determine the usefulness and applicability of a certain time series model. For the purposes of this analysis, therefore, I have chosen an adapted version of the *autoregressive distributed lag* model (henceforth: ARDL model).

ARDL time series models are modified ordinary least squares (OLS) linear regressions that transform a standard linear analysis with cross-sectional data into a linear regression using panel data. ARDL estimations contain two separate equations that are thereafter combined into one regression.

The *autoregression* in the ARDL model refers to the possible dependence of the current value of the outcome variable on earlier measurements of *the same* variable. In our case, this translates to the possibility of the current total number of monthly shoplifting incidents being autocorrelated with values form previous measurement periods. In other words, there exists a *lagging effect* of earlier observations on current observations of the same variable. Concretely, the autocorrelation of the number of monthly shoplifting incidents might indicate that the variable exhibits some form of persistence or, in other words, that there might be a mean value of shoplifts throughout the country that the data revolves around.

Furthermore, the *distributed lag* term in the ARDL model pertains to the possible dependence of the current value of the outcome variable on earlier measurements of *another* variable. In contrast to the autoregressive part of the model, this would contain the possibility that the current value of the total number of monthly shoplifting incidents is related to, or caused by, the minimum wage level during or change that occurred in a previous month. This relationship is particularly of the essence when attempting to construct the forecasting power of the minimum wage level on the number of subsequent shoplifting incidents, which is therefore of the essence for this paper's research question.

Thus, the combination of these two regression subdivisions allows the outcome variable to be both partly dependent on previous measurements of the same variable, as well as partly dependent on current and previous measurements of the independent variable via the aforementioned lagging effect. In general, and for the purposes of this analysis, the ARDL model takes on the following form:

$$ARDL(p,q): \quad Y_t = \alpha + \beta_1 Y_{t-1} + \cdots + \beta_p Y_{t-p} + \delta_0 X_t + \delta_1 X_{t-1} + \cdots + \delta_q X_{t-q} + \varepsilon_t \tag{1}$$

In this model, $p$ indicates the number of lags, i.e., periods before the unit of measurement, for the outcome variable $Y$, and $q$ represents the number of lags for the independent variable $X$. In this analysis, $Y_t$ indicates the national total number of shoplifting incidents in month $t$. Then, $\alpha$ is the intertemporal constant, and $\beta_p Y_{t-p}$ represents the estimates effect $\beta$ of the number of total shoplifting incidents $Y$ in month $t-p$ on the current number of shoplifting incidents in month $t$. Likewise, $\delta_0 X_t$ indicates the effect $\delta$ of the minimum wage level $X$ in the same month of measurement $t$, and subsequently $\delta_q X_{t-q}$ represents the estimates effect $\delta$ of the minimum wage level $X$ in month $t-q$. Finally, $\varepsilon_t$ indicates the error term $\varepsilon$ in the month of measurement $t$. Given the above, $\delta_q$ is assumed to be the coefficient of interest for this paper. As implied in the explanation above, the period of measurement, i.e. month, is formulated by $t$. This time indicator takes on the value of any whole number between 1 and 148, with $t = 1$

indicating January 2012 and $t = 148$ indicating April 2024, with values 2 to 147 corresponding to every subsequent month in between those periods.

In addition to the standard variables in the ARDL setting, the adjusted model also includes both year and month dummy variables. This inclusion has two benefits. Firstly, year variables can take away the idiosyncrasies that are measured within a twelve-month span that are unique to a given year. For instance, in 2020 The Netherlands had undergone the Covid-19 pandemic. The public health crisis that followed led to the pre-emptive closure of most retail stores and large restricted access to grocery stores and supermarkets.

This inherently lowered the number of opportunities to undertake shoplifting, leading to fewer recorded shoplifting incidents across the year that did not have anything to do with changes in the national minimum wage – which can be recollected from Figure 3.2 as well. If the 2020 year-variable were not included in the ARDL model, the regression would assume that this shoplifting decrease was a direct consequence of the minimum wage level in that month/year or a previous period. Therefore, the inclusion of a year variable greatly diminishes endogeneity of the forecasting model.

Furthermore, the month dummy variable in the adjusted ARDL model can account for possible seasonality effects that otherwise would have been included in the minimum wage forecasting power. Therefore, the month dummy captures cyclical shoplifting hikes which are idiosyncratic for a specific month for all years of measurement. For instance, it might be possible that the winter months – December, January, and February – are more prone to shoplifting than other months because of early darkness and the higher-than-average expenditures for national holidays, such as Christmas and New Year's Eve. Their yearly recurrence would constitute some form of trend, which the inclusion of month dummies can filter from the relationship of interest for this paper, namely between the real minimum wage and the monthly number of shoplifting incidents.

Finally, it is necessary to determine the number of lags for both the real minimum wage level and the monthly number of shoplifting rates. The dummy variables as discussed above cannot be subject to lagging effects in this model since that would create issues regarding collinearity.

For the autoregressive lag of the dependent variable, i.e., the monthly number of shoplifting incidents, a standard lag of one month may be deemed plausible. For instance, it might be the case that some short-term shock occurred in The Netherlands, which could have caused temporary higher inflation that siphoned through into the next month before the inflation level reconverged to its long-term trend. This might have caused an uptick in shoplifting levels in the month of the shock and the month after, and therefore the level of shoplifting incidents may, at least in part, be dependent on the observed shoplifts a month prior. On the other hand, overstretching lags in this regard may not be plausible, because within a longer period the exact autocorrelation may be smoothened over too many periods, therefore diluting the true short-term autocorrelation between two months. Furthermore, a longer month-on-month lag period risks capturing part of the year dummy effect which, therefore, may cancel out part of the effect that lowers endogeneity.

The real minimum wage lag, secondly, is also set at one month. This has to do with both the nominal minimum wage increase and the month-on-month inflation rate. For instance, whenever the minimum wage is raised every January and July, this is done on the first day of the month. Yet, most Dutch employees and social welfare beneficiaries receive their income or benefits within the last week of the month as explained before. Thus, they cannot materialise their increased income within the month in which the nominal minimum wage was increased. Therefore, a true minimum wage increase can only affect the number of shoplifting incidents once all beneficiaries have received at least one full pay-out, which will only have occurred in the month after the actual increase. Thus, shoplifting forecasts should be dependent on both the month in which they take place as well as previous months in which the minimum wage would have increased.

Aside from the biannual minimum wage increases, the number of shoplifts can also be influenced by both the current monthly inflation rate and the previous one. This is because the inflationary effect on, for example, groceries is not immediately included into the prices of goods during the same month since most cost increases need to be passed down the supply chain. Therefore, it stands to reason that these consumer prices are partly dependent on the inflation of production costs one month before the observation period. In sum, therefore, the real minimum wage, which is the product of the CPI and nominal minimum wage, can also affect shoplifting incidents from the period before that of the actual observation.

Considering all the above, the main regression equation of this paper becomes as follows:

$$ARDL(1,1): \quad Y_t = \alpha + \beta_0 Y_{t-1} + \delta_0 X_t + \delta_1 X_{t-1} + \gamma A + \theta m + \varepsilon_t \tag{2}$$

In this model, $\alpha$ is the intertemporal constant, and $\beta_0 Y_{t-1}$ represents the estimated effect $\beta_0$ of the number of total shoplifting incidents $Y$ in month $t-1$ on the current number of shoplifting incidents in month $t$. As mentioned before, the time indicator $t$ takes on the value of any whole number between 1 and 148, with $t = 1$ indicating January 2012 and $t = 148$ indicating April 2024, with values 2 to 147 corresponding to every subsequent month in between those periods.

Likewise, $\delta_0 X_t$ indicates the effect $\delta_0$ of the real minimum wage level $X$ in the same month of measurement $t$, and subsequently $\delta_1 X_{t-1}$ represents the estimates effect $\delta_1$ of the minimum wage level $X$ in month $t-1$. Furthermore, $\gamma$ indicates the idiosyncratic year effect of year dummy $A$, where $A$ takes on any whole number value between and including 2012 and 2024, while $\theta$ indicates the seasonality effect of month $m$, where $m$ takes on any whole number value between and including 1 and 12. Finally, $\varepsilon_t$ indicates the error term $\varepsilon$ in the month of measurement $t$. Given the above, $\delta_1$ is assumed to be the coefficient of interest for this paper.

However, when merely including a one-lagged model in the regression analysis, it remains unknown whether the choice for said one lag in real minimum wages and shoplifting autocorrelation was justified. Namely, it may still be possible that the data indicate that the real minimum wage lag on shoplifting incidents is, in fact, slightly longer than just a one-month delay. For instance, it may occur that those receiving minimum wages and government welfare beneficiaries take longer to adjust their attitude and behaviour away from shoplifting when their real income has already increased. Instead of the presumed one-month delay in equation (2), for example, recipients might be more inclined to wait for more income security before changing their shoplifting behaviour. Thereto, they would need a longer period after the increase in real minimum wages before deciding whether to forego shoplifting.

Furthermore, it may also be plausible that the shoplifting autocorrelation is longer than the aforementioned one-month period. Namely, it is plausible that certain shocks in the number of shoplifting incidents nationwide can be more persistent, therefore leading to shoplifting observations being more strongly autocorrelated over multiple periods than merely one month in between. One can, concretely, think of the Covid-19 pandemic and subsequent retail store closures. Namely, the initial shock in March 2020 lasted until the first easement of certain movement and store opening restrictions in the summer of 2020. Thus, the autocorrelation of the number of shopliftings might, in fact, be persistent over a timespan of approximately four months, rather than the previously held one-month autocorrelation.

In analysing these different lag lengths and their potential alternate outcomes to the results from equation (2), it is helpful to set up an alternative lag length approach to investigate whether the one-month lag has more explanatory power of the data – ergo the number of shoplifting incidents – than any other lag choice. As an adequate counter to that regression equation, then, I propose a *two*-lag second model estimation for both the real minimum wage and shoplifting autocorrelation. This secondary equation takes on the following form:

$$ARDL(2,2): \quad Y_t = \alpha + \beta_0 Y_{t-1} + \beta_1 Y_{t-2} + \delta_0 X_t + \delta_1 X_{t-1} + \delta_2 X_{t-2} + \gamma A + \theta m + \varepsilon_t \qquad (3)$$

The coefficients and variables have the same meaning as their namesakes in equation (2), in addition to which $\beta_1 Y_{t-2}$ estimates the effect $\beta_1$ of the number of total shoplifting incidents $Y$ in month $t-2$ on the current number of shoplifting incidents in month $t$. Furthermore, $\delta_2 X_{t-2}$ indicates the estimated effect $\delta_2$ of the real minimum wage level $X$ in month $t-2$ on the national monthly number of shoplifting incidents in the current month. Like equation (2), the time indicator $t$ takes on the value of any whole number between 1 and 148, with $t = 1$ indicating January 2012 and $t = 148$ indicating April 2024, with values 2 to 147 corresponding to every subsequent month in between those periods.

Aside from these two main regressions, I will perform three subtests which are set out below.

The first subtest pertains to the possible outcome differences between the inclusion of either the real minimum wage or nominal minimum wage in equation (2). Thereto, I will analyse whether the direct inclusion of the monthly inflation-adjusted minimum wage in equation (2) produces a significantly different outcome to the inclusion of both the monthly nominal minimum wage and month-on-month inflation separately.

If the total effect of the separate variables combined is, for example, relatively large compared to the simple effect of the inflation-adjusted wages, this might indicate that people have more incentive to shoplift as a mere consequence of rising inflation and that they dilute the importance of their monthly salaries when deciding to shoplift or not. On the other hand, if the adjusted effect is larger than the sum of the separate effects, this might imply that beneficiaries already discount the inflation rates into their wages before making the choice to shoplift. This equation, therefore, becomes as follows:

$$ARDL(1,1,1): \quad Y_t = \alpha + \beta_0 Y_{t-1} + \mu_0 N_t + \mu_1 N_{t-1} + \nu_0 I_t + \nu_1 I_{t-1} + \gamma A + \theta m + \varepsilon_t \tag{4}$$

Where $\alpha$ is the intertemporal constant, and $\beta_0 Y_{t-1}$ represents the estimates effect $\beta_0$ of the number of total shoplifting incidents $Y$ in month $t-1$ on the current number of shoplifting incidents in month $t$. Again, $t$ takes on the value of any whole number between 1 and 148, with $t = 1$ indicating January 2012 and $t = 148$ indicating April 2024, with values 2 to 147 corresponding to every month in between those periods. In contrast to equation (2), however, here $\mu_0 N_t$ indicates the effect $\mu_0$ on the *nominal* minimum wage level $N$ in month $t$, and $\mu_1 N_{t-1}$ represents the estimated effect $\mu_1$ of the nominal minimum wage level $N$ in month $t-1$. Then, $\nu_0 I_t$ indicates the effect $\nu_0$ of the monthly CPI level $I$ in month $t$, with base value 100 for $t = 1$, and subsequently $\nu_1 I_{t-1}$ indicates the effect $\nu_1$ on the monthly CPI level $I$ in month $t-1$. Lastly, $\gamma A$, $\theta m$ and $\varepsilon_t$ retain the same definitions as set out for equation (2).

As in equations (2) and (3), $\gamma$ indicates the idiosyncratic year effect of year dummy $A$, where $A$ takes on any whole number value between and including 2012 and 2024, while $\theta$ indicates the seasonality effect of month $m$, where $m$ takes on any whole number value between and including 1 and 12. Finally, $\varepsilon_t$ indicates the error term $\varepsilon$ in month $t$.

Hereafter, the second subtest is intended to analyse the forecasting power of the real minimum wage in the estimated model on the number of monthly shoplifting incidents using a graphical analysis. I shall explain the underlying procedure below.

After the results of the two main models in this paper have been analysed, namely regression equations (2) and (3), I will compare the explanatory power and statistical significance of the results of both models. The model that possesses the largest and most accurate forecasting power will then be chosen as the base for an out-of-sample forecasting analysis.

To this, the model with the highest forecasting ability will be regressed once more, yet this time only using the available datapoints from January 2012 until February 2016. Then, the coefficients and other variable estimates that result from this limited model regression are applied to the actual monthly real minimum wage observations from February 2016 until March 2020. Therefore, using data from January 2012 until February 2016 on both the number of shoplifting incidents and the real minimum wage, the model will produce forecasted values for the number of shoplifting incidents between February 2016 and March 2020.

The upper time limit, March 2020, has been chosen specifically since this month contains the start of the first Covid-19 lockdown. If one were to include the pandemic years into the model forecasts, this would likely lead to substantial losses of forecasting power because the sudden decrease in shoplifting incidents was not tied to the real minimum wage, but rather due to store closures in light of the pandemic restrictions. The out-of-sample analysis in this paper, therefore, rather analyses whether regression model (2) or (3) can forecast future shoplifting values under normal economic circumstances.

One must note that the year dummies between 2016 and 2020 will not be estimated in the limited model because the data included in said regression only extends to February 2016. Therefore, it is plausible that the out-of-sample prediction will consistently over- or underestimate the number of shoplifting incidents between 2016 and 2020 due to the absence of the year constants.

To placate some of this bias, I propose to use the estimated coefficient of the year dummy value in 2015 – in the reduced model – as the standard year dummy between 2016 and 2020 as well. This is reasonable since the period between 2016 and 2020 has not seen a drastic reduction in the number of shoplifting incidents and has not experienced any long-lasting shocks, as can also be seen in Figure 3.2. The application of the month dummies, on the other hand, will remain unaffected since all months are still observed within the limited model.

The forecasted values will then be compared graphically to the true observed values over the latter half of the measurement period to determine whether this model is able to accurately forecast the number of shoplifting incidents without any interfering crisis as time progresses, which in this case would be the Covid-19 pandemic. This test can therefore analyse whether the estimated ARDL model *itself* a significant baseline forecasting power.

Ultimately, the third subtest contains a *Granger Causality test* (henceforth: GCT) on either regression model (2) or (3), depending on the earlier explained factors of forecasting power and statistical significance. The GCT investigates whether the correlation between both variables in the model, i.e., the national total number of monthly shoplifting incidents and the real minimum wage level, is indicative of a strong forecasting ability of the independent variable on the outcome variable.

In contrast to the graphical forecasting power analysis as explained above, this test is purely statistical. Furthermore, the GCT is not intended to conclude whether the model itself has a significant forecasting ability, but whether a *variable* within the model possesses said power.

The GCT assesses whether the value of the minimum wage level in month $t - p$ is useful in forecasting the value of the total number of shoplifting incidents in month $t$. If this forecasting ability is indeed significant, it is said that the independent variable *Granger causes* the dependent variable.

In wording, the GCT evaluates whether the exclusion of an independent variable increases the error variance of the dependent variable. This process is repeated in reverse to examine whether the exclusion of the independent variable – which was the dependent variable in the first regression equation – increases the error variance of the other dependent variable – the erstwhile independent variable.

The GCT contains two sets of hypotheses as set out below. The first reads as follows:

$$H_0: Minimum\ wage\ levels\ are\ not\ Granger\ caused\ by\ shoplifting\ incidents$$
$$H_a: Minimum\ wage\ levels\ are\ Granger\ caused\ by\ shoplifting\ incidents$$

Conversely, the other hypotheses indicate a reverse relationship to the set above:

$$H_0: Shoplifting\ incidents\ are\ not\ Granger\ caused\ by\ minimum\ wage\ levels$$
$$H_a: Shoplifting\ incidents\ are\ Granger\ caused\ by\ minimum\ wage\ levels$$

The combination of the ARDL regression analysis and the Granger Causality test makes for a comprehensive approach in establishing whether the real minimum wage level is useful in forecasting values of later records of the national monthly reported shoplifting incidents.

Yet, it must be noted that *Granger causality* is not identical to *true* causality per se. Namely, it may very well be possible that other variable that have not been included in the model exert influence, significantly even, on both the independent and outcome variable. Such a situation, which is by no means unthinkable, will lead to omitted variable bias and, therefore, makes for an over- or underestimation of the true causal effect of the minimum wage level and increase thereof on the number of national and municipal shoplifting incidents, if it were to exist at all.

Ultimately, the national shoplifting sample is a mere sum, and not a weighted one, of all municipalities across The Netherlands. Therefore, a change in the national total of shoplifting incidents is assumed to be the percentage change that *all* municipalities will have experienced as well. Yet, it is highly plausible that some municipalities have had more drastic shifts in shoplifting behaviour than others.

## V. Results

The results of the $ARDL(1,1)$ model as described in equation (2) are presented below.

*Table 5.1: Regression results of the one-lag model estimation with month and year dummies*

| Variable | Coefficient and standard error | Variable | Coefficient and standard error |
|---|---|---|---|
| *Shoplifting variables* | | *Month dummies* | |
| One-lag number of shoplifts | 0.325*** | January | 109.060 |
| | (0.086) | | (92.186) |
| *Minimum wage variables* | | February | 373.583*** |
| Zero-lag real minimum wage | -473.265 | | (83.509) |
| | (307.145) | March | 587.797*** |
| One-lag real minimum wage | -464.575** | | (79.977) |
| | (214.180) | April | -49.266 |
| *Year dummies* | | | (77.120) |
| 2012 | -518.158** | May | 126.889 |
| | (200.375) | | (85.575) |
| 2013 | -542.468** | June | -40.099 |
| | (214.223) | | (83.556) |
| 2014 | -613.938*** | July | 8.143 |
| | (214.292) | | (87.972) |
| 2015 | -668.842*** | August | 67.656 |
| | (214.132) | | (89.747) |
| 2016 | -652.254*** | September | 210.550** |
| | (198.761) | | (88.152) |
| 2017 | -637.303*** | October | 424.228*** |
| | (177.644) | | (84.094) |
| 2018 | -527.851*** | November | 364.035*** |
| | (162.445) | | (78.526) |
| 2019 | -439.188*** | | |
| | (159.438) | *Regression constant* | 10,404.790*** |
| 2020 | -652.592*** | | (2,353.439) |
| | (155.801) | | |
| 2021 | -887.303*** | | |
| | (192.074) | | |
| 2022 | -855.548*** | | |
| | (266.698) | | |
| 2023 | -32.531 | | |
| | (127.778) | | |

*Notes:* Table 5.1 denotes the regression results from equation (2). Standard errors are provided in parentheses. For the year dummies, the variable *2024* has been omitted due to multicollinearity. The same holds for the month dummies, where *December* has been omitted. Model F-statistic = 23.62, $N$ = 147, and $R^2$ = 0.837; ** $p < 0.05$, *** $p < 0.01$.

First and foremost, Table 5.1 indicates a relatively strong autocorrelation between the current number of monthly shoplifting incidents and the observation one month prior. Namely, if the number of shoplifts in the previous month were to have increased by 1, the current month, on average, should see an increase in the reported amount of shoplifts of 0.325. Therefore, when assuming the average month has seen around 3,500 shoplifting incidents nationwide, the persistence of this number in the previous month is around 1,138. This implies that, again on average, approximately 2,362 monthly shoplifting incidents are related to variables that lay outside of the scope of the outcome variable's autocorrelation.

Furthermore, it appears that the one-lag real minimum wage effect on the current number of monthly shoplifting rates is significant at the 5%-level. This is largely in line with the reasoning behind the lag-order choice as described before. On average, if the real minimum wage were to increase with €1, then the monthly number of shoplifting rates would decrease with approximately 465 incidents. When comparing this information to the average number of monthly shoplifting rates during the measurement period, which is around 3,500 nationwide, this translates into an 13.3% reduction in the number of shoplifts for every €1 increase in the real minimum wage – save for the persistence and autocorrelation as explained above. This is a substantial relationship and core to this paper's relevance. Interestingly, the concurring real wage effect, as seen in Table 5.1, is largely insignificant. Therefore, one cannot interpret the relationship between the monthly number of shoplifts and the concurring real minimum wage.

Then, save for the year dummy 2023, one can clearly see the great statistical significance of the year effects on the number of monthly shoplifting incidents. These effects are all in comparison to 2024, with the 2024 dummy effect taking on value zero, to combat issues with multicollinearity. The year dummies indicate the clear downward trend until 2020 as illustrated in Figure 3.2, and a more drastic reduction in the number of shoplifting incidents between 2020 and 2022. This might have been driven by lockdowns because of the Covid-19 pandemic.

Contrastingly, most of the month dummies – again, as an absolute comparison to the omitted December dummy – indicate no statistically significant effect on the national number of shoplifts. If, however, one compares the differences between the significance of the month effects, it becomes apparent that the autumn and winter months have a relatively large positive impact on the number of shoplifting incidents, while the spring and summer months have a largely insignificant and small positive effect on the number of shoplifts. This difference might indicate some form of seasonality, yet this remains uncertain given the available data and insignificant results of the analysis.

Finally, one must acknowledge that the regression constant is highly significant. In itself, it does not have any predictive power or relevance. Namely, the data does not contain months with more than 10,000 reported cases of shoplifting, nor are there any months in which the real minimum wage is zero. When the constant is read in conjunction with the other variables, which are all negative, contrastingly, it becomes apparent that the variable averages combined with the constant term do make for a sensible shoplifting estimation.

Hereafter, the results from the regression of the $ARDL(2,2)$ model as set out in equation (3) are presented in Table 5.2.

*Table 5.2: Regression results of the two-lag model estimation with month and year dummies*

| **Variable** | **Coefficient and standard error** | **Variable** | **Coefficient and standard error** |
|---|---|---|---|
| *Shoplifting variables* | | *Month dummies* | |
| One-lag number of shoplifts | 0.341*** | January | 133.418 |
| | (0.093) | | (93.763) |
| Two-lag number of shoplifts | -0.093 | February | 325.161*** |
| | (0.089) | | (90.670) |
| *Minimum wage variables* | | March | 587.636*** |
| Zero-lag real minimum wage | -505.834 | | (88.058) |
| | (308.781) | April | -40.011 |
| One-lag real minimum wage | -268.317 | | (86.045) |
| | (283.067) | May | 165.455* |
| Two-lag real minimum wage | -270.050 | | (89.196) |
| | (219.552) | June | -57.998 |
| *Year dummies* | | | (86.337) |
| 2012 | -577.097*** | July | -5.029 |
| | (206.598) | | (88.762) |
| 2013 | -609.743*** | August | 36.809 |
| | (219.271) | | (93.163) |
| 2014 | -685.031*** | September | 189.120** |
| | (219.848) | | (93.400) |
| 2015 | -750.997*** | October | 398.511*** |
| | (221.767) | | (90.307) |
| 2016 | -732.564*** | November | 356.505*** |
| | (206.716) | | (84.371) |
| 2017 | -712.358*** | | |
| | (185.285) | *Regression constant* | 11,620.470*** |
| 2018 | -593.957*** | | (2,482.255) |
| | (170.364) | | |
| 2019 | -496.575*** | | |
| | (165.528) | | |
| 2020 | -724.727*** | | |
| | (164.873) | | |
| 2021 | -988.786*** | | |
| | (208.690) | | |
| 2022 | -959.843*** | | |
| | (278.018) | | |
| 2023 | -59.249 | | |
| | (129.185) | | |

*Notes:* Table 5.2 denotes the regression results from equation (3). Standard errors are provided in parentheses. For the year dummies, the variable *2024* has been omitted due to multicollinearity. The same holds for the month dummies, where *December* has been omitted. Model F-statistic = 21.94, $N$ = 146, and $R^2$ = 0.802; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Like the one-lag autocorrelation of the monthly number of shoplifting incidents as described in Table 5.1, the result in Table 5.2 indicates a substantial and highly significant one-month autocorrelation – or persistence – for shoplifts. Namely, if the number of registered shoplifts in the previous month were to increase by 1, according to the two-lag model results the number of shoplifts in the current month ought to increase by 0.341.

Contrastingly, the results of equation (3) suggest that the two-month autocorrelation is largely insignificant. This might imply that, on average, past shocks or other permanent changes in the number of shoplifting incidents, which are caused by factors that have not been included into this model, only affect the number of shoplifts by a significant magnitude within a one-month timespan after their occurrence.

The starkest difference in results, compared to the estimated coefficients of equation (2) at least, is the major insignificance of all real minimum wage variables in model (3). It appears that, when accounting for a two-month adjustment period for the shoplifting behaviour of minimum wage earners and government welfare beneficiaries, all possible effects of the inflation-adjusted minimum wage become insignificant.

Not only does this inclusion reduce the explanatory power of the current minimum wage level, but also of all delayed effects. Therefore, one cannot interpret the paramount forecasting power of the real minimum wage level on the number of monthly shoplifting incidents in this model, at least when provided with the available data. This would greatly hamper the assessment set out in this paper, which is to establish that such a forecasting power exists.

Simultaneously, both the year and month dummies appear to pick up some of the effects of the lagged real minimum wages and shoplifting incidents autocorrelation. Namely, the downwards sloping trend in the number of shoplifts throughout the measurement period becomes visible in the highly statistically significant year dummy effects – save for the year dummy in 2023. Clearly, the pandemic years 2020-2022 indicate the part of the overall year effect that substantially decreases the number of monthly shoplifting incidents.

Moreover, as was the case in Table 5.1 as well, the aforementioned seasonality effect between the autumn and winter months on the one hand and the spring and summer months on the other hand comes forward in the results illustrated in Table 5.2. Compared to December, for instance, on average February will see around 325 more monthly shoplifts nationwide, whereas June and July do not indicate a significant difference in shoplifting rates.

Finally, without the inclusion of the lagged real minimum wage effects, the constant factor in regression (3) on itself is of no use. Namely, there are no observations that exceed 5,000 monthly shoplifting incidents. Thereto, it must be noted that the real minimum wage variables may not be included anyway since these are all highly insignificant.

Furthermore, Table 5.3 sets out the results of the $ARDL(1,1,1)$ subtest as per equation (4).

*Table 5.3: Regression results of the adjusted model estimation with month and year dummies*

| Variable | Coefficient and standard error | Variable | Coefficient and standard error |
|---|---|---|---|
| *Shoplifting variables* | | *Month dummies* | |
| One-lag number of shoplifts | 0.333*** | January | 115.360 |
| | (0.087) | | (104.199) |
| *Minimum wage variables* | | February | 375.605*** |
| Zero-lag nominal minimum wage | -266.685 | | (98.913) |
| | (434.688) | March | 595.936*** |
| One-lag nominal minimum wage | -368.495* | | (94.211) |
| | (204.090) | April | -36.189 |
| *Inflation/CPI variables* | | | (89.497) |
| Zero-lag CPI | 33.432 | May | 148.326 |
| | (26.947) | | (96.499) |
| One-lag CPI | 25.879 | June | -19.699 |
| | (26.033) | | (95.418) |
| *Year dummies* | | July | 10.920 |
| 2012 | -213.204 | | (93.498) |
| | (1,337.551) | August | 73.759 |
| 2013 | -234.863 | | (90.385) |
| | (1,290.576) | September | 215.743** |
| 2014 | -316.364 | | (89.237) |
| | (1,256.490) | October | 426.565*** |
| 2015 | -382.738 | | (84.710) |
| | (1,230.560) | November | 368.358*** |
| 2016 | -391.391 | | (81.598) |
| | (1,189.609) | | |
| 2017 | -416.773 | *Regression constant* | 1,634.823 |
| | (1,102.296) | | (4,336.270) |
| 2018 | -343.226 | | |
| | (1,015.259) | | |
| 2019 | -268.910 | | |
| | (933.800) | | |
| 2020 | -507.948 | | |
| | (856.046) | | |
| 2021 | -742.432 | | |
| | (807.211) | | |
| 2022 | -721.095 | | |
| | (704.740) | | |
| 2023 | 1.847 | | |
| | (257.352) | | |

*Notes:* Table 5.3 denotes the regression results from equation (4). Standard errors are provided in parentheses. For the year dummies, the variable *2024* has been omitted due to multicollinearity. The same holds for the month dummies, where *December* has been omitted. Model F-statistic = 21.50, $N$ = 147, and $R^2$ = 0.797; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Akin to the estimated regression coefficient in Table 5.1, the results from equation (4) indicate a substantial autocorrelation between the current monthly shoplifting records and the previous month. Namely, if the number of shoplifts in the prior month were to increase by 1, the current number of shoplifting incidents would increase by one-third, or 0.333. Moreover, in a similar pattern to the estimators of equation (2), the autumn and winter months signal a strongly significant and positive increase in the number of monthly shoplifting incidents compared to December, whereas there exists no such relationship, at least statistically significant, between December and the spring and summer months, thereby only partly explaining some form of seasonality.

Contrastingly, the one-lag real minimum wage effect has lost part of its predictive power to the CPI variable. Yet, as one must acknowledge, whereas the one-lag real minimum wage estimand is somewhat statistically significant, this does not hold for either the zero-lag real minimum wage or both CPI variables. Therefore, given the available data, one cannot interpret the CPI forecasting power on the number of monthly shoplifting incidents.

Notwithstanding the insignificance of the CPI effects, one can compare the real minimum wage estimands from Table 5.1 with the nominal minimum wage and CPI coefficients in Table 5.3 as an indication of the relative importance of the variables. For instance, in January 2016 the real minimum wage was €8.35, with a nominal minimum wage of €8.77 and a CPI of 105.03 (where January 2012 takes on value 100).

The one-lag real minimum wage effect for February 2016, as per Table 5.1, would then be approximately -4,074 compared to the constant of 10,405. In Table 5.3, the combined effect of the one-lag nominal minimum wage and one-lag CPI on the number of shoplifts would be approximately -514, namely a shoplifting decrease of 3,232 due to the nominal minimum wage and a shoplifting increase of 2,718 because of the CPI, compared to a constant value of 1,635. Therefore, both effects are roughly comparable to their percentage decrease relative to the respective constant factors.

Yet, as is pertinent to this paper, when the nominal minimum wage and CPI effects are estimated separately, the total effect becomes highly insignificant. Contrastingly, the one-lag *real* minimum wage effect, thus when the nominal and CPI effects are combined, becomes greatly statistically significant. This might imply that minimum wage recipients and beneficiaries of government welfare programs take their inflation-adjusted income as the base to decide whether to partake in shoplifting behaviour, rather than receiving the nominal income and basing their decision on the inflated prices in, say, grocery stores.

Ultimately, the inclusion of the CPI variables leads to a drastic reduction in the significance of the year dummies in Table 5.3. Since the CPI measures month-on-month changes in the inflation level for every month during the year, it may very well be possible that the CPI lagging effects take away a substantial part of the dummy year effects. Namely, the year dummies account for yearly idiosyncrasies in the number of shoplifting incidents, but these idiosyncrasies might also be attributed to the changes and fluctuations in yearly inflation rates. As these developments are now independently covered by the inclusion of the CPI variables, this detrends the year effect on the number of shoplifting rates, thereby rendering the year dummies obsolete or redundant in the regression equation – which is reflected in the complete insignificance of all year dummies in Table 5.3.

The second subtest revolves around the out-of-sample prediction as described in the previous chapter. Therefore, one must first assess whether model (2) or (3) possesses a greater forecasting power and statistical significance than the other.

In general, one can see that the one-lag model, i.e., model (2), is more fitting for the data at hand than model (3) in light of various characteristics. For instance, Table 5.1 indicates a highly significant coefficient for the one-lag real minimum wage level, whereas Table 5.2 contains solely largely insignificant real minimum wage coefficients and effects on the national number of shoplifting incidents. Especially provided the aim of this paper, which is to establish a possible forecasting power of a lagged minimum wage effect on the number of shoplifting incidents, it becomes apparent that model (2) exhibits a superior usefulness to model (3) in such an analysis.

Furthermore, the complete model in equation (2) has a larger test statistic and therefore significance than model (3), namely 23.62 against 21.94 respectively. Moreover, the $R^2$-value, i.e., the percentage of the variance in the outcome variable – the number of shoplifting incidents – that is explained by the independent variable – the real minimum wage level – is also higher in model (2) than in model (3), namely 0.837 compared to 0.802.

All in all, model (2) should be applied in the out-of-sample prediction and graphical analysis since it has greater applicability than model (3), both in reasoning and statistical significance.

Thereto, the $ARDL(1,1)$ model in equation (2) was regressed once more, yet this time only using the available shoplifting and minimum wage data from January 2012 until February 2016. The resulting estimated coefficients and regression constants were thereafter applied to the actual real minimum wage levels in the period between February 2016 and March 2020.

The resulting forecasted (or predicted) values of the monthly national number of shoplifting incidents were then plotted against the shoplifting rates that were actually observed. The figure below illustrates the results of the out-of-sample predictions as described above.

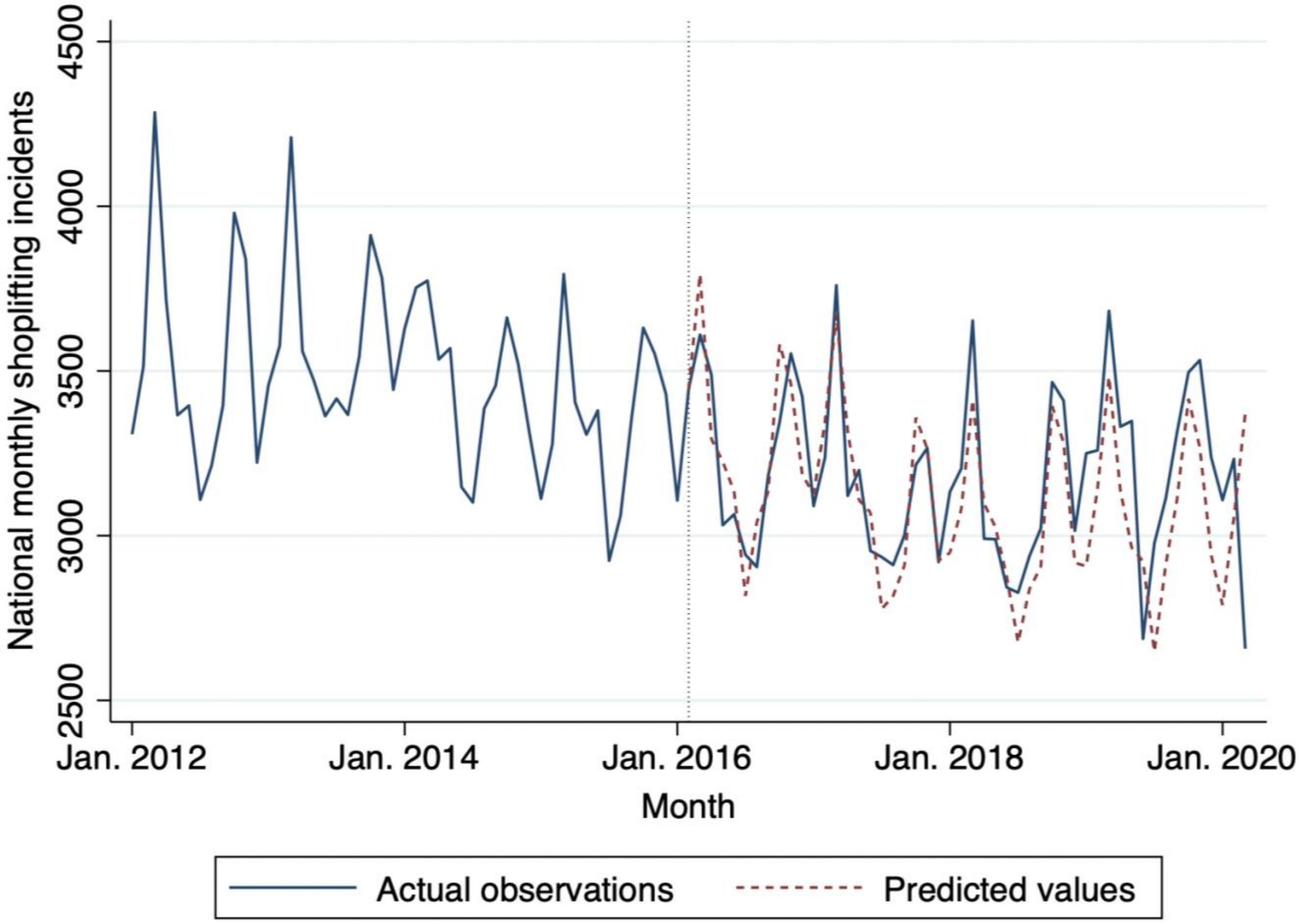


*Figure 5.4: Comparison between the actual records of the national monthly number of shoplifting incidents and the corresponding regression outcome predictions, January 2012 to March 2020*

As one can clearly deduct from Figure 5.4, the predicted values of the number of shoplifts in the model between 2016 and 2020 are substantially similar to the true observed datapoints. The regression in equation (2), therefore, in general may be seen to have an accurate predictive and forecasting power for future values that are not subject to sudden social or economic shocks. This is even more the case since the predictions in Figure 5.4 are based on only a third of the number of observations that the fully estimated coefficients in Table 5.1 are based upon.

The largest estimation error is found in March 2020, where the estimated number of shoplifting incidents is approximately 25% higher than the recorded value. Yet, it is highly plausible that this shoplifting decrease was caused by the first national Covid-19 lockdown which commenced halfway through March 2020. It stands to reason that the estimations will more closely follow the true observations afterwards since the initial shoplifting shock will have subsided by then.

Furthermore, the predicted values are partly dependent on a year dummy that could not be estimated and wherefore the constant value of the dummy in 2015 has been applied. Given the slightly decreasing trend in the overall predicted values, yet the similar way in which these predicted values are developing compared to the actual observations, it is plausible that the real year dummies between 2016 and 2020 should have been increased, thereby diluting most of the largest differences – akin to the estimated true year dummies in the full model of Table 5.1. This is, however, a problem that will necessarily arise as well when predicting future values of the number of shoplifting incidents instead of predicting current values on the basis of past observations of the real minimum wage level.

The third and final additional test encompassed the Granger Causality test (GCT) as elaborated upon previously. This test is intended to signal whether one time series variable is useful in predicting both past and future values of another time series variable. The analysis compares the variance of the error terms of the outcome variable when the running variable is included in the regression model to the situation where this independent variable is excluded from the estimation. If it reduces the error variance of the outcome variable, then the independent variable – the real minimum wage level – *Granger causes* the number of shoplifting incidents.

As discussed before, this test is repeated in reverse to assess whether the explanatory power is vested in the number of shoplifting incidents itself rather than the real minimum wage level. The results of these two Granger Causality tests are demonstrated in Table 5.5.

*Table 5.5: Results of the Granger causality Wald tests for forecasting power*

| **Null hypothesis** | **Outcome variable** | **Excluded variable** | **$\chi^2$-statistic** | **P-value** |
|---|---|---|---|---|
| I | Real minimum wage | Shoplifting incidents | 4.430 | 0.109 |
| II | Shoplifting incidents | Real minimum wage | 18.620 | 0.000 |

*Notes:* Table 5.5 indicates the results of the Granger Causality tests as described earlier on. Granger causality refers to the predictive or forecasting power of one variable on another variable.

As one may recall, the first null hypothesis stated that the number of monthly shoplifting incidents did not Granger cause the real minimum wages. The result of the first Granger Causality test, as per Table 5.5, suggests that this null hypothesis may not be rejected at the 5%-significance level since the p-value equals 0.109. Therefore, one cannot conclude that the number of shoplifts Granger causes the real minimum wage level.

On the other hand, the second null hypothesis stated that the real minimum wage did not Granger cause the number of monthly shoplifting incidents. The strong test statistic in the second Granger Causality test and the corresponding p-value imply that this null hypothesis may be rejected at both the 5%-significance and 1%-significance level. Therefore, one rejects the second null hypothesis.

Thus, it may be established that the real minimum wage *Granger causes* the number of monthly shoplifting incidents. Concretely, this means that the real minimum wage *is* a good forecasting variable for future values of the national – and by extent municipal – number of monthly shoplifting incidents.

## VI. Discussion

In the various analyses and regressions that were set out in this paper, I have investigated the role of monthly real minimum wage levels on the Dutch national monthly number of shoplifting incidents. The ARDL time series models as elaborated on earlier all show – albeit that some perform more convincingly than others – that an increase in the real minimum wage level in both the current month and the previous month of record forecasts a decrease in the number of shoplifting incidents nationwide. Specifically, the results suggest that the one-month delay in real minimum wage effects – as presented in Table 5.1 – leads to a 13.3% reduction in the number of shoplifts for every €1 increase in the real minimum wage level.

This effect can be explained using the opportunity cost approach as set out by Beauchamp and Chan (2014). Namely, as workers – and for the purposes of this paper, beneficiaries of government welfare – are earning a higher real wage than before, they will become less inclined to commit crimes that may have consequences for their employment and livelihood. This is the case, because the opportunity costs of committing crimes, such as shoplifting, will increase when the real wage increases as well; one only commits crimes if one expects a net utility gain, which is lowered when the expected crime costs (like unemployment or prison time) rise. Individuals, therefore, adjust their cost-benefit analyses and will gradually substitute away from committing petty crimes, such as shoplifting.

In sharp contrast to this paper, however, Beauchamp and Chan (2014) find that the negative unemployment effects greatly outweigh the positive effects associated with an increase in (real) minimum wages. Thereto, their results would suggest that crime rates generally ought to increase when minimum wages rise, whereas this paper suggests the exact opposite. This difference might have arisen because Beauchamp and Chan have focused on aggregate crime numbers, i.e., the sum of all different types of crime combined, whereas this paper merely pertains to the act of shoplifting. Therefore, it might be the case that the results of this paper could mirror those of Beauchamp and Chan if the data at hand were to encompass total nationwide crime reports as well.

Meanwhile, the results of this paper substantially reflect those established by Gould *et al.* (2002). In their research, the positive minimum wage effects consistently played a domineering role to the negative unemployment effects. This is in line with the expectations in this paper, namely that the presence of opportunity costs will lower crime rates when minimum wages increase and is also replicated in the results of the ARDL time series model estimation. Although the empirical strategy of Gould *et al.* differs from the ARDL models in this research, both use panel data and measure some form of persistence or idiosyncrasy in the data, either via fixed effects constants or using time-variant estimands. Both models, therefore, seem to create similar outcomes and interpretations.

ARDL models, such as those utilised over the course of this paper, have many advantages over other linear estimation panel data models. For instance, ARDL time series can account for not only time-invariant factors by grouping these into an intertemporal constant – similar to fixed effects approaches – but they can also distinguish between autocorrelative trends or effects and time-variant concurring or lagged effects of other factors than the outcome variable. Therefore, ARDL models differ from standard multiple and panel data linear regressions in the sense that they may more easily assume persistence and allow for prolonged and simultaneous effects of multiple observations of one specific variable.

Yet, ARDL time series in general suffer from the same endogeneity issues as regular OLS estimations and variants thereof. For instance, it is plausible that, besides the real minimum wage level, other factors that were omitted from the ARDL models described in the chapters above might have contributed to the eventual outcomes of the monthly number of shoplifting incidents. Think, for example, of the fact that the changes in nominal minimum wages in The Netherlands every January and July: by no means are these the only government program and policy changes that occur during those same months or a short period thereafter. For proper measurement of each individual effect, all policy changes should have been incorporated into the ARDL model. Even then, non-governmental and non-economic changes, which are nearly impossible to measure objectively, remain unmentioned.

Although some of the resulting omitted variable bias could have been picked up by the autocorrelation of the outcome variable, so that fewer possible biases remain for the variable of interest, it remains reasonable that exogenous variables that show little to no correlation with the outcome variable can still influence the outcome. By not disclosing these variables in the model, the estimations assume that at least part of the omitted effects are contributed to the real minimum wage effects, thereby overestimating the latter coefficients. Omitted variable bias, therefore, will still leave room for endogeneity in the models of this research. While the inclusion of the year and month dummies into the standard ARDL model of equation (1) certainly eliminates potential seasonality and yearly idiosyncratic biases, complete negation of endogenous factors in the models above remains unreasonable.

Most ARDL models, including those set out in this paper, are only used for forecasting purposes. While they can be applied to causal hypotheses in theory, the endogeneity issues as explained above render any affirmations of such causality difficult to prove empirically. Subverting endogeneity, therefore, is core when trying to analyse whether the relationship between real minimum wages and crime rates not only exists in forecasting but could also be interpreted as causal. Ideally, these problems could be combated via a randomised experiment in which a perfectly balanced population sample is divided into control and treatment groups.

The treatment group would, in a given period, receive ever-increasing minimum wages that at the minimum keep up with inflation, whereas the control group only receives a set minimum wage that stays at nominal value. The difference in crime rate developments, such as the difference in shoplifting behaviour, between these groups could then be attributed solely to the increase in nominal minimum wages or the parity of real wages to inflation. Save for any possible ethical or practical objections, the population sample must be incredibly large to retrieve reliable differences since the actual number of reported shoplifts per capita, on average, is virtually non-existent.

All in all, this paper establishes that real minimum wage observations are useful and a substantial forecasting variable when estimating future rates of nationwide shoplifting in retail and grocery stores. By providing data driven and accurate estimation models, this paper could function as a base for government calculations of monthly shoplifting rates – which may also be replicated or adjusted to accommodate regional or municipal analyses – through which government agencies can more accurately allocate security and surveillance funds to combat this criminal behaviour during months where said action is truly needed.

## VII. Conclusion

Over the course of this research, I set out to establish whether and in what way an increase in the Dutch national minimum wage is of forecasting value when predicting nationwide monthly shoplifting rates. Using panel data on the monthly number of shoplifts in The Netherlands between 2012 and 2024, and in addition monthly nominal and real wage levels, as well as monthly CPI data, I designed an ARDL time series model with both month and year dummies to account for seasonality and idiosyncrasy effects. The results indicate that an increase in the monthly real minimum wage, i.e., the inflation-adjusted nominal minimum wage, of €1 leads to an average decrease of the total number of shoplifting incidents of approximately 13.3%.

Additional tests thereafter confirmed that the estimated coefficients and models are substantially accurate when comparing predicted shoplifting values to actual observations, and that the real minimum wage is a well-founded forecasting variable for the future number of shoplifting incidents.

Thus, an increase in the nominal minimum wage will, since this leads to an increase in the real minimum wage as well, significantly reduce the national number of shoplifts. This result implies that minimum wage effects greatly outweigh any negative unemployment effects as a consequence of higher employer wage costs, or higher taxes in regard to beneficiaries of government welfare programs that are tied to the minimum wage level.

Ultimately, future research into the relationship between minimum wages and property crimes such as shoplifting could focus on establishing causality between these variables, rather than merely analysing forecasting utility as was the case in this research. Thereto, one can very well use the assumptions and results in this paper as a proper base and elaborate thereon.

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