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Master Thesis

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Spatial preferences in a choice experiment

Visual impact of offshore wind turbines as a case

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Abstract

In this study, I examine how differences in choice experiment survey designs change the obtained results. The survey designs differ by the reminders included, such that one sample was given no reminders, one sample was given a cheap talk reminder and one sample was given both a cheap talk and an opt-out reminder. Using the opt-out reminder in choice experiment surveys is quite new in the literature (Ladenburg and Olsen (2014)), thus, this study is very relevant and explores issues that have not yet been addressed. Furthermore, I both examine how the results differ for the full sample and, additionally, how results differ across gender. By including the gender aspect, the main question that I wish to answer is whether or not male and female respondents react similarly to the reminders and, generally, if willingness to pay estimates differ across gender.

The case used to illustrate this is placement of offshore wind turbines in Denmark. Specificly, I examine preferences for placing wind turbines farther off shore. Using a choice experiment survey design, which includes photo-shopped images of wind turbines placed at 8, 12, 18 or 50 km off shore and a monetary variable reflecting respondents' hypothetical cost from choosing the different alternatives, preferences are estimated. Preferences are evaluated relative to the status quo alternative where wind turbines are placed 8 kilometers from shore with no additional cost for the respondents.

There are a couple of things that are expected to be seen from the results. First off, we expect that when respondents receive any form of reminder in the survey the estimated willingness to pay decreases. This is because reminders are designed to mitigate hypothetical bias, that has been proven to exist in stated preference methods (hereunder the choice experiment method). Much literature reveals this to be true in the case of the cheap talk reminder, however, not much literature exists that evaluates the opt-out reminder, which is why this study is an important addition. Secondly, we expect that respondents state preferences that reveal spatial dependence, i.e. a distance-decay in willingness to pay. In this study, therefore, we expect that the willingness to pay in order to move wind turbines to distances further than 8 kilometers from shore, is lower for respondents who live far from the site in question than for the ones living close to it. In more general terms we could say, that peoples willingness to pay for a geographically fixed improvement decreases as respondents live farther away from this point.

I find that both the cheap talk reminder and the opt-out reminder effectively mitigate hypothetical bias, thus, the estimated willingness to pay estimates are generally lower when respondents have received reminders in the survey. Furthermore, I find that the results calculated from the sample that received an opt-out reminder reveal a willingness to pay that is lower than the ones calculated from the sample that received only a cheap talk reminder. These results seem to hold for both gender

groups, although spatial dependence in the male sample makes it more difficult to see. Note, however, that I don't test if these magnitudes are significantly different from one another, this will be accounted for in the text.

I also find that the spatial dependence, that we believe exists, is only picked up consistently when respondents are given both a cheap talk and an opt-out reminder in the survey. This result is very interesting and strongly suggests implementing the opt-out reminder together with the cheap talk reminder in future choice experiment studies. I have tested consistency of results by weighing the samples as one another by underlying demographic variables. That is, I have weighed one sample in order to represent another by background demographic variables, such as gender, income, education etc., i.e. if females were underrepresented relative to the reference sample, female respondents are given a larger weight than males. This way, I effectively portray having three samples that received each kind of survey design and I am able to test consistency. In general, the results, both in terms of level and structure of willingness to pay, seem to stabilize when both a cheap talk and an opt-out reminder are included in the survey. This indicates that less randomness is included in the responses when both these reminders are given - another strong argument to use both the cheap talk and opt-out reminder in choice experiment survey designs.

When examining how spatial dependence is picked up across gender, the results are not as clear. The male sample that received both the cheap talk and the opt-out reminder react very much to spatial differences. On the other hand, the females only react significantly to spatial differences in two out of three of the cases where sample weights differ and they received both a cheap talk and an opt-out reminder. This tells us that male and female respondents either perceive the survey design differently, i.e. the reminders are not effective at revealing the spatial preferences for females while they are for males, or that females don't have as strong spatial preferences as males and this effect, therefore, is more difficult to pick up. Perhaps the truth can be found from a combination of these two explanations. This study, however, suggests that there are differences in responses across gender and this is an issue that would be relevant to examine further. Nonetheless, this study provides an argument in favor of implementing the cheap talk and opt-out reminder in future choice experiment surveys.

Note that this study was implemented to examine methodical issues regarding choice experiments and survey designs in particular. The focus is, therefore, not on the magnitudes of the estimated willingness to pay, as would be the case if this study was implemented to help policy makers optimize decisions regarding offshore wind turbine placement. On the contrary, the results from this study, hopefully, will help to yield more trustworthy results from future choice experiments, that policymakers can then base their decisions on.

Acknowledgments

This thesis is written with Peter Birch Sørensen as supervisor. Additionally, Jacob Ladenburg (KORA) has delivered the data that is the basis for this study and, throughout the process, Jacob Ladenburg has effectively worked as a co-supervisor. His supervision has been part of his work as WP Leader in Project 1305-00021B under the Danish Council for Strategic Research (Wind 2050).

Contents

1 Introduction

In theory, the free market will ensure that society is in a state where welfare is optimized by minimizing deadweight losses, and thus maximizing welfare. However, this is only the case if the free market doesn't bring with it negative externalities distorting the optimal equilibrium for society, such that the true optimal equilibrium is, in fact, different from that established by the free market. In a western capitalist economy, the free market is in many cases the baseline scenario and receives an 'innocent-until-proven-guilty' treatment. Within this paradigm in which society operates, it is, therefore, of immense importance that qualified and trustworthy methods exist for valuing goods that are not traded in a market. When such methods exist, researchers have an opportunity to test some of the consequences that the equilibria found by the free market may have on welfare. In this study I wish to contribute to the understanding of this field. Specifically, I will examine the influence of differences in survey designs used in choice experiments on estimated results.

The present study builds on three different samples of choice experiment data related to the visual impact of offshore wind turbines. When collecting each of the three samples the survey design was marginally changed. I examine the differences in obtained results caused by these marginal changes in the survey design in order to establish which survey design is most trustworthy. Specifically, I investigate if the survey design catches the spatial effects that we believe exist in preferences. In addition, I examine whether or not the survey designs are effective across gender.

Specifically, an opt-out reminder[1] was included in the survey design when collecting one of the samples. Using an opt-out reminder in a choice experiment survey design is new to the literature, why this study is very relevant.

In this study, the primary interest is not the actual willingness to pay in order to place offshore wind turbines at different distances from shore. On the contrary, I take a methodical perspective. I choose, however, a very relevant case, especially from a Danish point of view. Denmark is a leader in wind energy and was the country with the greatest contribution (43 %) by wind to its electricity supply in 2015 (Sørensen et al. (2015)). Denmark passed a 'climate law' (Klima-, Energi-, og Bygningsministeriet (2014)) in 2014 which, among other things, sets the goal of the country having an energy supply based on renewable energy in 2050. Wind is expected to continue to be a dominating energy source in the Danish energy mix (Energinet.dk (2016)) throughout this period. Wind turbines are increasingly being placed offshore and a number of new offshore wind farms are already projected (Energistyrelsen (2016)). While it is technically possible to place wind turbines

[1] The specific marginal changes in the survey designs used to collect data will be thoroughly presented in Section 5 as well as what reminders are and the purpose of them.

so far off shore that they are not visible from land, increasing the distance from land also increases the price of establishing wind turbine farms offshore. Therefore, it is important to know how much the public, in general, values not being able to see wind turbines from shore. There is evidence that some individuals feel very strongly negatively about seeing wind turbines in the landscape and/or are concerned over other issues relating to wind turbines, i.e. noise generation, etc. However, there is a need for an understanding of how the Danish population, in general, values the placement of offshore wind turbines in order to optimize the national investment in offshore wind energy. The Danish Energy Agency recently (October, 2015) started a project designed to investigate the potential influence of offshore wind turbines on house prices in the coastal region nearest the wind turbines. The purpose of this project was to supplement the existing 'Loss Assessment Authority' (Taksationsmyndigheden (2016)), specifically in relation to offshore wind turbines.

In the next section (Section 2), I present a brief introduction to the underlying theory upon which this study is based. In section 3, I summarize previous findings. Both literature discussing the main findings regarding survey designs and previous studies regarding offshore wind turbines are included. Section 4 provides an overview of the statistical method used to derive my results, while Section 5 introduces the data that my results build on. The models estimated are presented in Section 6. First, a 'basic model' is presented in section 6.1, while Section 6.2 and 6.3 present models building on the one in Section 6.1. Section 7 summarizes and discusses the main results found in section 6. Finally, I conclude in Section 8 and give a few final remarks in Section 9.

2 Theory

In this section, a brief overview of some important and basic ideas regarding valuation of environmental goods are presented. It is important for the reader to be familiar with these concepts in order to grasp the main idea, and thus value, of this study. In the Section 2.1, the term 'total economic value' is introduced and, in Section 2.2, specific methods used to derive total economic value are presented.

2.1 Total economic value (TEV)

Many goods include other values than those that can be traded in a market. Defining and attempting to measure these non-traded values is important. In order to make socially optimal policy decisions, it is furthermore, important that the measured value of the non-traded goods precisely reflects the value that people attribute to it. A market values all traded goods by finding an equilibrium between supply and demand. When goods are not traded in a market setting, one might make the mistake of thinking that the value of the good is zero, but this, of course, is not the case. To make this point more clear consider, clean air. Clean air is (thankfully) not a good traded in a market setting and yet we value it very highly, as we depend on clean air to breathe. In environmental economics, we speak of the total economic value (TEV) of goods. The TEV includes a good's non-marketed value as well as its market value. For example, the TEV of a forest is not just the value of the wood, once it is cut up and sold in the market (the market value), but also the value of the clean air it produces while growing, the joy that people receive from visiting the forest and the beauty of the trees in the landscape, etc. The TEV is, therefore, made up of many values and these are split up into two categories - use-values and non-use-values. Use-values consist of the value one gets from actual direct and indirect use of the good in question. While walking in the forest, for example, is direct use, breathing the clean air the forest produces is indirect use. In addition, use-values consist of an option-value. The option-value is the value one gets from having the possibility to use the good. The possibility to use the good can be associated with oneself using the good, but it can also be associated with giving others the possibility (altruistic) or future generations the possibility (bequest value) of using the good. The non-use-value is the value one gets from the goods' existence. For instance, the existence of the rainforest might give me some comfort - not because I plan to visit or in other ways get use-value from it - but simply because it seems comforting to me that such a wild place still exists. In figure 2.1.1, an overview of total economic value is given.

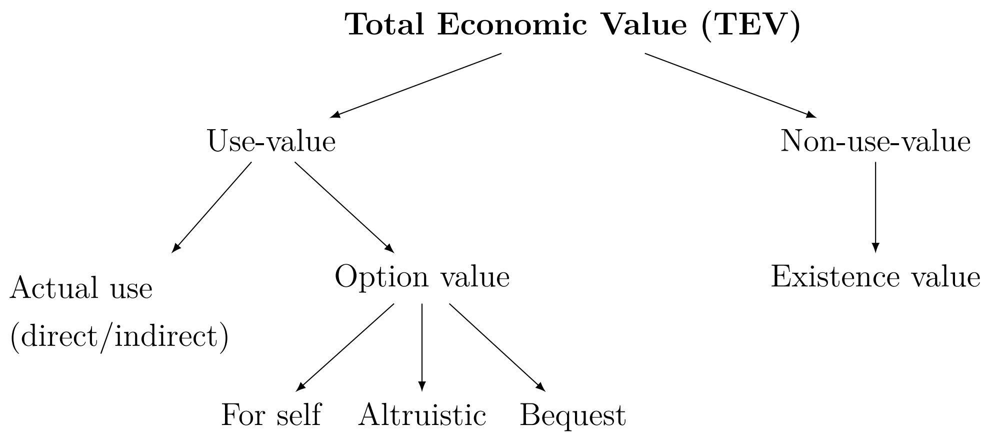


Figure 2.1.1: Total economic value

In this study, I examine how TEV can be calculated. Specifically, I look at how variation in a design used to extract the TEV is reflected in its calculations. I do this by calculating one of the non-traded values associated with establishing offshore wind turbine farms. More precisely, I calculate one of the non-traded values of potential wind turbine farms placed at varying distances from shore - namely the view of the ocean with a wind turbine farm present. The purpose of a study like this is to improve the calculation of the TEV of placing offshore wind turbines at different distances from shore (disturbing the view less when they are placed at greater distances from shore). Thus, such studies can help policymakers optimize according to the TEV rather than the market-value alone when making decisions. Note, though, that this particular study is more focused on methodical issues than the actual magnitudes of willingness to pay.

2.2 Valuation methods

There are different methods used to estimate the value of goods that are not traded in a market. These can be divided into two categories; revealed preference methods and stated preference methods.

Revealed preference methods exploit the fact that the value of some non-marketed goods may be reflected in a marketed good. A common example is examining the property market in order to find the value of a recreational area. If people value the recreational area, they will pay a more to live near it. So, after controlling for everything else, the excess value of the houses close to the recreational area relative to houses far away can be attributed to the presence of the recreational area. This method is known as the hedonic pricing method (Rosen (1974)). The travel cost method is also a method that has been used to determine the value of goods, i.e. examining the cost spent on traveling to a recreational area and letting this price reflect the value of the good. In some sense, the travel cost can be seen as an entrance fee, thereby revealing what users are willing to pay to enjoy the good. Other methods based on revealed preference include averting expenditure. Here researchers observe the costs that the population of interest pay in order to avoid disturbance from a given project. An example here could be the cost of building a sound limiting barrier around affected houses when a new highway is established. Notice that revealed preference methods are only able to measure use-values (figure 2.1.1). This is because non-use-values won't be reflected in the market good that portrays the non-marketed good. The stated preference methods, however, are able to estimate both the use and the non-use-values.

Stated preference methods are, as the name indicates, based on the preferences that are stated by the respondents. Within the stated preference framework, there are two types of methods: contingent valuation methods (CVM) and choice modeling methods (CMM). In a contingent valuation study, the respondents are presented with a discrete change of some good and asked whether or not they would pay for this change. This could be changing a field to a forest. The respondent, therefore, values the change in the good holistically and determines whether or not he/she is willing to pay for it. There are different variations of the contingent valuation method. For example, the price schedule can vary, i.e., in some cases, respondents are asked if they are willing to pay a given price (dichotomous) while, in other cases, respondents are asked what price they are willing to pay (open-ended). There are upsides and downsides to each of these approaches of revealing respondents' willingness to pay. On the one hand, asking for the exact price respondents are willing to pay (open-ended) may yield more specific results. On the other hand, respondents may have a hard time determining how much a good is worth if it is not a good that is frequently traded. Therefore, asking for the specific willingness to pay may give invalid results. However, there is as trade-off between each method used to ask for preferences and both methods may be used (Loomis (1990)). General for all stated preference methods is that the implemented survey design needs to be chosen with great care and, in some cases, may be dependent on the good being valued.

The choice modeling method builds on Lancaster's consumer theory (Lancaster (1966)). Lancaster's consumer theory utilizes the assumption that any good is made up of multiple attributes and each individual has preferences towards specific attributes. Daniel Mcfadden was the first to utilize this theory in order to set up a choice model (McFadden et al. (1973)). It can be explained by taking the example of a house. This house is placed in a given neighborhood, has a given number of rooms, is built in a certain architectural style and costs a given amount of money. If these four attributes are the only measurable characteristics of the house each attribute weighs in on the total utility an individual would get from choosing to purchase this exact house. The utility one individual gets or loses from a marginal change in some attribute varies by individual. Therefore, by asking respondents to choose the most attractive alternative, the researcher can measure the mean preference for a marginal change in a given attribute. When respondents are asked to choose the most attractive alternative, this method is known as the choice experiment (CE) method. Sometimes, instead of choosing the most attractive alternative, respondents are asked to rank or rate alternatives (contingent ranking/rating). By including a monetary variable as one of the attributes, the researcher is able to calculate the willingness to pay for the other attributes by looking at substitution rates between the attribute in question and the the monetary variable (as will be seen in Section 6.1). Note that the main difference between the CVMs and CMMs is that CMMs allow the researcher to estimate preferences for specific changes in given attributes. In the case of changing a field into a forest, this would allow the researcher to estimate the preferences for the size of the trees, their type etc. Also, it is common in a CMM study that respondents are presented with multiple choices, such that each individual is presented with different levels of the varying attributes (Hanley et al. (1997)). An overview of the relationship between the different valuation methods presented above is given in figure 2.2.1.

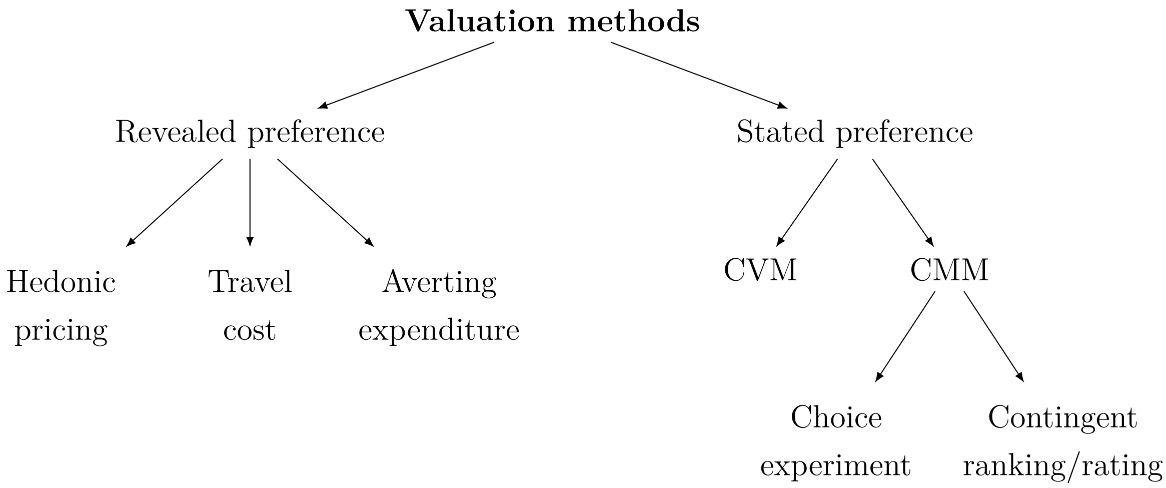


Figure 2.2.1: Valuation methods

The stated preference method allows the researcher to value goods that are non-existent. This means that policymakers can base their decisions about a future policy implementation on a factual background. On the other hand, and a major critique of the stated preference method, is that the hypothetical setting induces a hypothetical bias. This means that respondents recognize that the setting in question is actually hypothetical and, therefore, will give a hypothetical answer, which is not the same as the answer they would give were they actually faced with this decision in real life. Thus, both literature *for* the stated preference method (Carson (2012)) and *against* the stated preference method (Hausman (2012)) exists. The concern regarding hypothetical bias has been addressed by implementing different reminders in the design of the stated preference study. These are used in order to remind respondents to relate to the hypothetical setting as if it were a real life setting, thus urging them to answer truthfully. These reminders have been designed in different ways, and address different issues. Examples of reminders could be budget reminders, substitution reminders, consequential reminders, cheap talk reminders and opt-out reminders.

In environmental valuation studies, the CVM was implemented earlier than the CMM. Therefore, there is more experience regarding reminders in CVM studies than CMM studies, as will be reviewed in Section 3. Furthermore, some of the reminders are developed specifically for CVM, while others are specifically used in CMM studies. As the name indicates, the budget reminder is used to remind respondents to answer within their budget constraints. The substitution reminder is used to remind respondents that there exist substitutes for the good in question. If the respondent doesn't take into account that there, in a real world setting, may in fact be substitutes for the good being valued, he/she is likely to give an inflated willingness to pay response.

Consequential reminders (/scripts) are used to remind the respondent that their answer will have consequences for future policy decisions, thereby closing the gap between the hypothetical setting and the real world. The cheap talk reminder and the opt-out reminder (or the effect from these) will be a main area of focus in this paper and, therefore, will also be explained in further detail in the following. In essence, however, the cheap talk reminder is used to remind respondents that in a hypothetical setting it is common for respondents to state too large willingness to pay measures, and, therefore, respondents are asked to keep this in mind when they answer. The cheap talk reminder has been tested in both CVM and CMM studies. Likewise, the opt-out reminder is used to remind respondents before each choice that they should choose the opt-out reminder (the status quo) if prices for the other alternatives exceed what the household is able to pay. The opt-out reminder is specifically developed for CMM studies where respondents are given multiple choices to consider. In some senses, the opt-out reminder works as a budget constraint that is, because of the CMM setting, repeated for every choice. Note that different reminders exist and that one type of reminder can vary marginally between studies depending on the exact script used for its implementation.

In this study, I use stated preference data to determine the populations' preferences for placing offshore wind turbines at different distances from shore. A stated preference method is relevant in this case for several reasons. First off, if one wanted to do a hedonic pricing study, data would be scarce, both because of the limited number of offshore wind turbines but also because placement of the wind turbines often occurs in rural areas, thus limiting the number of houses affected. Perhaps, if the offshore wind turbine site is located near an attractive coastline for tourism, one could conduct a travel cost study. However, if one chose to use a revealed preference method, the offshore wind turbine farm in question either must already exist (and markets must have adapted) or an equivalent one must exist allowing for a benefit transfer. Furthermore, it is important to note, again, that the revealed preference method only measures use-values, while the stated preference method measures both use and non-use values. Therefore, in theory, the stated preference method does a better job at determining the true TEV than the revealed preference method does. However, admittedly, the non-use-values connected to the good in question in this particular study, perhaps are limited.

In this study, I specifically examine the methods used when collecting stated preference data and argue for ways to limit hypothetical bias, in order to generate the most trustworthy data. I hope that this study will give some insight to specific areas of stated preference methods - and, in particular, the choice experiment (CE) method.

3 Literature review

Valuation methods used to value non-marketed environmental goods have been around for a long time. CVMs were introduced before CMMs. The first example of a CVM study being used to measure an environmental good was Davis (1963) who looked at the willingness to pay for outdoor recreational areas and, as a pioneering study, it tries to do away with the thinking of its time:

> *"It is commonly charged that recreation values are 'priceless', that recreation is an esthetic pursuit having unique personal and spiritual values, that economic worth implies commercialization, and that economic processes serve only mass tastes. Such views are clearly erroneous in the present context and deserve refutation."* Davis (1963) p. 3.

Since then, CVM studies to measure values of environmental goods have increased in popularity, especially through the mid 1970s. Also in the 1970s, Mcfadden developed the CMM to be used in general economic work (McFadden et al. (1973)). An early example of an environmental choice experiment study, such as the one done in this paper, is Adamowicz et al. (1994). In this case, the authors compare a stated and a revealed preference method for the preferences towards recreational sites primarily on the basis of attributes regarding water quality and use.

3.1 Hypothetical bias

A major criticism of the stated preference method has been the presence of hypothetical bias. Hypothetical bias has been proven to be present when formulating hypothetical questions in order to find answers to real life questions (Blumenschein et al. (1998), Harrison et al. (2008)), although there is some evidence that choice based surveys reduce this bias (Murphy et al. (2005)). In order to minimize hypothetical bias, researchers have tried inducing more true answers from respondents by including reminders in their survey designs.

3.1.1 Use of reminders in contingent valuation studies

Already in the NOAA (National Oceanic and Atmospheric Administration) report of 1993 (Arrow et al. (1993)) describing which procedures to use when doing contingent valuation studies, substitution and budget reminders are mentioned. Later, Cummings and Taylor (1999) developed a script designed to mitigate hypothetical bias. This script is known as the 'cheap talk' reminder. In Cummings and Taylor (1999), the authors use a real referenda valuation that they compare to both a hypothetical valuation with and without a cheap talk script. They find that the null hypothesis, i.e. that the valuation with a cheap talk script is equal to the real referenda, cannot be rejected. Furthermore, they find that this is not the case for the hypothetical valuation without the cheap talk script.

Following Cummings and Taylor (1999), many more contributions have been made to the literature regarding cheap talk scripts in contingent valuation studies. In List (2001), sports cards are the base of the study. Here, it is shown that the cheap talk script is effective in mitigating hypothetical bias for inexperienced dealers, while hypothetical bias is still present with a cheap talk script when dealers are experienced. List (2001) concludes that the cheap talk script is not effective for certain customer types. Likewise, Murphy et al. (2005) find that the cheap talk script is effective in a contingent valuation study, but only for respondents facing larger payments. Bulte et al. (2005) expands on the cheap talk script, introducing a consequential script. The consequential script is similar to the cheap talk script but, instead of informing respondents that they must consider this situation as if they actually had to pay the money, it informs respondents that the results will be viewed by relevant policymakers and, therefore, may have a consequence for future policy.

Bulte et al. (2005) use a declining seal population in Waddenzee (a body of water north of the Netherlands) as the case and utilize a dichotomous price schedule to collect respondents' willing to pay in order to stop this decline. They vary the cause of the declining seal population between the respondents. The causes vary between: virus in the seal population (natural), climate change (caused by humans) and oil and gas drillings (caused by humans). First off, they find that the respondents who received a consequential script and the respondents who received a cheap talk script responded similarly, while both of these groups of respondents had a significantly lower willingness to pay than the ones that received no reminder. Secondly, Bulte et al. (2005) find evidence of an 'outrage effect', meaning that respondents reacted more (revealed a higher willingness to pay) when the fall in the seal population was caused by humans rather than nature. Landry and List (2007) also tested the hypothetical setting against a cheap talk and a consequential script and find both the cheap talk and the consequential script successful in eliminating hypothetical bias.

3.1.2 Use of reminders in choice experiment studies

The cheap talk reminder has been tested in choice experiment studies as well as contingent valuation studies. Carlsson et al. (2005) find that hypothetical bias is present in CE studies as well as in CV studies. Furthermore, the authors find that using a cheap talk script in their CE study significantly reduces the willingness to pay. Bosworth and Taylor (2012)

test the effect of a cheap talk script on the extensive margin and intensive margin, i.e. decision to enter the market at all (choice $\neq$ status quo) and choice of alternatives once in the market. They find that respondents who received a cheap talk script are more likely to enter the market than respondents in a real-life setting. However, once respondents who received a cheap talk script are in the market, they have a lower willingness to pay than when payments are real. Because these two effects work in opposite directions, when calculating the average willingness to pay, the payment in the real setting 'happens' to equal the payment estimated in the hypothetical setting with a cheap talk script.
Ladenburg and Olsen (2014) introduce the opt-out reminder in CE studies. They find that the opt-out reminder mitigates the hypothetical bias on the extensive margin; increasing the preferences for not entering the market, i.e. choosing the opt-out alternative. They also find that the cheap talk script is not as present in the respondents' mind, when they also are presented with an opt-out reminder. Thus, potentially, also removing hypothetical bias on the intensive margin.

3.2 Preferences towards offshore wind turbines

Previous studies have generally indicated that there are negative preferences towards offshore wind turbines that are visible from shore.

Ladenburg and Dubgaard (2007) build on a choice experiment study done on the Danish population (2006) addressing the visual impact of offshore wind turbines. In each choice set, respondents are given two alternatives where wind turbines are placed at hypothetical distances varying between 8, 12, 18 and 50 km from the coast. Respondents are not given an opt-out (/status quo) alternative. The wind turbines in question have a total height of 160 meters. The results from this study after taking into account an over representation of respondents with high education and middle and high income level, were that there was a willingness to pay of 246 DKK for moving wind turbines to 12 kilometers, 702 DKK for moving wind turbines to 18 kilometers, and 799 DKK for moving wind turbines to 50 km. All relative to moving wind turbines to 8 km. The payment schedule used was an annual fixed increase in the households' energy bill.

In Westerberg et al. (2011), choice experiment data were collected by interviewing tourists (summer 2010) in coastal areas of France. Respondents were asked to choose between 3 alternatives in each choice set. One of the alternatives is a status quo option, where nothing changes (i.e. no wind turbines). The other two alternatives include wind turbines placed at hypothetical distances of either 5, 8 or 12 kilometers off shore. The potential wind turbine farms consisted of 30 wind turbines, each with a total height of 133.5 meters. The payment schedule was a change in the weekly accommodation price, which could both take positive and negative values (willingness to accept/willingness to pay). In that study, tourists are split into different groups. Visitors and loyal local tourists are willing to accept wind turbines at 5 kilometers if the weekly accommodation price decreases by 22 EUR (164 DKK). At a distance of 8 kilometers, they don't percive a visual nuisance and, therefore, don't need to be compensated, while they are willing to pay 24 EUR (179 DKK) more per week if wind turbines are placed at 12 kilometers.

Krueger et al. (2011) collect choice experiment data (2006) from three different samples, in Delaware, USA; individuals living inland, individuals living in a bay area and individuals living by the ocean. Respondents are given three choice sets, each with three alternatives. In each choice set, there is a status quo option (rely on coal and gas - no wind) and two hypothetical options. In the hypothetical options, wind turbines are placed at either 0.9 miles (1.44 km), 3.6 miles (5.76 km), 6 miles (9.6 km), 9 miles (14.4 km) or 'too far to see' from the coast. The wind turbine farm in question consists of 500 turbines, each with a total height of 440 feet (135 m). The payment schedule is a monthly renewable energy fee for a period of three years. Krueger et al. (2011) find that the largest willingness to pay estimates are estimated from the sample of individuals living by the ocean while the smallest is estimated from the inland sample. Given that higher willingness to pay estimates are attributed individuals living near the ocean, a spatial effect (distance-decay) is found in Krueger et al. (2011).

Landry et al. (2012) studied the coastal impact of, among other attributes, offshore wind turbines in North Carolina, also using the choice experiment method. Data were collected via a telephone survey done in the summer of 2009. The study includes other coastal attributes than wind turbines. However, the wind turbines in question have a total height of 130 meters and the hypothetical distances they are placed from the shore are either 1 mile (1.6 km) or 4 miles (6.4 km). The status quo is that no wind turbines are erected. The authors only find significant (negative) preferences towards placing wind turbines 1 mile from shore.

Ladenburg et al. (2011) utilize part of the same data as are used in the present study, i.e. choice experiment data collected in 2006. The purpose of Ladenburg et al. (2011) was to estimate the willingness to pay for placing wind turbines at 12, 18 or 50 kilometers relative to the status quo, which is 8 kilometers. The wind turbines in question have a total height of 160 meters and are placed in wind turbine farms consisting of 100 turbines. In the Ladenburg et al. (2011) paper, two samples are collected, one that received no reminder and one that received a cheap talk reminder, in order to see if hypothetical bias can be mitigated by using a cheap talk script. As in the present study, they define an alternative-specific constant for choices different from the status quo ($\neq$ 8 km). From the sample that received no reminder, they estimate a willingness to pay of 167 DKK for the alternative-specific constant, 162 DKK for the 18 km attribute and 275 DKK for the 50 km attribute. However, the estimate for the alternative-specific constant was not significantly different from zero. From the sample that received a cheap talk reminder, they estimate a willingness to pay for the alternative-specific constant of 153 DKK, 63 DKK for the 18 km attribute and 233 DKK for the 50 km attribute. In this case, the estimates from the 18 km attribute and the alternative-specific constant are not significantly different from zero. Although differences in willingness to pay are not significant, there is an overall decrease in willingness to pay when the cheap talk script is included in the questionnaire.

Another recent paper that draws from part of the same survey that is used in this paper is Ladenburg and Knapp (2015). Specifically, the ct-sample, introduced in Section 5 in this study, is used. Ladenburg and Knapp (2015) show a distance-decay in willingness to pay for placing wind turbines at distances further from shore. This means that individuals living farther away from the given site are less willing to pay for placing wind turbines at distances further from shore. The present study finds the same result but expands on it in several ways, as will be seen. In Knapp and Ladenburg (2015) a review of studies regarding spatial preferences and wind turbines is given. The authors argue that biases in willingness to pay estimates may be present when spatial effects are ignored.

Bishop and Miller (2007) study how the perception of wind turbines changes at different distances when the weather changes or the underlying demographics of the sample changes. They find that young individuals have a more positive perception of offshore wind turbines in the scenery than old, both when wind turbines are placed at 4, 8 and 12 kilometers from shore.

In Ladenburg (2009), systematic differences in the perception of offshore wind turbines are investigated using the different samples. One sample represents the Danish population as a whole and two samples are drawn from areas where offshore wind turbines are a reality (Hornsrev and Nysted[2]). The authors find that previous experience with wind turbines generally give individuals a better perception of them compared to people who do not live close to offshore wind farms. It is shown that the Hornsrev sample has a significantly better perception of wind turbines than both the Nysted sample and the sample representing the Danish population as a whole.

4 Econometric method

In this section I will first present the conditional logit model. It is important to be familiar with this model in order to understand the econometric method used. Then, I will discuss limitations of the conditional logit model and, finally, how these limitations are dealt with in the more general mixed logit model, which is the model I use for estimation in Section 6. Note that this is a quite basic overview of the statistical methods used and is presented

[2]At Hornsrev wind turbines are located 14 km from the coast and at Nysted they are located 10 km from the coast.

in order to give the reader a general understanding of how estimates are derived. A more comprehensive description of both the conditional logit model, the mixed logit model and discrete choice modeling in general can be found in Train (2009), from which parts of this section are also drawn.

4.1 The conditional logit model

The general model builds on the utility function given by:

$$U_{nj} = V_{nj} + \epsilon_{nj} \tag{4.1}$$

In equation 4.1, U_{nj} is the utility that decision maker n gets from choosing alternative j. U_{nj} is split into two parts, namely, a part known to the researcher, V_{nj}, and a part unknown to the researcher, ϵ_{nj}. By assuming that ϵ_{nj} follows an extreme value distribution, the density and cumulative distribution of ϵ_{nj} are given by equation 4.2 and 4.3, respectively:

$$f(\epsilon_{nj}) = e^{-\epsilon_{nj}} e^{-e^{-\epsilon_{nj}}} \tag{4.2}$$

$$F(\epsilon_{nj}) = e^{-e^{-\epsilon_{nj}}} \tag{4.3}$$

This assumption concerning the distribution of the unobserved part of utility makes it possible to derive some information about the difference between utility from different choices because the difference between two terms that are extreme value distributed follows a logistical distribution. However, the key implication from this is that the ϵ's are independent of one another. This may seem restrictive and it is. I will return to this in section 4.1.1.

The logit choice probabilities, i.e. the probability that individual n chooses alternative i (where i denotes an alternative different from j), are given as:

$$\begin{aligned} P_{ni} &= Prob(V_{ni} + \epsilon_{ni} > V_{nj} + \epsilon_{nj}) \\ &= Prob(\epsilon_{nj} < \epsilon_{ni} + V_{ni} - V_{nj}) \end{aligned} \tag{4.4}$$

From equation 4.4, we see that individual n chooses alternative i if the difference in observed utility plus the unobserved part of utility from choosing alternative i is larger than the unobserved utility from choosing a different alternative than i. Because the ϵ_{ni}'s are independent, this can be evaluated as:

$$P_{ni} = \int \left(\prod_{j \neq i} e^{-e^{-(\epsilon_{ni}+V_{ni}-V_{nj})}} \right) e^{-\epsilon_{ni}} e^{-e^{-\epsilon_{ni}}} d\epsilon_{ni} \tag{4.5}$$

Equation 4.5 is the product of the individual cumulative distributions over all ϵ_{ni} weighed by its density and can be rewritten as[3]:

$$P_{ni} = \frac{e^{V_{ni}}}{\sum_j e^{V_{nj}}} \tag{4.6}$$

Note that, in equation 4.6, the scale parameter is set equal to one. This is common practice as we, when estimating probability models, are interested in the choice probabilities relative to one another and not levels of absolute utility. Scaling the utility of different choices by some arbitrary factor will still yield an equal relative size of utility between the choices. Therefore, this will not change behavior. It is, however, important to recognize that a more general equation representing the choice probabilities is given by equation 4.7, in which the scale parameter(σ) has been set to one in equation 4.6:

$$P_{ni} = \frac{e^{V_{ni}/\sigma}}{\sum_j e^{V_{nj}/\sigma}} \tag{4.7}$$

If utility is specified to be linear in parameters, which is usually the case, the choice probabilities are given by:

$$P_{ni} = \frac{e^{(\hat{\beta}/\sigma)' x_{ni}}}{\sum_j e^{(\hat{\beta}/\sigma)' x_{nj}}} \tag{4.8}$$

It is important to be aware of the scale parameter because $\hat{\beta}$ and σ cannot be identified separately and, therefore, what is estimated is $\hat{\beta}/\sigma$. Accordingly, the β estimated in the model is defined by equation 4.9:

$$\beta = \hat{\beta}/\sigma \tag{4.9}$$

Again, this doesn't make a difference for interpretation of the model itself because scaling utility from all choices doesn't change behavior. However, it does mean that models estimated from different samples are not directly comparable as it may not be reasonable to assume that the variance of the unobserved effects (scale parameter) are equal between samples. Thus, comparing parameter estimates' magnitudes from different models needs to be avoided.

[3] This derivation is somewhat complex and not of interest for this study, however, it can be verified on p. 74-75 in Train (2009).

The conditional logit model is estimated by maximum likelihood. The log-likelihood function is given in equation 4.10, where y_{ni} is equal to 1 only if individual n chooses alternative i, otherwise it is 0:

$$LL(\beta) = \sum_{n=1}^{N} \sum_{i} y_{ni} ln P_{ni} \tag{4.10}$$

Inserting equation 4.8 and 4.9 into 4.10 gives us:

$$LL(\beta) = \sum_{n=1}^{N} \sum_{i} y_{ni} ln \left(\frac{e^{\beta' x_{ni}}}{\sum_j e^{\beta' x_{nj}}} \right) \tag{4.11}$$

In equation 4.11, the value of β which maximizes the function is the estimated value. Thus, the name (maximum likelihood estimator), since it estimates the value that maximizes the (log)likelihood function.

4.1.1 Limitations of conditional logit

There are three general limitations of the conditional logit model; it doesn't allow random taste variation or correlation between unobserved factors over time and substitution patterns are fixed. All of these limitations are related to the assumptions that are made regarding the error term in the model.

That the conditional logit model doesn't allow for random taste variation means that it assumes that preferences are equal for all individuals. In reality, however, individuals may have personal preferences for given alternatives that the model then cannot capture. Thus, a limitation of the conditional logit model is that it does not take this into account.

When dealing with a panel data set, where every individual is presented with multiple choice sets, there may be correlation in errors (unobserved components) over time. In the conditional logit model, errors are assumed to be independent and the model, therefore, cannot handle this. This is, in fact, a quite crucial assumption and a strong limitaion of the model because we could easily imagine that there may be individual specific factors that the model does not capture.

The assumption of fixed substitution patterns is known as the assumption of independence of irrelevant alternatives (IIA). The IIA assumption states, as the name indicates, that the preference for one alternative should be independent of all other alternatives. In other words, the unobserved components, ϵ, between two alternatives must be uncorrelated. By making this assumption, our choice probability is given in a nice closed form (equation 4.6). However, the IIA assumption is not relevant at all times. Consider the classic example in the literature, the red-bus blue-bus example. Here, respondents choose between commuting by a red bus or by car. Thereafter, a blue bus option is offered. If the alternatives are truly independent of one another, then the relative choice probability between choosing to commute by car or by the red bus must stay the same. This means that, if we look at an individual who is indifferent with respect to travel by car or bus, i.e. 50 % probability for choosing either the red bus or the car, the relative choice probability in this case is 1 (0.5/0.5=1). In order for this to hold when we introduce the blue bus option, the blue bus needs to draw equally from the two probability masses. This means that, if the individual is also indifferent about the color of the bus, the model predicts the choice probabilities of all modes of transport being chosen with a 33 % probability. However, in real life, we would expect that the probability for choosing car still would be 50 %, while the probability of choosing either the red or the blue bus would be 25 % because the individual was indifferent between choosing either car or bus (independent of color). This problem presents itself because the choices of red bus and blue bus are correlated. In fact, they are almost identical and, therefore, the IIA assumption does not hold in this case.

4.2 The mixed logit model

In this study, I use the mixed logit model to estimate preferences. The mixed logit model is very general and, in fact, relaxes all of the above mentioned assumptions of the conditional logit model, i.e. it allows for taste variation between individuals, correlation of preferences over time and unrestricted substitution patterns. However, the mixed logit probabilities do not take a closed form as is the case for conditional logit probabilities. The mixed logit choice probabilities are the logit choice probabilities evaluated for specific values of β:

$$P_{ni} = \int L_{ni}(\beta) f(\beta|\theta) d\beta \tag{4.12}$$

Where, if we again assume that the utility is linear in parameters, the likelihood conditional on β is given by:

$$L_{ni}(\beta) = \frac{e^{\beta' x_{ni}}}{\sum_j e^{\beta' x_{nj}}} \tag{4.13}$$

Note that, if the researcher knows β and, therefore, is able to condition on it, equation 4.12 becomes equivalent to equation 4.8. However, the researcher does not know β and, therefore, instead assumes the shape of the underlying distribution, characterized by the parameter θ, that the β's are drawn from and integrate over this. For example, it would be reasonable to assume that the β-estimates for the number of rooms in a house would follow a normal distribution, given that some people like small houses and some people like big houses and that these preferences are distributed equally around some mean. On the other hand, the researcher wouldn't assume the price coefficient to be normally distributed as the normal distribution does not take strictly positive values. In the case of a price coefficient, the researcher might choose a log normal distribution, as this strictly takes positive values and we don't expect any respondents to get negative utility from money. Once the researcher has chosen the appropriate distributions, the model is estimated. The mixed logit model allows the parameter estimates of the random parameters to differ between respondents within the distribution, allowing for preference heterogeneity across respondents. Therefore, the researcher doesn't actually estimate single parameter estimates but, rather, the properties of the distribution of parameter estimates. For example, if the normal distribution is believed to fit a given parameter, the mean and standard deviation characterizing this distribution are estimated.

4.2.1 Estimation with MSLE

The mixed logit model is estimated by the maximum simulated likelihood estimator (MSLE). MSLE works like the maximum likelihood estimator, except that the β-estimates which are used in the optimization process are simulated based on given properties.

Essentially, in each step of the maximization process, R draws of β are made from an underlying distribution. For example, if the underlying distribution is assumed to be normal; $\beta \sim N(b, s^2)$, where b is the mean and s is the standard deviation, then the draws of β can be generated as:

$$\beta = b + s \times \eta \tag{4.14}$$

Where η is a random draw from a standard normal distribution. For given values of b and s, R draws of β are made and, for each draw, the likelihood is calculated from equation 4.13. The results from each of the R draws are averaged to find the simulated probability:

$$\bar{P}_{ni} = \frac{1}{R} \sum_{r=1}^{R} L_{ni}(\beta_r) \tag{4.15}$$

In equation 4.15, the subscript r denotes the r'th draw. The simulated log-likelihood is calculated from the simulated probabilities:

$$SLL(\theta) = \sum_{n=1}^{N} \sum_{j=1}^{J} d_{nj} ln \bar{P}_{nj} \tag{4.16}$$

In the following iterations of the maximization process, the parameters of θ are adjusted. In the case where the underlying distribution is normal, the mean, b, and standard deviation, s, are adjusted. Once again R draws of β are taken and the simulated log-likelihood calculated. The value of θ (which consists of b and s in the case of the normal distribution) that maximizes $SLL(\theta)$ is the *Maximun Simulated Likelihood Estimator* (MSLE).

5 Data

The data used in this study were collected in 2006 by Jacob Ladenburg for his Ph.D (Ladenburg (2007)). The data focus on the placement of offshore wind turbines. The wind turbines in question are 100 meters tall with a wingspan of 120 meters. Therefore, the total maximum height of one of the potential wind turbines is 160 meters. Respondents are informed that placement of the wind turbine farms will be done with consideration of both landscape and wildlife. Furthermore, respondents are given a map showing where off of the Danish coastline wind turbine farms are placed (at the time of the survey) and where potential sites could be. They are also told that the existence of current wind turbine farms doesn't exclude the possibility of placing new wind turbines in the same area. Therefore, in the map that respondents are given, both the areas which are pointed out be be potential sites and the ones that are existing sites are areas that could be possible sites for placement of the wind turbines that this survey is asking about. Respondents are also told that each of the proposed wind turbine farms will consist of 100 wind turbines.

Since 2006, of course, some of the proposed projects have actually been carried out. An up-to-date overview of which offshore wind turbine farms have been built since these data were collected and where existing offshore wind turbine farms were placed in 2006, can be found in figure 5.0.1[4].

A choice experiment design was created and sent to randomly drawn respondents via the internet. Respondents were asked to choose their preferred alternative out of three available options. Each collection of these three alternatives is called a choice set, and each respondent receives six choice sets. In other words, each respondent faces six decisions, which are each based on three different alternatives[5]. In the following, the 'number of observations' refers to the number of choice sets that have been answered. An example of a choice set is included in Appendix A.

Three different samples were collected. One where respondents did not receive a re-

[4]Note that the two farms placed in northern Jutland were not included in the map from the survey. The reason was that they are placed very near shore and consist of few turbines.

[5]Depending on the statistical program used to do the econometric modeling, this amounts to either 18 (one per alternative) or 6 (one per choice) lines of data per individual. The modeling done in this study has been done in Biogeme 2.3 (Bierlaire (2003)), where data are set up with one line per choice.

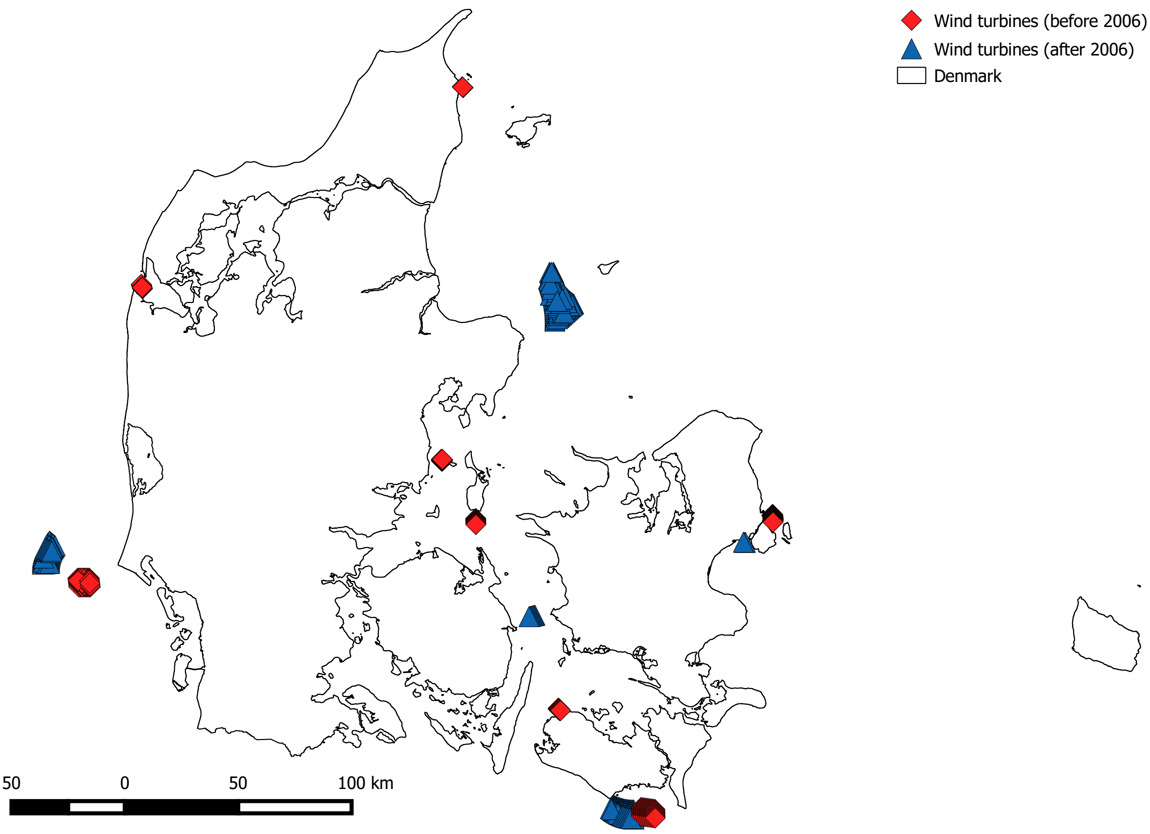


Figure 5.0.1: Offshore wind turbines before and after 2006

minder (nonct-sample), one where respondents received a cheap talk reminder (ct-sample) and one where respondents received both a cheap talk and an opt-out reminder combined (oor-sample). The questionnaires with no reminders (nonct-sample) were emailed to 623 potential respondents, ones with a cheap talk reminder (ct-sample) were emailed to 619 potential respondents and, finally, questionnaires with an opt-out and a cheap talk reminder (oor-sample) were emailed to 618 potential respondents. There were 386, who received no reminder, that completed the questionnaire; 355, who received a cheap talk reminder that completed the questionnaire and 365, who received both a cheap talk and an opt-out reminder, that completed the questionnaire.

The cheap talk reminder is given by including the following script at the beginning of the questionnaire:

> '*Please note that the annual cost to renewable energy is the cost that your household will have to pay, if the chosen alternative is implemented. Earlier studies on willingness to pay have shown that people tend to overstate their willingness to pay. Therefore, consider carefully how the annual extra costs will affect your budget, so that you are certain that you are willing to pay the annual cost associated with the*

alternative you choose.'

The opt-out reminder is given by adding the following script before each choice set:

> '*N.B.!!!!! If the amounts at alternativ 2 and 3 are larger than what your household is willing to pay, you should choose alternative 1*'[6]

Respondents were given follow-up questions. These are given in order to identify protest answers, etc. If the respondent answered that they thought that offshore wind turbines ought to be moved further away from shore but that they were not willing to pay for it or that they couldn't imagine paying a higher annual electricity bill, the individual was removed from the sample. This was done because, in these cases, the survey design wasn't successful in capturing the preferences from the given respondents, i.e. their choices were dependent on the design of the survey rather than their preferences. Furthermore, spatial data were missing for 20 respondents. These respondents were excluded from this study because a large part focuses on spatial relationships and, therefore, this is a very important variable. An overview of the original sample sizes and the sample sizes after removing protest respondents and respondents with missing spatial data is given in table 5.0.1.

Table 5.0.1: Sample sizes (number of observations in parentheses)

Sample	nonct	ct	oor
Questionnaire sent to	623	619	618
Completed responses	386 (2316)	355 (2130)	365 (2190)
Protest answers	19 (114)	17 (102)	12 (72)
Missing spatial data	5 (30)	8 (48)	9 (54)
Effective sample size	362 (2172)	331 (1986)	345 (2070)
Effective response rate	58.1 %	53.5 %	55.8 %

Number of observations are given in parenthesis (6 per respondent).
Note that there are 2 respondents that are both characterized as protest respondents and have missing spatial data (1 from ct and 1 from oor). Therefore, the effective sample size cannot be found by simply subtracting protest respondents and respondents with missing spatial data from completed responses.

Each of the samples are drawn randomly and, therefore, represents the Danish population. In Appendix B a map representing each sample is given. Note that the map includes the geographic locations of the wind turbine farms. This is done in order to relate each sample to this variable.

The monetary variable (or price variable) in each choice set varies. Specifically, it takes the values 100, 400, 700 or 1400 DKK. The payment vehicle was chosen to be an *annual fixed increase in the electricity bill* for the given household. Another option for

[6]Both the cheap talk and the opt-out scripts are translated from the original questionnaire, which was in Danish.

the payment vehicle was a marginal increase in the electricity price. However, after feedback from a focus group, it was decided that peoples' uncertainty about their electricity consumption would potentially make the results biased if this payment vehicle was chosen.

The distance for placement of wind turbines, different from the status quo, varies between 12, 18 and 50 kilometers from shore. This gives 12 (3×4) hypothetical scenarios from which a full-factorial design was constructed[7]. Following Kuhfeld (2005), the choice sets were chosen efficiently, i.e. such that the probability of attribute levels repeating themselves within choice sets is minimized (minimal overlap), the levels of a given attribute occur with the same frequency (level-balance) and that the joint occurrence between two levels in two attributes occur with the same frequency across different levels[8] (Huber and Zwerina (1996)).

In table 5.0.2, the frequencies of the choices are given. In each sample, the status quo option has been chosen about half of the time, while the remaining choices are split between alternative 2 and 3.

Table 5.0.2: Frequency of choices

	Alt. 1 (Status-quo)	Alt. 2	Alt. 3
nonct-sample	0.48	0.28	0.24
ct-sample	0.49	0.28	0.23
oor-sample	0.49	0.27	0.24

In the models, I focus, of course, on the variables describing the attributes. In each alternative, the offshore wind turbines are placed either 8 km (status quo), 12 km, 18 km or 50 km from shore, which is illustrated by photo shopped images in the choice set. Each alternative comes with a price, namely the price reflecting the rise in the respondents annual electricity bill. The distance variables are included in the models as dummies for 18 km and 50 km, and by an alternative-specific constant for a choice different from the status quo (see Section 6.1). In addition to the variables describing the alternatives, I focus on how preferences change spatially. In order to do this, I include a spatial variable, i.e. the travel time by car to the nearest potential offshore wind turbine site.

Since this study focuses on spatial differences in particular, it is important to be familiar with the spatial variable used. The spatial variable is given by driving time to the nearest potential wind turbine farm from the respondent's residence. Therefore, in the following analysis keep in mind that 'distance' is measured in minutes. Because it is, obviously, not possible to drive all the way to the actual wind turbines, 'fix-points' are chosen on the coastline nearest the wind turbine farm, and driving time is measured to

[7]Notice that the full-factorial design is possible due to the low number of attributes and attribute levels. Alternatively, one could have used a fractional-factorial design.

[8]Note that there is a trade-off between this and level-balance.

these ‘fix-points’. The distribution of the spatial variable across all samples can be seen in Appendix C.

From an interpretation perspective, it is important to recognize and remember that the respondents are not asked *if they want* wind turbines or not. On the contrary, the wind turbines in question are already a reality as an energy source and the respondents are merely asked at want distance they would prefer them placed. The distance at which the wind turbines are placed is assumed to be uncorrelated with their energy production. This is not mentioned explicitly in the questionnaire, however, it is mentioned that the cheapest way to produce energy from offshore wind turbines is by placing them close to shore. Therefore, the study is not estimating preferences towards green energy sources, per se, but instead preferences with respect to geographical placement of a the green energy source, i.e. offshore wind turbines.

In order to ensure that the unobserved part of utility varies due to differences in the reminders and not differences in the samples, I (in Section 6.1, 6.2 and 6.3) weigh the ct and the oor-sample as the nonct-sample based on demographic variables. The weighing is done in relation to the respondent’s age, income, school, education, gender, distance to closest potential wind turbine farm, distance to closest wind turbine farm and whether or not there are wind turbines in the area where the respondent resides. A technical description of the details in the weighting procedure is given in Appendix D. In addition, an overview of how descriptive statistics of demographic variables vary across samples is presented in Appendix E.

6 Models

The basic model (Section 6.1), which will be further developed in Section 6.2 and 6.3, is an attributes only model, containing preferences for choosing an alternative where wind turbines are placed 18 or 50 kilometers from shore, an alternative-specific constant and a price variable. The alternative-specific constant is a dummy variable taking the value one if the alternative is different from the status quo option. Therefore, the alternative-specific constant can be interpreted as the willingness to undergo a change. There can be resistance towards something different and new. This is known as ‘familiarity-bias’ and it can prevent the alternatives from being evaluated equally. In order to correct for this ‘familiarity-bias’, the alternative-specific constant is introduced (Scarpa et al. (2005)).

Note that I in the following refer to ‘random’ and ‘fixed’ parameters, this is the terminology when using a mixed logit model and refers to variables that vary across the population and, therefore, are estimated as a distribution and variables for which preferences are fixed across the population, respectively. Thus, a random parameter is, although the name may indicate otherwise, a clearly defined variable. However, preferences for this attribute vary 'randomly', within some estimated distribution, across the population.

I have tested different specifications of the models. First off, I have run models with various specifications of the distribution of the random parameters. I have found that the normal distribution seems to suit the random parameters best[9]. Secondly, I find that preferences vary across respondents, particularly for the alternative-specific constant and the 18 km attribute. However, preferences for the 50 km attribute are more stable across respondents, indicating that respondents do, in fact, agree on preferences for this attribute across the samples. Therefore, I model the 50 km attribute as a fixed variable rather than as a random variable. In contrast, preferences for the 18 km attribute and the alternative-specific constant vary. The distribution for the preferences of both of these attributes can be represented by a normal distribution.

In McFadden et al. (2000), it is shown that, if the appropriate distributions are chosen for random parameters, the choice probabilities of a model derived from random utility theory can always be estimated appropriately by a mixed logit model. In order to justify the choice of the normal distribution for both the preferences of the alternative-specific constant and the 18 km attribute, I have used a semi-parametric test proposed in Fosgerau and Bierlaire (2007). The idea is that the true distribution can be transformed to the assumed distribution and this transformation factor can be approximated. If the transformation factor is significantly different from 1, the null-hypothesis, i.e. that the assumed distribution is equal to the true distribution, is rejected. The transformation is approximated using three Legendre polynomials[10] to allow for sufficient flexibility (Fosgerau and Bierlaire (2007)). I have run models with the Legendre polynomials for both the alternative-specific constant and the 18 km attribute, and tested the distributions separately. Estimation of the models with the Legendre polynomials is carried out in Biogeme 2.3 (Bierlaire (2003)). The models with the Legendre polynomials are tested against the models without. Results from the likelihood-ratio tests are given in table 6.0.3.

By looking at the test scores (and respective p-values) in table 6.0.3, it can be seen that the null-hypothesis, i.e. that the preferences are, in fact, normally distributed for both the alternative-specific constant and the 18 km attribute, cannot be rejected for any of the samples.

The preferences for the price attribute are assumed to be fixed, i.e. the marginal utility of money is assumed to be the same across respondents. I acknowledge that this is perhaps a somewhat unrealistic assumption, as one could easily imagine that preferences for prices

[9]Below I include a test that confirms the normal distribution's fit on data.

[10]Therefore, the additional three parameters in the model that I test against the basic model in table 6.0.3.

Table 6.0.3: Semi-parametric test results of random parameters' distributions

Sample	Model	Number of parameters	Final log-likelihood	Test score (χ^2-dist.)	P-value
	basic	6	-1382.18	-	-
nonct	ASC-test	9	-1381.58	1.19	0.76
	d18-test	9	-1381.22	1.92	0.59
	basic	6	-1457.59	-	-
ct	ASC-test	9	-1456.94	1.30	0.73
	d18-test	9	-1456.93	1.31	0.73
	basic	6	-1642.51	-	-
oor	ASC-test	9	-1641.91	1.19	0.75
	d18-test	9	-1641.62	1.78	0.62

Note: The 'basic'-model is the model without the Legendre polynomials, i.e. the model run in the following section, and is the model which the others are tested against.

varied across respondents, possibly as a function of income, etc. However, this assumption is necessary in order to ease calculation of welfare effects from the different scenarios, i.e. calculate willingness to pay for each attribute. For that reason, this assumption is also made quite frequently in the literature (Revelt and Train (1998)) and has also been made in previous studies using the the data employed in this study (Ladenburg et al. (2011)).

All models are run in Biogeme 2.3 using 500 draws to simulate the distributions of the random parameters. There is no clear answer as to what number of draws is correct (Hensher and Greene (2002)). However, my results appeared to have stabilized at 500 draws[11]. The general code included in Appendix F is the code used to run the spatial model for the nonct-sample in Section 6.2. However, as described, this can easily be modified to estimate any of the models run in this section.

In the case of the optimization algorithm used in Biogeme 2.3 to optimize the simulated log likelihood function, the results from the models run in this study have proven to be less consistent when using the default setting. Therefore, a more consistent algorithm was chosen for optimization[12]. Finally, the model run on the female sample that received a cheap talk reminder in Section 6.3 was not able to converge at all using the default algorithm.

6.1 Basic models

As noted above, the first model is an attributes only model including an alternative-specific constant (ASC), an 18 km attribute (β_{d18}), a 50 km attribute (β_{d50}) and a price

[11]Furthermore, Biogeme utilizes Halton draws which have proven to be more efficient than regular random draws (Train (2000)).

[12]The default algorithm in Biogeme 2.3 is 'BIO'. The models are optimized with the 'SOLVOPT' algorithm which gave results consistent with the ones found using the 'CFSQP' algorithm. In addition, the 'SOLVOPT' algorithm generally seemed to converge faster than both 'BIO' and 'CFSQP'.

variable (β_{price}). The results are given in table 6.1.1.

Table 6.1.1: Estimation results of the basic model

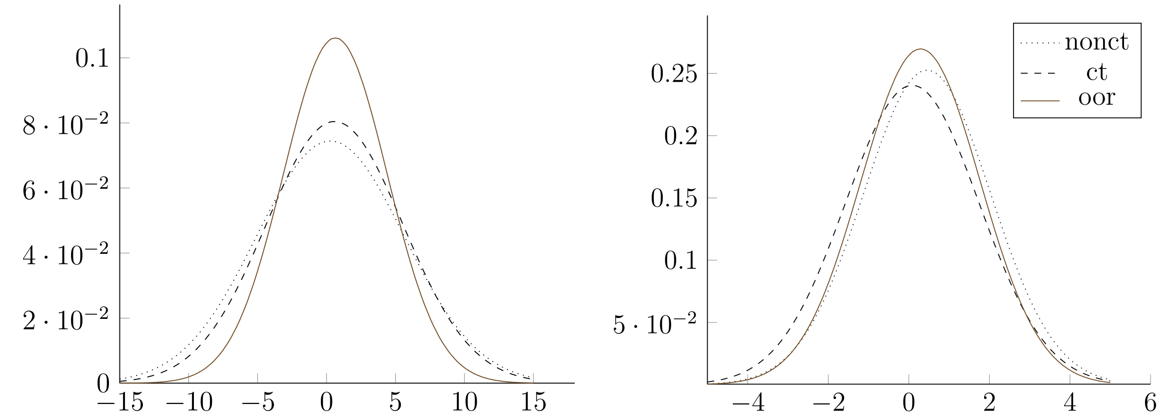
Variable	nonct sample Parameter estimate	nonct sample Std. error	ct sample Parameter estimate	ct sample Std. error	oor sample Parameter estimate	oor sample Std. error
ASC	0.280	0.308	0.582*	0.331	0.636**	0.239
β_{d18}	0.454***	0.150	0.085	0.165	0.291**	0.148
β_{d50}	0.789***	0.106	0.728***	0.112	0.668***	0.105
β_{price}	-2.62E-3***	0.159E-3	-2.92E-3***	0.183E-3	-3.01E-3***	0.174E-3
σ_{ASC}	5.36***	0.450	4.96***	0.412	3.76***	0.278
σ_{d18}	-1.58***	0.184	1.66***	0.208	1.48***	0.185
Number of observations:		2172		1986		2070
Number of individuals:		362		331		345
Number of draws:		500		500		500
Final log-likelihood:		-1382.715		-1249.809		-1412.597

Note: *** = significant on 99% level, ** = significant on 95% level and * = significant on 90%.

As explained in Section 4, when estimating a mixed logit model, the mean and standard deviation are estimated from the random parameters. Thus, a distribution of preferences is essentially estimated. Interpretation of the random parameters is more complex than whether or not the influence of a parameter is significantly different from zero[13]. Because the distribution of preferences across individuals is estimated, a mean that is not significantly different from zero can be the consequence of heterogeneity in preferences. This does not mean that the variable doesn't have an influence on preferences towards the alternative but, rather, that the preferences vary over the population. It can be the case that the preferences for a given variable vary in sign, such that some respondents find it to be a positive attribute while others the opposite. This causes the distribution of preferences to be on both sides of zero and makes it likely that the mean is not significantly different from zero. Therefore, in figure 6.1.1, I have drawn the normal distributions representing the spread of preferences across the different samples for the random parameters.

From table 6.1.1 and figure 6.1.1, we see that the standard deviations of the random parameters all are significantly different from zero. This is an acceptance of the hypothesis, that preferences for the different attributes vary across all the different samples for the two random parameters. The mean estimates are positive, meaning that more than half of the population considers moving wind turbines farther away from shore (alternative-specific constant) and, more specifically moving them 18 kilometers from the shore, as a positive

[13]Note that the price and the 50 kilometer attribute are fixed and therefore cannot vary across the population.



(a) ASC

(b) 18 km attribute

Figure 6.1.1: Preference distribution

improvement with respect to the status quo in all samples. Furthermore, the standard deviations are smallest for the estimates calculated in the oor-sample. This indicates that the respondents agree more on the effect of a change in the attributes when they receive an opt-out reminder in the questionnaire. This effect is also illustrated in figure 6.1.1, where the 'peaks' of the distributions calculated from the oor-sample are higher.

Also, from table 6.1.1, it is seen that the estimate of the 50 km attribute is significantly different from zero across all samples. In addition, the estimate of the price attribute is significantly different from zero and negative as expected across all samples. Furthermore, the estimate of the price attribute rises (in absolute value) from the model estimated on the nonct-sample to the ct-sample and from the ct-sample to the oor-sample. This, combined with the fact that the estimates for the 50 km attribute are largest for the nonct-sample and smallest for the oor-sample, indicates that the preference for this attribute decreases when respondents are given a reminder and, especially, when they are given the opt-out reminder. This is also seen from the willingness to pay calculations below and, specifically, the decrease in preferences from when respondents are given no reminder to when they are given a cheap talk reminder is in line with the results from Ladenburg et al. (2011). Furthermore, in general, this supports the hypothesis that the opt-out reminder (and to some extent the cheap talk reminder) can be used as a tool to mitigate hypothetical bias. Note also that, although alike, the results from Ladenburg et al. (2011) (included in Section 3) differ marginally from the ones found here even though they are produced from the same survey data. There are several possible reasons for this difference. Firstly, the sample is changed marginally, in that I have excluded respondents whose spatial data were missing. Secondly, the model specification differs between the two studies in that the 50 km attribute is estimated as a random variable in Ladenburg et al. (2011). Finally, a different optimization algorithm is used in Ladenburg et al. (2011) than the one used in Biogeme 2.3 for the present study.

From the parameter estimates, the willingness to pay for each given attribute can be derived for each of the different samples. The willingness to pay for an attribute (X) can be found by looking at substitution rates, i.e. it can be calculated as the parameter estimate from the variable of interest divided by the price parameter estimate and multiplied by −1, since the price parameter estimate is negative (because more money will always be better than less money, all else being equal):

$$\text{Willingness to pay} = -\frac{\beta_X}{\beta_{\text{price}}} \tag{6.1}$$

Note that, when calculating the willingness to pay estimates, the scale parameter (discussed in Section 4.1) cancels out and we are, therefore, able to compare estimates across models. This can be seen by combining equations 4.9 and 6.1, and seeing that the scale parameter, σ, cancels out as it is just a scalar present in both the numerator and denominator. Based on equation 6.1 and table 6.1.1, the willingness to pay estimates are calculated. These are presented in table 6.1.2.

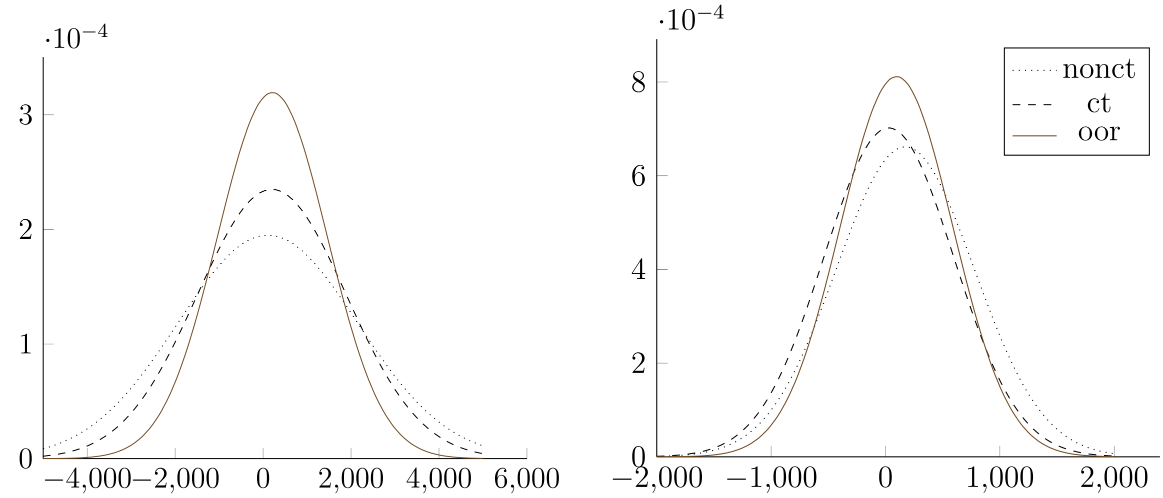
Table 6.1.2: WTP (in DKK) calculated from the basic model

Variable	nonct sample	ct sample	oor sample
ASC	106.9	199.3	211.3
σ_{ASC}	2045.8	1698.6	1249.7
β_{d18}	173.3	29.1	96.7
σ_{d18}	603.1	568.5	491.7
β_{d50}	301.1	249.3	221.9

At first glance, the results from table 6.1.2 may seem counter intuitive, given that the willingness to pay, in some cases, in fact increases from the sample that received no reminder to the sample that received both the cheap talk and opt-out reminder. Note, however, that since the alternative-specific constant and the 18 km attribute are random parameters, the willingness to pay is also given by normal distributions[14]. In figure 6.1.2, the willingness to pay distributions for both the alternative-specific constant and the 18 km attribute are presented.

As can quite easily be seen from figure 6.1.2, the willingness to pay distributions overlap greatly due to the rather large standard deviation around the mean estimate. Therefore, even though deviations in the mean willingness to pay is seen across samples, these differences are not significantly different from one another. Thus, it is not possible to claim that the mean willingness to pay to move wind turbines to a distance further than 8 km from shore (alternative-specific constant) is rising when reminders are added to the survey design.

[14]This is done by dividing both the mean estimate and standard deviation by the price estimate.



(a) ASC

(b) 18 km attribute

Figure 6.1.2: WTP distribution

Turning attention to the 50 km attribute, for which the preferences across the samples are fixed, we see a clear decrease from the estimates calculated from the nonct-sample to the ones calculated from the ct-sample and from the ct-sample to the ones calculated from the oor-sample. This is in line with the hypothesis, that respondents decrease their willingness to pay when they receive a reminder and, thereby, hypothetical bias is mitigated. In this case, reminders, both cheap talk and opt-out, can be used to mitigate hypothetical bias.

Note here, that there is, of course, still statistical uncertainty of whether or not the estimates from the 50 km attribute are different from one another, even though they are modeled as fixed variables. However, testing if these differences are significant is somewhat complex since each willingness to pay estimate consists of two parameter estimates with each their standard error. Furthermore, in the next section (Section 6.2) a spatial dimension is added complicating the calculation of willingness to pay estimates further. Because the primary focus in this study is whether or not spatial dependence is picked up, it is not central for the study to test if specific differences in willingness to pay are statistically significant.

6.2 Spatial models

By including the respondents' distance to the potential offshore wind turbine farms, it is possible to estimate how preferences change spatially. In the following model, I have included the distance to the potential wind turbine farms by interacting it with the variables of interest from the model presented in Section 6.1. From economic theory, we expect that individuals' willingness to pay for an improvement decreases as this improvement is located farther away geographically. This distance-decay in willingness to pay for geographical specific environmental goods is widely accepted and has been shown to exist several times before (e.g. Pate and Loomis (1997), Hanley et al. (2003), Bateman et al. (2005), Campbell et al. (2009) and He et al. (2015)). For both the alternative-specific constant and the 18 km attribute there weren't any significant spatial effects in any of the models. Therefore, I have left these two interacted terms out in the following model. The new variables in this model are the interacted distance terms for both the price ($\beta_{\text{price}\times\text{distance}}$) and the 50 km attribute ($\beta_{\text{d50}\times\text{distance}}$). Distance to the potential wind turbine farms is measured as driving time from the residence of the respondent, which was also more thoroughly discribed in Section 5[15]. By including the spatial variable as an interaction term in this way, I am able to estimate the preferences for the given attribute conditional on the distance to the site.

I have also tried estimating models with the distance variable being driving time to the actual offshore wind turbine farm (and not potential sites), without getting a good fit. Perhaps some of the explanation for this can be found in Ladenburg (2009), where it is shown that familiarity with offshore wind turbines decreases the dislike for them. Likewise, I have tried using log-transformed driving time to both actual and potential offshore wind turbine farms. The reason why I thought this might yield a better fit was that I expected the marginal utility loss from having wind turbines close to shore would decrease with the distance. However, it turned out not to give a better fit and, therefore, I retained the non-transformed 'driving time to potential wind turbine farms' as the spatial variable in the model. I do still believe that there is some non-linearity in the marginal loss of utility as respondents move further away from the site, i.e. the marginal loss of utility is larger between respondents located between 30 and 15 minutes away than it is between respondents located between 90 and 75 minutes away. One explanation for why the non-transformed spatial variable still gave the better fit can perhaps be the natural non-linearity in the variable itself. Because the distance is measured as *travel time*, individuals living far away will actually have a relatively short distance to travel due to straight motorways with high speed limits while individuals living closer will have a relatively long traveling distance because all of their traveling is done on smaller roads.

The results from the spatial model are shown in table 6.2.1.

The estimation results from table 6.2.1 indicate, that only the opt-out reminder catches the spatial effects in the price-variable. This is surprising as we expect that this effect exists, i.e. that people are less willing to pay for improvements that are not located geographically close to them. The spatial effect in the 50 km attribute is - interestingly - caught in the sample that wasn't given a reminder at all (nonct-sample). It is quite

[15]Note that because this variable is of particular interest, the distribution across samples is included in Appendix C.

Table 6.2.1: Estimation results of the spatial model

	nonct sample		ct sample		oor sample	
Variable	Parameter estimate	Std. error	Parameter estimate	Std. error	Parameter estimate	Std. error
ASC	0.342	0.309	0.601*	0.333	0.686***	0.243
β_{d18}	0.491***	0.151	0.083	0.165	0.293*	0.149
β_{d50}	1.11***	0.221	0.915***	0.235	0.802***	0.210
$\beta_{\text{d50}\times\text{dist.}}$	-4.47E-3*	2.67E-3	-2.58E-3	2.84E-3	-1.76E-3	2.64E-3
β_{price}	-2.26E-3***	2.95E-4	-2.53E-3***	3.38E-4	-1.69E-3***	3.03E-4
$\beta_{\text{price}\times\text{dist.}}$	-5.43E-6	3.66E-6	-5.51E-6	4.14E-6	-2.03E-5***	4.36E-6
σ_{ASC}	5.31***	0.438	4.97***	0.439	3.77***	0.277
σ_{d18}	1.59***	0.191	1.66***	0.208	1.50***	0.190
Number of observations:		2172		1986		2070
Number of individuals:		362		331		345
Number of draws:		500		500		500
Final log-likelihood:		-1379.177		-1248.164		-1398.832

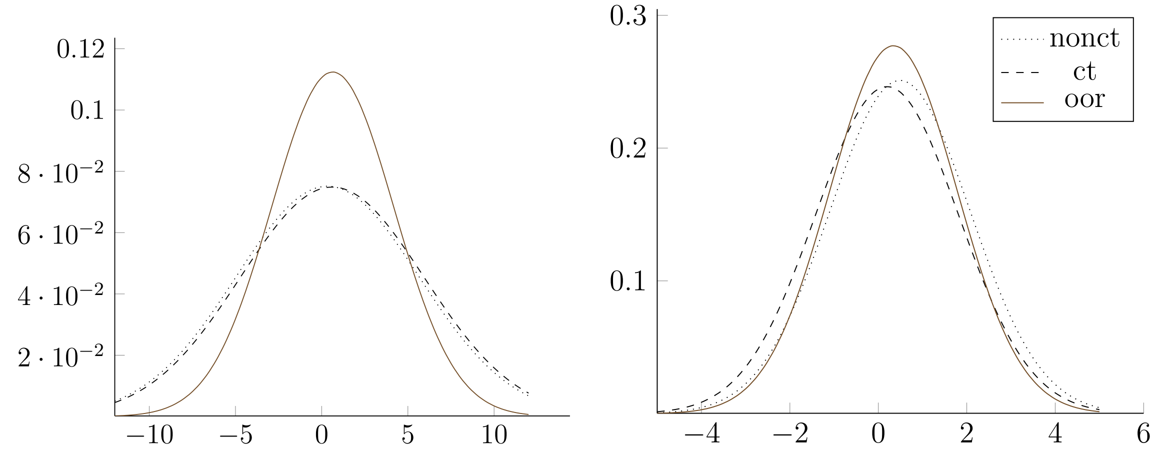
Note: *** = significant on 99% level, ** = significant on 95% level and * = significant on 90%.

interesting that the spatial relationship is picked up in this manner. What this essentially tells us is that preferences to move wind turbines out to 50 kilometers decrease when respondents live farther away, ceteris paribus (i.e. controlling for price). Though this spatial dependence may also be intuitive, it is worth realizing that this is a different spatial dependence than the one caught in the price variable. The spatial dependence in the 50 km attribute can perhaps be explained by respondents being indifferent about placement of the wind turbines when they live far away. However, it is unexpected that only this effect is caught, since we would expect the spatial dependence in the price variable to be the dominant spatial dependence. Note that the interacted term for the 50 km attribute and distance in the nonct-sample is significant at a 90 % confidence level. In contrast, the interacted term for price and distance is significant at a 99 % confidence level for the oor-sample. Thus, the spatial relationship with the price-variable is very clear in the oor-sample.

The parameter estimates are significantly different from zero at a 99 % confidence level for the 50 km attribute across all the samples. In addition, the estimated standard deviations of the alternative-specific constant and the 18 km attribute are significantly different from zero across all samples.

The price variable is significantly (99 % confidence level) different from zero and with negative sign across all samples. After receiving both the cheap talk and the opt-out reminder (oor-sample), respondents take into consideration how far away the good is that they are paying for and adjust their preferences in accordance to this. Furthermore, we see that the parameter estimate for the interacted price variable is negative, such that the respondents' willingness to pay for an increase in one of the attributes decreases with respondents distance from the offshore wind turbines. This makes sense and is in line with what we would expect from economic theory.

The estimated distributions of the parameter estimates for the random parameters are illustrated in figure 6.2.1.



(a) ASC

(b) 18 km attribute

Figure 6.2.1: Preference distribution

From figure 6.2.1 we see, once again, that the peaks are higher for the distributions estimated from the oor-sample. This can, agian, be interpreted as the respondents who received both the cheap talk and the opt-out reminder agree more on their preferences than respondents from the other samples. Note that figure 6.2.1 is included for illustrative purposes and the same conclusion can be drawn from looking at the estimated standard deviations in table 6.2.1.

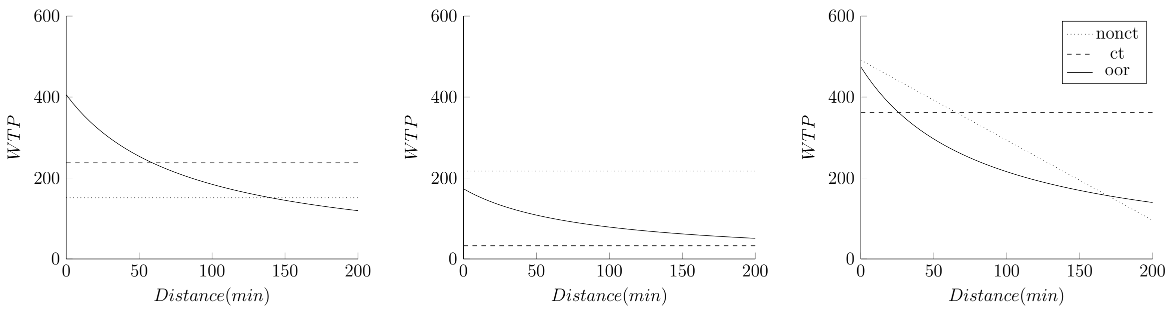
Calculating the willingness to pay from these estimates is more complex than in Section 6.1. This is, once again, because the preferences for price and the 50 km attribute are (in some cases) dependent on the distance variable. Therefore, the willingness to pay is not a static measure but, in fact, evaluated conditional on distance. Additionally, because we calculate willingness to pay by dividing with the estimated price parameter which is now dependent on distance, this adds a new dependency. However, the real problem occurs when we need to calculate willingness to pay for the attributes that are modeled by random parameters. Again this is done by dividing the mean and standard deviation with a price estimate. As the price estimate is now dependent on distance (oor-sample) we are, in fact, looking at a distribution with a shifting mean as well as a shifting standard deviation. Not only does this complicate calculation of willingness to pay, it also complicates comparison of willingness to pay across samples. I will focus on the mean willingness to pay in order to compare results across samples. However, it is important to keep in mind that we, for the alternative-specific constant and the 18 km attribute, are only looking at the means from underlying distributions of willingness to pay.

I calculate willingness to pay using equation 6.1 from Section 6.1. Now, however, this function may be dependent on distance. We can, therefore, rewrite equation 6.1 to include this. This is done in equation 6.2[16]:

$$\text{Willingness to pay} = -\frac{\beta_X + \beta_{\text{X}\times\text{distance}}\times\text{distance}}{\beta_{\text{price}} + \beta_{\text{price}\times\text{distance}}\times\text{distance}} \tag{6.2}$$

In equation 6.2, $\beta_{\text{price}\times\text{distance}}$ and $\beta_{\text{X}\times\text{distance}}$ represent the parameter estimates of the interaction terms with distance of the price attribute and the attribute in question, respectively. It is important to note that I treat $\beta_{\text{X}\times\text{distance}}$ and $\beta_{\text{price}\times\text{distance}}$ as being different from zero if they are significantly different from zero at a 90 % confidence level. Therefore, from the figure produced from these calculations (figure 6.2.2) it is not possible to differentiate between the confidence levels (90, 95 and 99 %) of the spatial terms. I do this consistently throughout the rest of this paper.

In figure 6.2.2a, I have plotted the estimated mean willingness to pay conditional on the distance variable for the alternative-specific constant. Note that the x-axis represents the distance and is given by the approximate distance interval from which respondents were located in the samples, i.e. [0;200) minutes (see Appendix E and C).



(a) ASC *(b)* 18 km *(c)* 50 km

Figure 6.2.2: Willingness to pay estimates (in DKK)

In figure 6.2.2a, we first notice that, for the oor-sample, the mean willingness to pay is given by a convex curve, while for the nonct and ct-sample it is represented by horizontal lines. This is due to non-spatial dependency in the models estimated on both the nonct and ct-sample. Furthermore, it is interesting to note that the mean willingness to pay for an attribute different from status quo (ASC) is actually higher when respondents have received a cheap talk reminder than when they have received no reminder at all. This

[16]Note that $\beta_{\text{X}\times\text{distance}}$ is only relevant when X = the 50 km attribute.

is not what we would expect, given that the cheap talk reminder is used to alleviate upwards hypothetical bias and therefore is expected to adjust willingness to pay downwards. However, what figure 6.2.2a doesn't show is that the standard deviations associated with these results are very large. Therefore, one shouldn't take the results too literally. The same is, of course, an issue when looking at the willingness to pay calculated from the oor-sample. However, because of spatial dependence in the estimates calculated from the oor-sample, we see that the willingness to pay is actually higher than both the ones calculated from the nonct and ct-sample for respondents who live very close to the potential sites, while it is lower for those living far away. The willingness to pay calculated from the oor-sample crosses the willingness to pay calculated from the ct-sample at 59 minutes and the nonct-sample at 140 minutes.

Figure 6.2.2b shows the 18 km attribute plotted in the same manner as in figure 6.2.2a. In this figure we see, once again, that the willingness to pay calculated from the nonct and ct-sample is independent of distance. However, now the willingness to pay is higher for the nonct-sample. This is what we would expect given that the upward hypothetical bias is not dealt with in the nonct-sample. Note that the standard deviation on these distributions is still quite large, although much lower than those calculated for the alternative-specific constant[17]. The willingness to pay calculated from the oor-sample is represented by a convex curve that is lower than the willingness to pay calculated from the nonct-sample for any given distance. It is also higher than willingness to pay calculated from the ct-sample at any given distance. However, again, we must interpret this result with some caution as the functions in figure 6.2.2b are actually distributions where only the mean has been plotted, and the associated standard deviation is omitted.

Finally, in figure 6.2.2c, the willingness to pay estimates are plotted for the 50 km attribute across the three different samples. Note that we are now looking at actual willingness to pay estimates rather than means plotted from many different distributions as in figures 6.2.2a and 6.2.2b. This, of course, is because the 50 km attribute is modeled as a fixed variable. Interpretation of figure 6.2.2c is, therefore, more straight forward than for the two previous figures. Note that, while willingness to pay for the 50 km attribute calculated from the oor-sample is still given by a convex curve, the willingness to pay calculated for the nonct-sample is now also dependent on distance. This dependency comes from the interaction between distance and the 50 km attribute ($\beta_{\text{d50}\times\text{distance}}$) being significant and is illustrated by a linear negative dependency. Once again, no significant spatial dependence was found in the sample that received only the cheap talk reminder. Willingness to pay is, therefore, again expressed by a horizontal line. The willingness to pay calculated from the oor-sample is lower than the one calculated from the nonct-sample up until respondents live farther than 169 minutes away. However, once again,

[17]This can be verified from table 6.2.1

it is important to bear in mind that the spatial dependence comes from two different sources, namely a decrease in the willingness to pay for improvements that occur far away (oor-sample) and a decrease in the (positive) preferences towards the 50 km attribute for people who live far away (nonct-sample). The willingness to pay calculation and comparability is still theoretically correct but it is worth making clear that the spatial dependence that one would believe to be dominant, i.e. that respondents willingness to pay for geographical improvement decreases as they live farther away, is not caught in the nonct-sample.

The willingness to pay calculated for the ct-sample starts out lower than for both the nonct and oor-sample. However, as no spatial dependence is captured in this case, a higher willingness to pay is estimated for respondents living far from the wind turbines than both the ones calculated from the nonct and oor-sample. The willingness to pay calculated from the ct-sample exceeds the one calculated from the oor-sample when respondents live farther than 26 minutes away and likewise exceeds the one calculated from the nonct-sample when respondents live farther than 65 minutes away.

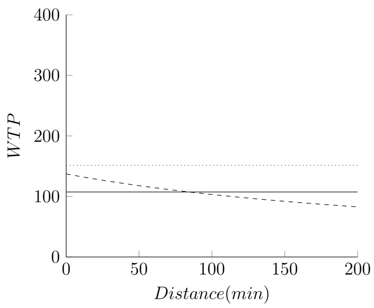
6.2.1 Spatial models with varying sample weights

When we draw a random sample in order to represent a whole population, we want, of course, the specific sample to truly represent the underlying population. In choice experiments, we want the estimated preferences to be representative for the population as a whole and not to depend on the specific random sample. Whether or not this is the case is difficult to test. Nevertheless, I will address this issue in this section.

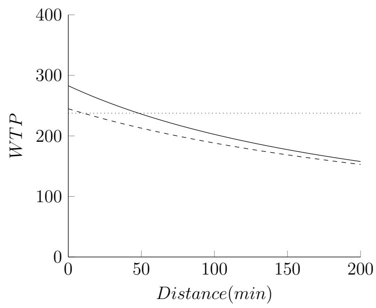
I focus here on how reminders in choice experiments influence the robustness of the estimated preferences when the underlying background sample is marginally changed. In the models run up until this point I weighed all samples as the nonct-sample given some background variables (this is also mentioned in Section 5, while the technical details of the weighting procedure are explained in Appendix D). Instead of weighing samples as the nonct-sample, in this section, I weigh the samples marginally different, run the spatial models and compare results for respondents who received the same reminder but where the weights have been marginally changed. In this way, it is possible to investigate if the results are robust over for marginal changes in the underlying random sample.

The samples are weighed as one another, i.e. the nonct-sample is run without externally specified weights (already done in section 6.2), with weights representing the demographics of the ct-sample and with weights representing the demographics of the oor-sample. A similar procedure was applied for the ct and oor-sample, respectively. Again, I will refer to Appendix D for a more comprehensive description of how weight are found. Note that I utilize an additional element in the data by assigning weights in this fashion, namely, the fact that these samples have been randomly chosen. Therefore, all samples are expected to represent the underlying population they have been chosen from, i.e. the Danish population as a whole. If I, instead, had generated random weights, this property of the data would have been lost in the following analysis, which would accordingly have lost strength.

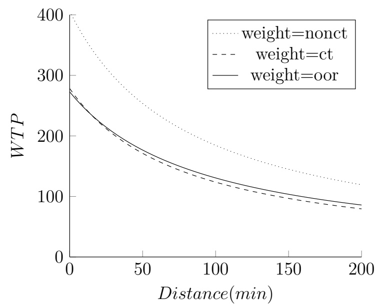
In figure 6.2.3, 6.2.4 and 6.2.5, the willingness to pay for the respective attribute is illustrated as a function of distance with the sample weights given in three different ways as noted above. Note again that the willingness to pay for the alternative-specific constant and the 18 km attribute are, in fact, represented by distributions and the figures only represent the means from these. The sub figures, from the figures mentioned above, represent the respondents who have received no reminder (nonct-sample), received only the cheap talk reminder (ct-sample) and received both the cheap talk and opt-out reminder (oor-sample), respectively. As it was too extensive to include the full table of model results from every model in the text, the results from the models run with sample weights representing the ct and oor-sample can be viewed in Appendix G, while the ones with sample weights representing the nonct-sample are presented in table 6.2.1. Note in this respect that I, again, treat a 90 % significance level as *significant* as was also mentioned in section 6.2.



(a) nonct



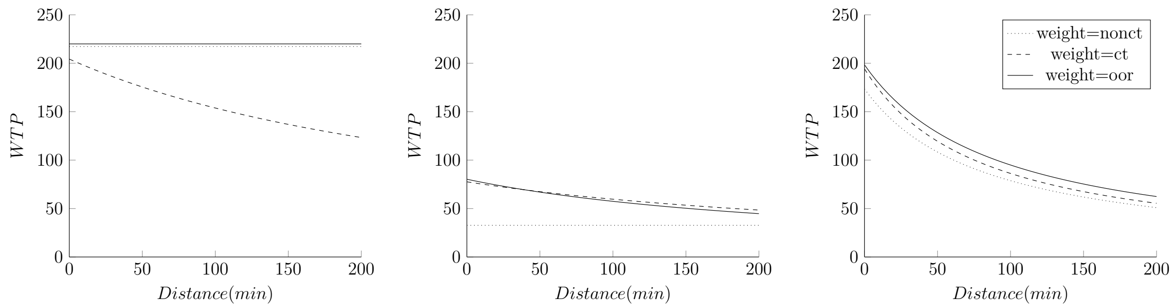
(b) ct



(c) oor

Figure 6.2.3: Willingness to pay estimates for the alternative-specific constant with different sample weights (in DKK)

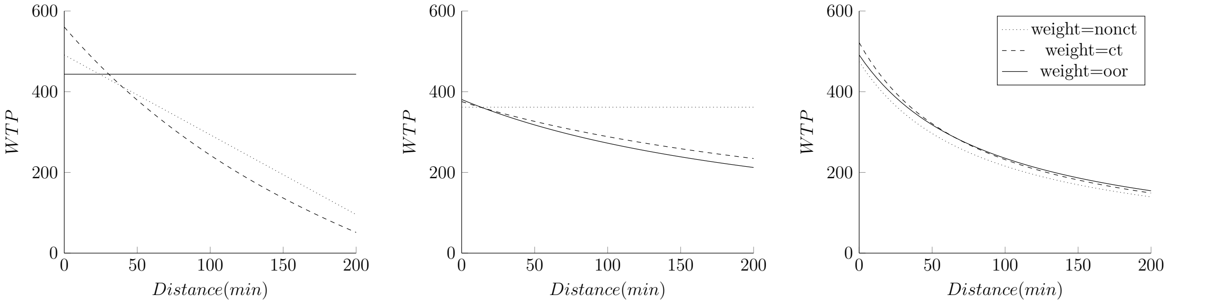
The willingness to pay estimates for the alternative-specific constant are presented in figure 6.2.3. Looking first at the estimates from the respondents who received no reminder (nonct), it is seen that when the sample weights are chosen to represent the ct-sample, the model picks up some spatial dependence. As can be verified from the results in table G.1 in Appendix G, this spatial dependence comes from the price variable. This explains the convex nature of the willingness to pay estimates for the nonct-sample, weighed as the ct-sample, present in all attributes. Furthermore, the willingness to pay level varies somewhat. However, once again, bear in mind that we are looking at only means from underlying distributions with quite large (relatively speaking) standard deviations when we evaluate the alternative-specific constant and the 18 km attribute. For the respondents who received the cheap talk reminder, the model picks up spatial variation in the price variable both when the sample weights are chosen to represent the ct and oor-sample. Looking at the respondents who received both the cheap talk and opt-out reminder, the model picks up spatial variation in the price variable no matter which sample weights are chosen. Thus, as is seen, the structure of the means of the willingness to pay distributions as a function of distance is the same when sample weights vary.



(a) nonct *(b)* ct *(c)* oor

Figure 6.2.4: Willingness to pay estimates for the 18 km attribute with different sample weights (in DKK)

A figure equivalent to figure 6.2.3 but for the means from the willingness to pay distributions of the 18 km attribute is given in figure 6.2.4. For the respondents who received no reminders, the estimated means are almost exactly equivalent when the sample weights are chosen to represent the nonct and oor-sample. When the sample weights are chosen to represent the ct-sample, spatial variation is picked up and the size of the means decrease with distance. Likewise, for the respondents who received the cheap talk reminder, when sample weights are chosen to represent the ct and oor-sample, the estimated means are almost equivalent, while when they are chosen to represent the nonct-sample, they are spatially independent and at a lower level. For respondents who received both the cheap talk and the opt-out reminder, the estimated means for a given distance keep the same structure and level.



(a) nonct *(b)* ct *(c)* oor

Figure 6.2.5: Willingness to pay estimates for the 50 km attribute with different sample weights (in DKK)

Finally, in figure 6.2.5, the willingness to pay estimates for the fixed 50 km attribute are presented. Considering first the respondents who received no reminder, it is seen that the structure of the willingness to pay given distance is completely dependent on which sample weights are chosen. When the sample weights are chosen to represent the oor-sample, there is no spatial dependence (horizontal line). When they are chosen to represent the nonct-sample, spatial variation is caught only in the 50 km attribute and, finally, when they are chosen to represent the ct-sample, spatial dependence is caught both in the 50 kilometer attribute and in the price variable (this can be confirmed by looking at table G.1 in Appendix G). Looking at the willingness to pay estimates from respondents who received a cheap talk reminder, we see that, when sample weights are chosen to represent the ct and oor-sample, they have the same structure and the same level. In contrast, when sample weights are chosen to represent the nonct-sample, there is no spatial dependence (as also seen in Section 6.2) and the level of willingness to pay is, therefore, higher for distances far away. Finally, considering respondents who received both a cheap talk and an opt-out reminder, the willingness to pay estimated for the 50 km attribute is almost exactly the same regardless of how the sample weights are chosen.

6.3 Gender specific spatial models

The spatial results presented in Section 6.2 represent the entire population. In some cases, it may be relevant to determine whether such results vary when we focus on specific segments of the population. In this section, I use the same model specification as in Section 6.2 but split the samples up by gender. The question that I wish to address here is whether spatial preferences vary across gender and if male and female respondents react similarly to the different reminders. Other, more general, examples exist of how gender differences, in some cases, influence preferences for environmental goods (e.g. Stern et al. (1993), Burger et al. (1998) and Dupont (2001)). The issues I wish to address here are ones that can play a role in choice experiment studies from a sample selection perspective but I also want to further examine the robustness of results produced from data where reminders in the survey designs differ. This is potentially also of more general interest as differences between male and female preferences and reaction to reminders can help to identify how, and if, the cognitive process differs between men and women. In fact, spatial differences across gender are also of particular interest in psychological studies as this is one of the few places where gender researchers have found evidence of cognitive differences between the genders (Hyde (2005), Lawton (2010)).

In this section I first, briefly, present the spatial models run on the male and female samples separately. Then I compare the results, in order to specify how responses vary by gender. Finally, I run the gender specific models again with varying sample weights, as in Section 6.2.1 for the full samples, in order to determine how robust the results found are.

6.3.1 Spatial model on the male sample

The results from the models estimated from the male sample are given in table 6.3.1.

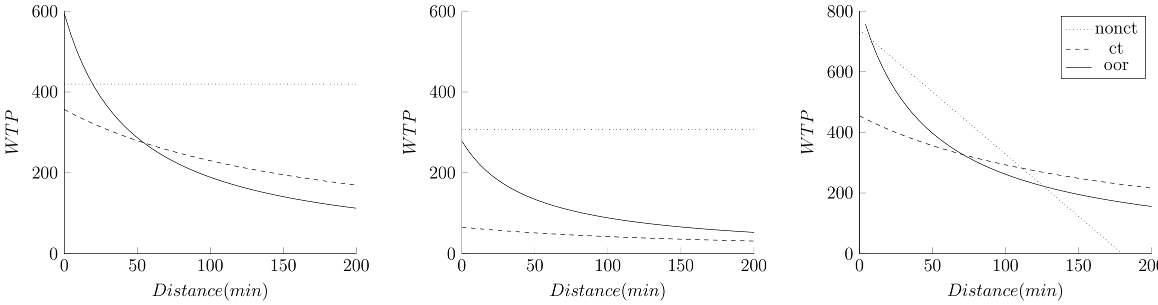
Table 6.3.1: Estimation results of the spatial model based on male sample

	nonct sample		ct sample		oor sample	
Variable	Parameter estimate	Std. error	Parameter estimate	Std. error	Parameter estimate	Std. error
ASC	0.818	0.495	0.653	0.413	0.713**	0.341
β_{d18}	0.600**	0.215	0.120	0.239	0.334*	0.196
β_{d50}	1.44***	0.310	0.832**	0.315	0.987***	0.271
$\beta_{\text{d50}\times\text{dist.}}$	-8.00E-3**	3.80E-3	-1.11E-3	3.87E-3	-4.57E-3	3.57E-3
β_{price}	-1.95E-3***	3.90E-4	-1.83E-3***	4.16E-4	-1.20E-3***	3.85E-4
$\beta_{\text{price}\times\text{dist.}}$	-8.38E-6	5.08E-6	-1.01E-5*	5.42E-6	-2.57E-5***	5.97E-6
σ_{ASC}	-5.86***	0.701	5.13***	0.604	-3.91***	0.389
σ_{d18}	1.70***	0.265	-1.84***	0.284	-1.47***	0.259
Number of observations:		1110		1008		1152
Number of individuals:		185		168		192
Number of draws:		500		500		500
Final log-likelihood:		-698.222		-678.354		-779.545

Note: *** = significant on 99% level, ** = significant on 95% level and * = significant on 90%.

The first thing to note from table 6.3.1 is that, regarding significance and spatial dependence, the results look quite similar to the ones from the spatial model calculated on the full sample in Section 6.2. Although, the model calculated from the ct-sample captures some spatial dependence, namely in the price variable that is significant at a 90 % level. In the model calculated from the nonct-sample, the interaction between the 50 km attribute and distance is significant and with negative sign. Finally, in the model calculated from the oor-sample, all variables, with the exception of the interaction between the 50 km attribute and distance, are significantly different from zero. Accordingly, the model calculated from the oor-sample again captures, as we would expect, a high degree of spatial dependence (99 % confidence level) through the interaction with the price variable.

As in the previous Section, 6.2, I calculate the willingness to pay for each of the attributes and present them conditional on distance. This is done in figure 6.3.1.



(a) ASC *(b)* 18 km *(c)* 50 km

Figure 6.3.1: Willingness to pay estimates for male sample (in DKK)

We see that figure 6.3.1 also bears some resemblance to figure 6.2.2. The main thing to notice here, however, is that the the model run on the ct-sample now catches spatial dependence in the interaction with the price variable. Therefore, the willingness to pay calculated from the ct-sample is now given by convex curves, as is also the case for the model run on the oor-sample.

Focusing on the willingness to pay estimates for the fixed 50 km attribute in figure 6.3.1c, we see a more complex picture than in the other two sub figures of figure 6.3.1. The willingness to pay estimates start out at the same level for the model run on the oor and nonct-sample, while for the model calculated from the ct-sample, willingness to pay starts out quite a bit lower. However, both estimates calculated from the nonct and oor-sample decrease to a level below the ones calculated from the ct-sample as distance increases. In fact, the willingness to pay estimates calculated from the nonct-sample are lower than the ones calculated from the ct-sample at distances larger than 111 minutes away and are lower than both the ones calculated from the ct and oor-sample at distances larger than 126 minutes away. The willingness to pay estimates calculated from the oor-sample decrease beneath the ones calculated from the ct-sample at 71 minutes.

Note also that the willingness to pay scale (y-axis) is adjusted upwards in figure 6.3.1c relative to the other sub figures in figure 6.3.1 or any of the sub figures in figure 6.2.2. Therefore, when comparing the estimates calculated from the male sample with the full sample, figure 6.3.1c with figure 6.2.2c, we see that the willingness to pay estimates generally start at a much higher level for the male sample but also decrease more steeply than the ones calculated for the full sample. This emphasizes that the spatial dependence is particularly strong in the male sample.

6.3.2 Spatial model on female sample

In table 6.3.2, results from the spatial models run on the female samples are presented.

Table 6.3.2: Estimation results of the spatial model based on female sample

	nonct sample		ct sample		oor sample	
Variable	Parameter estimate	Std. error	Parameter estimate	Std. error	Parameter estimate	Std. error
ASC	0.236	0.409	0.565	0.461	0.838**	0.357
β_{d18}	0.375*	0.214	1.14E-3	0.231	0.208	0.224
β_{d50}	0.715**	0.321	1.02***	0.361	0.533	0.333
$\beta_{\text{d50}\times\text{dist.}}$	-2.02E-4	3.82E-3	-4.11E-3	4.28E-3	-1.71E-3	3.94E-3
β_{price}	-2.64E-3***	4.59E-4	-3.65E-3***	5.95E-4	-2.58E-3***	5.05E-4
$\beta_{\text{price}\times\text{dist.}}$	-1.97E-6	5.50E-6	1.92E-6	6.60E-6	-1.07E-5	6.40E-6
σ_{ASC}	-4.82***	0.556	5.06***	0.605	3.42***	0.378
σ_{d18}	-1.48***	0.271	-1.45***	0.314	-1.49***	0.295
Number of observations:		1062		978		918
Number of individuals:		177		163		153
Number of draws:		500		500		500
Final log-likelihood:		-676.810		-560.348		-614.899

Note: *** = significant on 99% level, ** = significant on 95% level and * = significant on 90%.

From table 6.3.2 we see, as expected, that the price estimates are significantly different from zero with negative parameter estimates. Note, however, that the preferences towards the fixed 50 km attribute are not significantly different from zero in the model run on the oor-sample. This is surprising and not at all what we would expect. Note as well, that no spatial dependence is caught in any of the models. For this reason, I have run the basic model from Section 6.1 on the female samples alone, in order to determine if the unexplained results from table 6.3.2 are consistent with this. Also the spatial model run on the ct-sample did not converge with the default algorithm in Biogeme 2.3[18]. This suggests a bad model specification and is, in itself, a reason to respecify. The results from the basic model run on the female samples are given in table 6.3.3.

The results presented in table 6.3.3 are in line with what we would expect from this model. Namely we see that preferences towards the 50 km attribute are positive and significantly different from zero across all samples. Furthermore, the standard errors are generally smaller, while the parameter estimates don't vary too much between the model

[18]As mentioned earlier the results presented here are found using the 'SOLVOPT' algorithm for maximization of the (simulated) log-likelihood function.

Table 6.3.3: Estimation results of the basic model

	nonct sample		ct sample		oor sample	
Variable	Parameter estimate	Standard error	Parameter estimate	Standard error	Parameter estimate	Standard error
ASC	0.241	0.409	0.562	0.462	0.754**	0.335
β_{d18}	0.376*	0.213	3.85E-3	0.231	0.177	0.222
β_{d50}	0.699***	0.152	0.713***	0.172	0.647***	0.160
β_{price}	-2.78E-3***	0.239E-3	-3.50E-3***	0.319E-3	-3.33E-3***	0.286E-3
σ_{ASC}	4.82***	0.557	5.06***	0.605	3.36***	0.373
σ_{d18}	-1.48***	0.269	1.44***	0.315	1.44***	0.286
Number of observations:		1062		978		918
Number of individuals:		177		163		153
Number of draws:		500		500		500
Final log-likelihood:		-676.881		-560.814		-616.497

Note: *** = significant on 99% level, ** = significant on 95% level and * = significant on 90%.

presented in table 6.3.2 and 6.3.3. This is interesting because it tells us that adding the spatial terms to the model (i.e. $\beta_{\text{price}\times\text{distance}}$ and $\beta_{\text{d50}\times\text{distance}}$) only added noise and didn't contribute with any new information about the relationships within the data. By making this observation we conclude, quite surprisingly, that we don't find that females react to spatial differences.

Because there are no spatial dependencies for the female samples, the willingness to pay is a static measure. Therefore, instead of illustrating the willingness to pay estimates in a figure I present a table similar to the one given in Section 6.1. In table 6.3.4, the results from the willingness to pay calculations for the female samples are given.

Table 6.3.4: WTP (in DKK) calculated from the basic model on the female sample

Variable	nonct sample	ct sample	oor sample
ASC	86.7	160.6	226.4
σ_{ASC}	1733.8	1445.7	1009.0
β_{d18}	135.3	1.1	53.2
σ_{d18}	532.4	411.4	432.4
β_{d50}	251.4	203.7	194.3

Table 6.3.4 indicates that the means from the samples vary in size for the alternative-specific constant and the 18 km attribute. Note that the estimated mean for the 18 km attribute is very close to zero, suggesting that almost half of the female population actually considers moving wind turbines from 8 kilometers (status quo) out to 18 kilometers to be a negative change. Also, note that the standard errors are large relative to the size of the

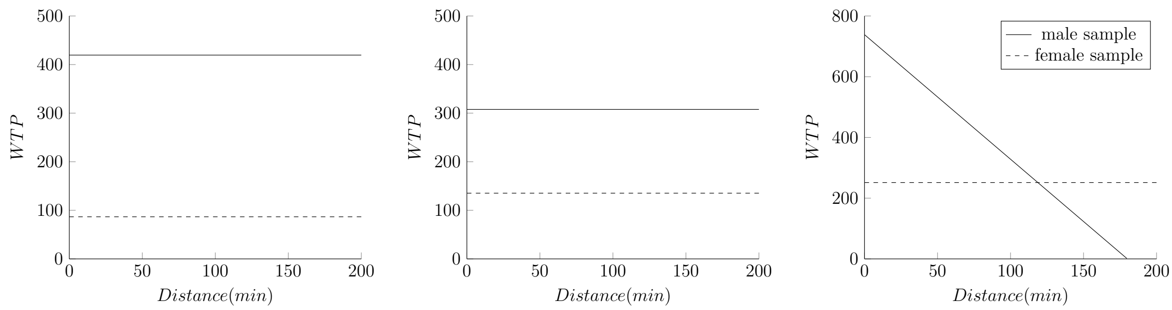
means.

For the 50 km attribute, the willingness to pay decreases from estimates based on the nonct-sample to the ones based on the ct-sample and, again, from the estimates calculated from the ct-sample to the ones from the oor-sample. Furthermore, we note that the difference in the willingness to pay estimates between the estimates from the nonct-sample and the ct-sample is large compared to the one between the estimates from the ct-sample and oor-sample. This suggests that the reminders work as expected for the female respondents. Namely, that willingness to pay estimates are lower when respondents receive a cheap talk reminder (ct-sample) and further decrease when they have received both a cheap talk and an opt-out reminder (oor-sample).

6.3.3 Variation by gender

In this section, I compare the willingness to pay estimates calculated from the different samples across gender. As we have seen, there is spatial dependence in the models run on the male samples, while this is not the case for the ones run on the female samples. Nevertheless, in figure 6.3.2, 6.3.3 and 6.3.4 I again illustrate willingness to pay as a function of the spatial variable (distance) for each of the respective samples. In the light of the non-spatial dependency in the female samples, the willingness to pay calculated from the female samples are illustrated with horizontal lines in the figures.

In figure 6.3.2, the willingness to pay calculated from the nonct-sample is presented.

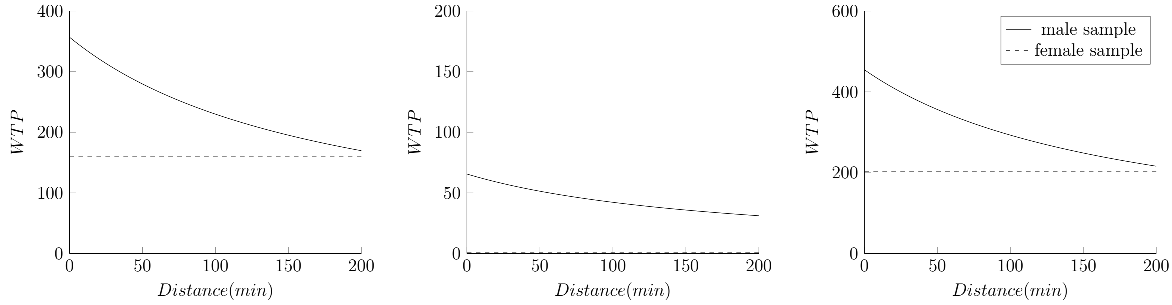


(a) ASC *(b)* 18 km *(c)* 50 km

Figure 6.3.2: Gender specific willingness to pay estimates for nonct-sample (in DKK)

From figure 6.3.2, we see that the means from the willingness to pay distributions for both the alternative-specific constant and the 18 km attribute are higher for males than for females. For the fixed 50 km attribute, the willingness to pay estimates calculated from the male sample are higher for respondents living close to the potential wind turbine farms. However, the model run on the male sample picks up some spatial dependence and, therefore, willingness to pay drops to a level below the one calculated from the female sample at 119 minutes. Therefore, from the gender models run on the nonct-sample, we conclude that the willingness to pay to move wind turbines out to 50 kilometers relative to 8 kilometers (status quo) is lower for males than females when respondents live further than 119 minutes away.

The willingness to pay estimates calculated from the ct-sample are given in figure 6.3.3.

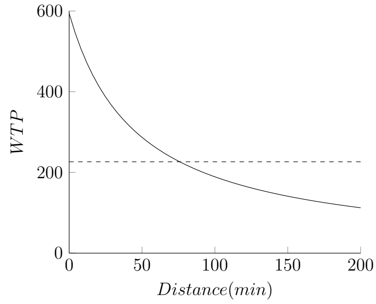


(a) ASC *(b)* 18 km *(c)* 50 km

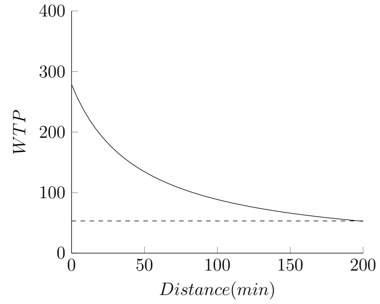
Figure 6.3.3: Gender specific willingness to pay estimates for ct-sample (in DKK)

Again, we see from this figure that the means from the willingness to pay distributions for both the alternative-specific constant and the 18 km attribute are higher for the male sample than the female sample. However, because of the spatial dependency in the price variable for the male sample, it looks as though the mean estimates from the male sample move asymptotically towards the ones from the female sample for the alternative-specific constant. Also for the fixed 50 km attribute, the estimated willingness to pay is higher for the male sample. In addition, although there is spatial price dependency in the male sample, the estimates from this sample do not drop below the ones from the female sample on the distance interval. However, also for these estimates it appears that the willingness to pay calculated from the male sample moves asymptotically toward those of the female sample as distance is increased. For this reason, the willingness to pay estimates for the 50 km attribute end up at the same level for both genders at the end of the distance interval.

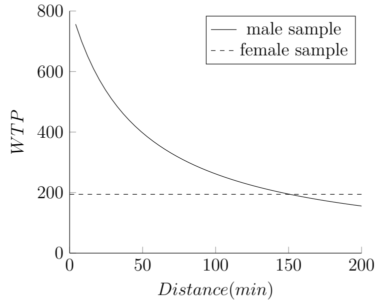
Finally, the willingness to pay estimates calculated from the oor-sample are given in figure 6.3.4.



(a) ASC



(b) 18 km



(c) 50 km

Figure 6.3.4: Gender specific willingness to pay estimates for oor-sample (in DKK)

Looking at the means calculated from the willingness to pay distributions for the alternative-specific constant for both gender samples in figure 6.3.4a, we see that estimated means from the male sample start out higher than the ones from the female sample but decrease to below the ones calculated from the female sample. This intersection is at 76 minutes. The means of the willingness to pay distribution calculated for the 18 km attribute indicate that those calculated from the male sample are higher than the ones calculated from the female sample (almost) over the entire interval. In fact they, at the very end of the interval, intersect; this intersection occurs at 198 minutes. Finally, for the 50 km attribute, the estimated willingness to pay is again higher for the male sample. In fact, from the y-axis, we see that the relative difference is quite large for individuals living in the near vicinity of wind turbines. However, due to the strong spatial dependency in the male sample, the willingness to pay estimates for the male sample drop below the ones calculated from female sample at 151 minutes. Accordingly, from the estimates calculated from the oor sample, we conclude that males willingness to pay to move wind turbines out to 50 kilometers relative to 8 kilometers (status quo) is higher for individuals living closer than 151 minutes to the proposed site and vice versa.

6.3.4 Gender models with varying sample weights

The results found in the gender specific spatial models are surprising, therefore, in this section, I will examine the robustness of these results in the same manner as in Section 6.2.1 for the full samples. As in Section 6.2.1 the sample weights are changed, in order to represent each of the underlying samples, i.e. the nonct, ct or oor-sample, for both the male and the female sub samples. Note that, due to the extensive number of models, the results will be reported in less detail than in Section 6.2.1. The full tables of model results are included in Appendices and referred to rather than included directly in the text.

Examining first the male samples where sample weights vary, we see a picture where similar conclusions can be drawn as the ones found when looking at the full sample in Section 6.2.1. This can be verified both by looking at table H.1 and H.2 in Appendix H but also the figures in Appendix I illustrating the estimated willingness to pay for the different attributes conditional on distance. Namely the estimates from the sample given both the cheap talk and opt-out reminder are very similar when sample weights vary.

The story is different when we examine the results found from the female samples with varying sample weights. This can be verified by looking at table J.1 and J.2 in Appendix J and the figures in Appendix K. Notice that I include the spatial variables to find the results in these appendixes, even though I in Section 6.3.2 established that no spatial dependence was picked up in any of the female samples, independent of reminders. I do this, of course, because I want to establish if the result, i.e. that no spatial dependence is present for female respondents, holds when sample weights vary. Although the results from the female samples with varying sample weights are quite different than the ones from the male samples, some spatial dependence is picked up by the price variable in the sample that received an opt-out reminder when sample weights are chosen to represent both the ct and oor-sample. When sample weights are chosen to represent the ct-sample, this spatial dependence is significant on a 90 % confidence level. Likewise, when sample weights are chosen to represent the oor-sample, the spatial dependence is significant on a 95 % confidence level. Therefore, to summarize, female respondents who received an opt-out reminder in the questionnaire, do state preferences that are spatially dependent in some cases, however, this spatial dependence is not as significant as for the male respondents and depends on the underlying sample weights chosen.

7 Discussion of results

The first thing that the results from Section 6 show us, is that the estimated random variables are all associated with relatively large estimated standard deviations. Therefore, the mean may be of some interest but especially what we are able to draw from these calculations is the underlying structure of the willingness to pay, i.e. whether or not there is spatial dependence. Furthermore, it tells us that the preferences for these attributes are, in fact, very volatile across the population. Some of the explanation for this volatility may be that respondents are not certain about preferences towards these attributes in general and, therefore, a large degree of randomness is included in the estimates. This could perhaps, to some extent, have been alleviated when collecting the data. Notice that in the example choice set in Appendix A, from the photo where wind turbines are placed 50 kilometers from shore, the wind turbines are not visible at all. This may be easier to relate to than choosing between distances from where the wind turbines are visible. One way to go about this could be to have included more background information in the questionnaire. As was also noted in the questionnaire the visibility of the wind turbines is dependent on weather, therefore, including how many days a year (given typical Danish weather) the wind turbines are visible at the different distances could perhaps have reduced some of the randomness causing preferences to vary. Likewise, with respect to the 50 km attribute, it might also have been relevant to inform respondents that, due to the curvature of the earth, wind turbines of this size placed at this distance are in fact hidden (or nearly so, Nielsen et al. (2009)), such that, even on a clear day with binoculars, they are not visible from ground level. This being said, we would not expect the lack of this information to draw the preferability of either the alternative-specific constant or the 18 km attribute in any specific direction. By this I mean that this randomness affects perhaps only the standard deviation and not the mean itself and it is the mean which is of interest from a policy perspective (as long as society is asked to pay for the improvement collectively). Therefore, I acknowledge that large variation must be associated with mean estimates of the willingness to pay for both the alternative-specific constant and the 18 km attribute, making it difficult to draw conclusions from the relative levels of the willingness to pay estimates across models. Nevertheless, the structure of the willingness to pay estimate is shown here (in some cases) to depend on spatial variation which is unrelated to the large variation of the mean estimates. Thus, differences in the structure of willingness to pay can be attributed sample differences, i.e. reminders and gender.

Because spatial variation is not included, and for the reasons stated above, the willingness to pay estimates attained from the basic model in Section 6.1 for both the alternative-specific constant and the 18 km attribute are not of great further interest for this particular study. From the model in Section 6.1, I found that the willingness to pay for the fixed 50 kilometer attribute decreased when respondents received a cheap talk reminder and decreased further when they also received the opt-out reminder (see table 6.1.2). As was also noted in Section 6.1, this is in line with the theory that reminders decrease willingness to pay estimates and, thus, alleviate hypothetical bias. Specifically, willingness to pay estimated from the ct-sample decreases with 17 % relative to the nonct-sample, while the one estimated from the oor-sample decreases with 26 % relative to the nonct-sample. Accordingly, willingness to pay estimated from the oor-sample decreases with 11 % relative to the ct-sample.

From economic theory, we believe that the willingness to pay for a geographical improvement is higher - ceteris paribus - if the individual in question lives near the geographical improvement. The results from table 6.2.1 show that only the model run on the sample that received both the cheap talk and the opt-out reminder caught this effect. Furthermore, this effect was highly significant (99 % confidence level) from this sample. Another interesting thing to note is that the model run on the sample that didn't receive a reminder caught a spatial effect in the preferences for the 50 kilometer attribute. This, as explained in Section 6.2, means that individuals who live further away from the potential wind turbine site are observed to have decreasing preferences towards moving wind turbines out to 50 kilometers. While this relationship may be possible to defend intuitively, we would expect the dominant spatial dependence to be the one caught in the price variable. In addition to the graphical illustration in figure 6.2.2c, I compare the willingness to pay estimates from the spatial model run on the full sample numerically in table 7.0.5. Notice, however, that table 7.0.5 and, specifically, the result for the ct-sample must be read with some caution, as discussed below.

	WTP (DKK)			%-change in wtp (sample x relative to [y])		
Distance (min)	nonct	ct	oor	ct [nonct]	oor [nonct]	oor [ct]
0	491	362	475	-26 %	-3 %	31 %
50	392	362	296	-7 %	-24 %	-18 %
100	293	362	216	23 %	-27 %	-40 %
150	194	362	169	86 %	-13 %	-53 %
200	96	362	139	278 %	46 %	61 %

Table 7.0.5: Willingness to pay comparison for spatial model (full sample)

From table 7.0.5, it appears that the willingness to pay estimates derived from the sample given no reminder resemble the ones estimated from the sample given both the cheap talk and opt-out reminder. Thus, table 7.0.5 on its own may lead one to believe that the benefit from giving respondents a cheap talk and an opt-out reminder is limited. However, on the contrary, the spatial dependence which makes the estimates appear similar is derived from two different sources, as has been discussed. Because the spatial dependence, that we trust exists, is picked up only by the model run on the sample given both the cheap talk and the opt-out reminder, we establish from this model that a cheap talk reminder, combined with an opt-opt reminder, plays an important role in estimating realistic willingness to pay estimates.

Note that the spatial model in Section 6.2 is not directly comparable to the basic (non-spatial) model in Section 6.1. This is easiest seen by looking at the willingness to pay estimates calculated from the sample that received only a cheap talk reminder. Although no (significant) spatial dependence is picked up in the spatial model, the willingness pay estimate (which is, therefore, constant) for moving wind turbines to 50 kilometers is 362 DKK, while it in the basic model is only 204 DKK. This large increase is due to the fact that, although the spatial variable was not significant at any reasonable level (p-value = 0.18), some spatial variation is still picked up. This means that part of the variation from the price variable is reflected in the insignificant spatial interaction term. This, in turn, means the price estimate, itself, is deflated, resulting in lower resistance to price increases and, thus, a greater willingness to pay. Therefore, the willingness to pay estimate calculated from the sample that received only a cheap talk reminder is upwards biased. It is important to recognize that this problem, amounting to the large differences between results, occurs because we are calculating willingness to pay as an additional step after estimating our model. And, furthermore, because the price estimate is correlated with an insignificant variable. Normally, when doing linear regression analysis, we would welcome any variable that helps to explain the relationships within the data and, thus, leave us with the most 'pure' parameter estimates for each given attribute. This, of course, is also the case here, although in the case of the model run on the ct-sample, it leaves us with a dilemma when calculating willingness to pay. This is because we have to choose a discrete 'cut-off'-point for the significance level that, in turn, influences the final willingness to pay estimate. It is, therefore, unsatisfying for the researcher who is looking at the ct-sample and sees that the cheap talk reminder did not quite pick up the spatial relationship that is believed to exist. And he must choose to ignore this dimension of the data in order to get reasonable willingness to pay estimates. This is, again, a major argument in favor of combining the cheap talk reminder with the opt-out reminder when designing choice experiment surveys.

However, with the exception of the case of the sample that received only the cheap talk reminder, looking at the samples that picked up spatial dependence the estimated willingness to pay from the basic model is within the interval of the willingness to pay calculated from the spatial model. This is what we would expect.

Turning to the results from Section 6.3 and looking first specifically at the male sample in Section 6.3.1, we see the following: As opposed to the model run on the full sample, spatial dependence is also picked up by the sample that received a cheap talk reminder only. As in the case of the sample given the opt-out reminder as well, spatial dependence is picked up by the interaction with the price variable. This is especially interesting given that none of the models calculated on the female sample, in section 6.3.2, picked up any spatial dependence of any sort. There can be two reasons for this.

One reason could be that females don't, on average, have preferences that change spatially. Perhaps females think more altruistically when they make choices, such that they might want to do something good for the people who live close to the wind turbine site, even though they, themselves, live far away. Alternatively, maybe females don't make the geographical connection to themselves when they answer a questionnaire. Another reason could be that females simply don't react to the reminders. However, from table 6.3.4 in Section 6.3.2, we saw that this was, in fact, not the case. When females received the cheap talk reminder, they decreased their willingness to pay for the 50 km attribute by 19 % relative to when they received no reminder and, when they received both the cheap talk reminder and the opt-out reminder, they decrease their willingness to pay by 23 % also relative to when they receive no reminder. The difference is smaller for the female sample than for the full sample between the sample that received a cheap talk reminder and the sample that received both the cheap talk and the opt-out reminder. However, females still decrease their willingness to pay for the 50 km attribute by 5 % when they receive both reminders.

This tells us that the females do, in fact, react to the reminders and leads us to believe either that females do not evaluate their preferences spatially or that the reminders aren't sufficient in addressing the females in a way that will make them reveal their true spatial preferences. Perhaps this could be further investigated by including an explicit spatial aspect in one of the reminders to see if females will then, reveal spatial preferences. Notice that a map was included in the questionnaire, such that geographical placement relative to potential wind turbines should be clear in the respondents' minds. However, more sophisticated methods of informing respondents about spatial relationships have proven to be successful. In Holland et al. (2014) the authors utilize that GIS[19] software programs have made it possible to calculate many geographical relationships fast, why they are able to present the respondent with the actual distance between their residence and the proposed site in question. I believe that such, more sophisticated, methods of informing respondents about spatial relationships will gain ground in the future and would be an interesting addition to the present study.

To sum up, the results in Section 6.3 are very clear. Given the same reminders, i.e. the cheap talk and the opt-out reminder, males reveal spatially dependent preferences while females don't. This, in itself, is interesting and tells us quite clearly that, for whatever reason, males and females behave differently when responding to the same questionnaire.

Furthermore, differences in magnitudes of willingness to pay between males and female are presented in Section 6.3.3. As a general remark, from these calculations, it looks like females are willing to pay less in order to move wind turbines further out than 8 kilometers from shore than males. By examining table 6.3.1 and 6.3.2 more thoroughly we see that this difference in willingness to pay cannot be attributed only that females are more averse to price changes or that they value the distance attribute in itself less than males, however, it is a combination of the two.

In Section 6.2.1, I have focused on the robustness of the results found from the full samples that received different reminders. This I have done by changing the underlying demographic sample weights marginally, and examining the variation in results accordingly. Note that despite the fact that we find female respondents don't reveal spatial preferences, at least when sample weights are chosen to represent the nonct-sample, we still expect spatial dependency to be present when examining the samples collectively (male and female). The first thing to note from the figures in Section 6.2.1 is that the sample that only received a cheap talk reminder react spatially both when the sample weights are chosen to represent the demographics of the ct and oor-sample. This spatial

[19]Geographical Information System.

dependence comes from the interaction with the price variable.

When looking at the sample that received no reminder, large inconsistencies are observed between the models run with different sample weights. This is especially visible from willingness to pay for the 50 km attribute in figure 6.2.5a. Here, we see that spatial dependence is either not picked up at all, is picked up by the interaction with the 50 km attribute or, lastly, is picked up by the interaction with the price variable. This observation is in some sense alarming because it leads us to believe that the preferences calculated from choice experiments where respondents didn't receive a reminder depend heavily on the random underlying sample. Because the underlying sample weights all represent real samples that are chosen to be representative of the same population, this is a serious problem. In order to relate this finding to the one above, where we saw that differences in demographics (gender) did in fact influence choices, I have included the share of male respondents across samples in table 7.0.6[20].

nonct-sample	ct-sample	oor-sample
0.510	0.497	0.555

Table 7.0.6: Share of males in sample

Interestingly, when the sample that received no reminder is weighed to represent the sample that has the largest share of males (oor-sample), this is the only case where no spatial dependence is significantly picked up (see table G.2 in Appendix G). This is interesting because figure 6.3.2 in Section 6.3, shows that the male sample does, in fact, pick up the spatial dependence in the 50 km attribute when respondents are not given a reminder. Therefore, the gender combination in the samples is not responsible for these changes.

What can be concluded from the difference in the structure of willingness to pay in terms of the sample that received no reminder after it is given different sample weights, may be that respondents reveal preferences that have a (more or less) random spatial structure. This means that some respondents reveal their true spatial preferences even though no reminder is given while others do not. This on the other hand means that, when we change the sample weights marginally, we weigh up or down respondents who react spatially in a completely random fashion, thus causing the spatial dependence to vary completely with the sample weights. The same is essentially the case when examining the sample that received a cheap talk reminder. Spatial dependence is picked up, as we would expect, in two out of three cases, which is better than for the case where respondents received no reminder. However, when we chose the sample weights to represent the

[20]Descriptive statistics of all underlying demographic variables used to weigh samples are included in Appendix E.

demographics of the nonct-sample, we choose an 'unlucky' sample - weighing respondents who didn't reveal spatial preferences (even when receiving the cheap talk reminder) up.

Finally, adding the opt-out reminder make 'the rest' of the respondents reveal spatial preferences, such as we would expect. In this case the willingness to pay structure is the same and even the level of the estimates are the same. It seems, therefore, that the opt-out reminder does a good job in nudging the remaining respondents in the right direction, i.e. making them reveal their spatial preferences, and, thereby, limiting the probability of choosing an 'unlucky' sample. Furthermore, here it is again relevant to bear in mind, that whether or not spatial dependence is picked up in this analysis is dependent on the discrete 'cut-off' point for significance. I have chosen to work with a 90 % significance level, i.e. spatial dependence is caught if the variables interacted with distance are significantly different from zero with, at least, a 90 % probability. However, it is very relevant to recognize that my conclusions actually gain strength if a more strict significance level is chosen, this can be verified by the results presented in table 6.2.1 (Section 6.2) and table G.1 and G.2 in Appendix G. No matter how the sample weights are chosen the sample that received both the cheap talk and opt-out reminder are found to state preferences for the price variable that are spatially dependent on a confidence level > 99 %. At a 99 % confidence level none of the samples that either received no reminder or the cheap talk reminder alone, with any of the sample weights, catch any spatial dependence in preferences.

This robustness 'test' of the reminders was also done for the spatial gender specific models in Section 6.3.4. The interesting thing to note from this section is that females did, in fact, reveal preferences that were spatially dependent when they received the cheap talk and the oor-reminder, this can, once again, be confirmed from Appendix K. However, this was only the case when sample weights were chosen to represent the ct and oor-sample. Thus, an important insight can be added to the analysis, namely that females may, in fact, have spatially dependent preferences, which we would also expect. These spatial preferences are, however, not as easily picked up as the spatial dependence in males' preferences. This may either be due to weaker spatial preferences or insufficiency in reminders regarding females.

8 Conclusion

From this study, several things can be concluded. In this section I will, briefly, sum up and underline the most interesting and, in the literature, original findings.

I have examined how respondents act when survey designs in choice experiments are changed marginally. As previous studies before this, I find that reminders, both the cheap talk reminder alone and a combination of the cheap talk and the opt-out reminder, are effective in decreasing willingness to pay estimates and, thus, estimating results with minimal hypothetical bias.

I find that when a cheap talk and opt-out reminder are combined and included in the survey design, respondents reveal spatial preferences. I also find that this spatial effect is primarily picked up due to males' preferences, as females do not react as much to spatial differences. This may suggest that more work can be done with reminders in order to make females state consistent spatial preferences.

Furthermore, I found that including a combination of a cheap talk and an opt-out reminder in the survey design made results robust to marginal changes in the sample weights. This was done by weighing samples, such that the samples represent one another, by the underlying demographics. Such that all sample weights represented real random samples of the true population. Both if respondents did not receive any reminder or if they received only a cheap talk reminder, the spatial effect was not significantly picked up in the price variable or not picked up at all, when sample weights were changed. This finding is particularly relevant because it is a strong argument to include both a cheap talk and an opt-out reminder in similar choice experiments in order to make results robust to marginal differences in the sample. Including both a cheap talk and an opt-out reminder in choice experiment designs is not standard procedure today, which is why this result is valuable in order to design future choice experiments in a way that reveals the most trustworthy results.

I also tried changing the sample weights, marginally, for the gender specific models. This revealed that spatial preferences were also caught in the female samples, when respondents received both a cheap talk and an opt-out reminder, in two out of three of the cases. This, again, indicates that it is more difficult to pick up spatial preferences for females.

To sum up, the present study strongly suggests including both the cheap talk and the opt-out reminder in choice experiment survey designs. The estimated preferences from doing this are, for the most part, in line with economic theory. However, more work can be done, especially, in relation to addressing female respondents effectively.

9 Final remarks

The main focus of this study has been methodical issues in stated preference studies. There are several reasons why I chose this focus, which I would like to justify.

First off, much literature has been produced regarding magnitudes of willingness to pay for moving offshore wind turbines further away from shore. Therefore, again taking this perspective was not of much interest. Secondly, exploring effects of the opt-out reminder has not been focused much on in previous literature. Therefore, an extensive study of this seemed to be in place and highly relevant. Finally, the perhaps most honest reflection, is that I acknowledge that I am working with relatively old data, collected in 2006, in a field that is changing fast. Therefore, focusing heavily on specific magnitudes of willingness to pay seems out of place, especially since no research, to my knowledge, has been done regarding changes in preferences for environmental goods over time. In other words, from my perspective, we could easily imagine that, in a time where green energy sources are quickly gaining ground, there is more acceptance of green energy sources (such as wind turbines) in the landscape and, therefore, data collected nearly ten years ago may not be representative of today's society. However, exploring respondents' reactions when presented with different reminders in a choice experiment setting is still highly relevant, even though we may be looking at 'old' data.

Accordingly, this study was not meant to present conclusive magnitudes of willingness to pay, that policy makers could use to optimize decisions in relation to placement of offshore wind turbines. This study was, instead, made for the scientific community in order to increase reliability of future surveys, that policy makers may then use in their decision making process.

A Example of choice set

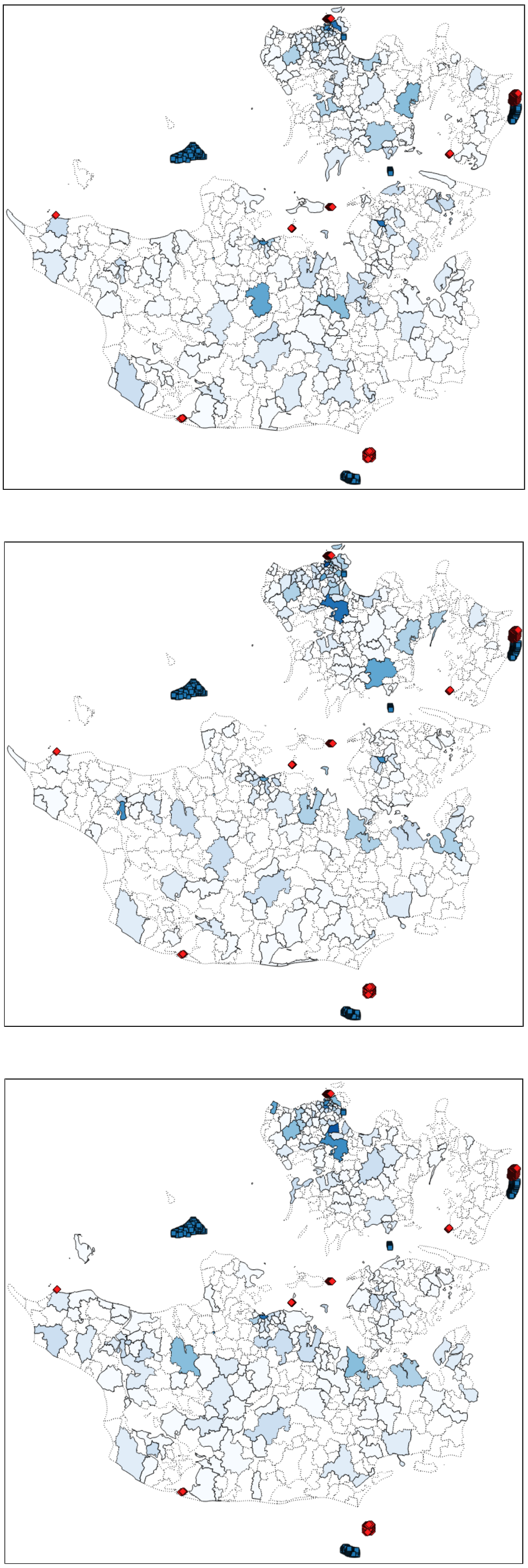
Alternative 1 Distance to the Coast: 8 km Increase in costs: 0 DKK

Alternative 2 Distance to the Coast: 12 km Increase in costs: 400 DKK

Alternative 3 Distance to the Coast: 50 km Increase in costs: 700 DKK

I prefer: **Alternative 1 ( )** **Alternative 2 ( )** **Alternative 3 ( )**

B Geographical distribution of samples



a: nonct-sample

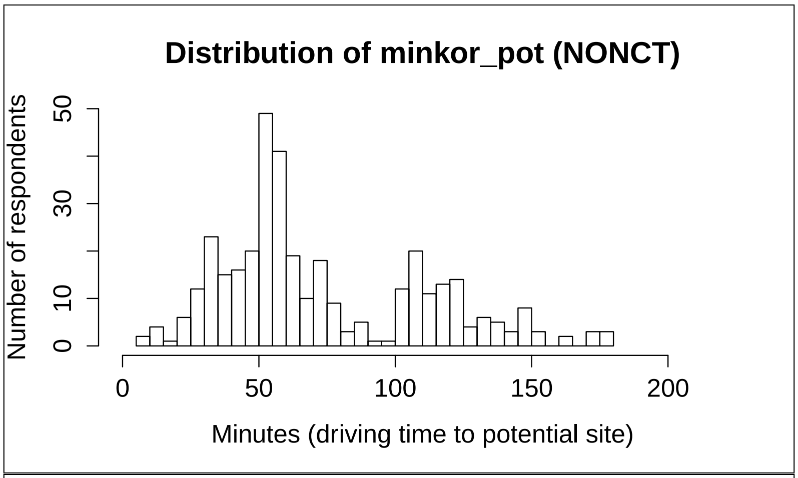
b: ct-sample

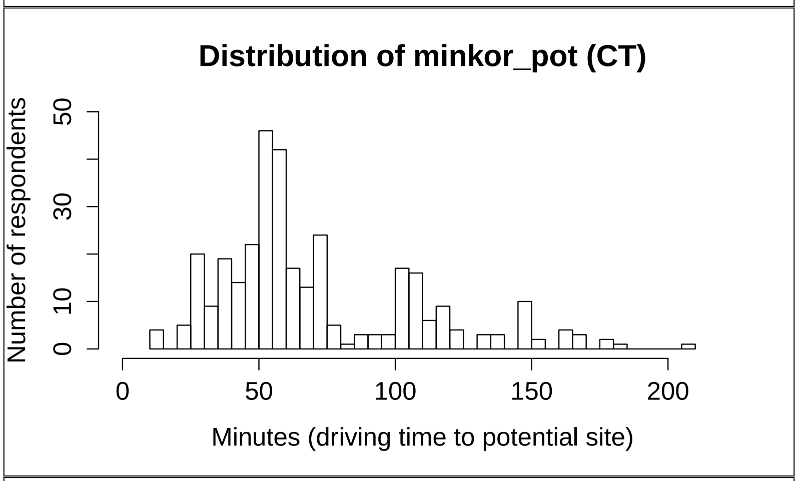
c: oor-sample

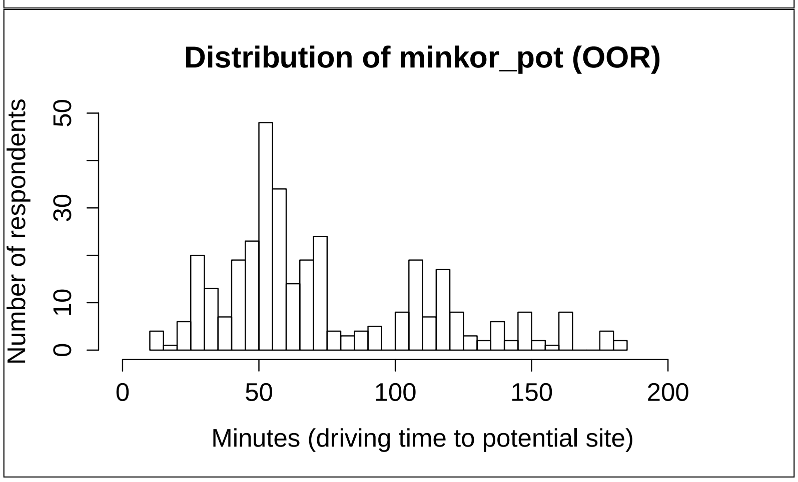
Figure B.1: Sample selection by area code. The darker shade of blue indicates that more respondents were drawn from this area code.

C Distribution of spatial variable

Distributions of the spatial variable (minkor_pot) across the different samples are included below.







D Weighing samples

The samples have been weighed as the nonct-sample. This means that each observation from the ct and oor-sample (every choice) has been given a weight different from one. The weights are derived from a logit model calculated from a sample where I have pooled first the nonct and ct-sample and then the nonct and oor-sample. In the logit model I have included demographic variables that could differ between samples as explanatory variables. The dependent variable is a binary variable describing whether or not the observation is drawn from the nonct-sample. The variables included in the model are: age, income, level of primary schooling, level of further education, gender, driving time to nearest potential offshore wind turbine farm, driving time to nearest offshore wind turbine farm and whether or not respondents claim that there are wind turbines in the area they live. I use the predicted values from the model to calculate my weights. The predicted values can be interpreted as the probability that an observation is drawn from the nonct-sample based on the demographic variables included. That is, if the respondents are on average richer in the nonct-sample there would, ceteris paribus, be a positive probability that the observation is drawn from the nonct-sample if the income-variable returns a large value.

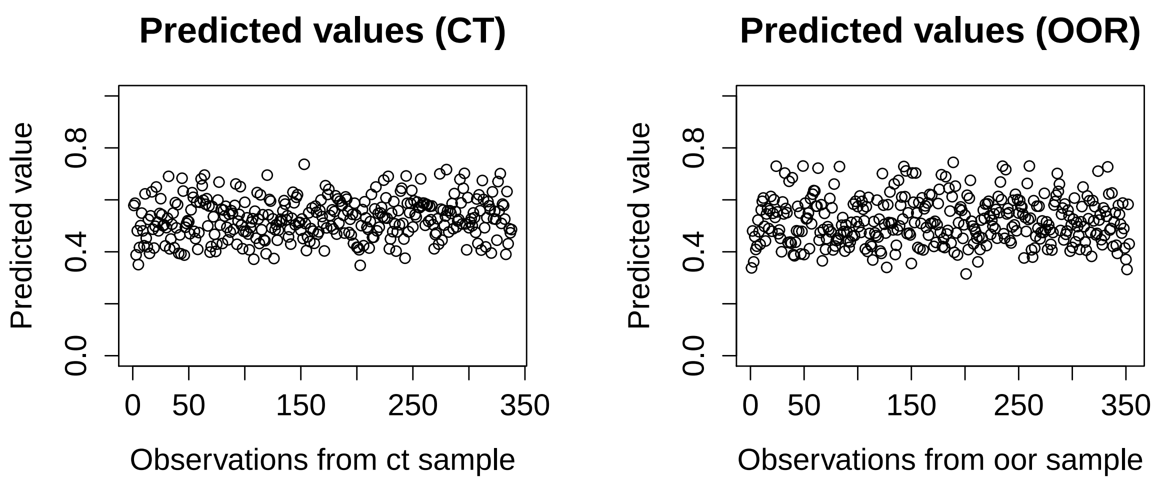


Figure D.1: Predicted Values

In figure D.1 I have plottet the predicted values from the logit models described above. As can be seen the predicted values behave quite nicely and stick to the approximate interval [0.3;0.7]. The weight for every observation i, that is not in the nonct-sample is calculated by equation D.1. If the value is drawn from the nonct-sample it gets a weight equal to one.

$$\text{Weight}_i = \frac{\text{predicted-value}_i}{1 - \text{predicted-value}_i} \tag{D.1}$$

This is intuitive because every value that has a predicted value larger than 0.5, meaning that the probability that it is drawn from the nonct-sample is larger than 50 %, is given a weight larger than one. Likewise, observations that get a predicted value smaller than 0.5 get a weight lower than one. Thereby, essentially what we are doing is weighing observations that resemble the reference sample (nonct-sample) well up and weighing observations that don't down.

E Demographic weight variables

Variable: age

Age is given by the respondents total age. A summery of the age variable's distribution across the three samples is included in table E.1.

Table E.1: Distribution of age variable

nonct-sample			ct-sample			oor-sample		
min	mean	max	min	mean	max	min	mean	max
16	44.79	83	16	45.17	86	16	44.65	80

Variable: income

Income is given by the respondents total income catagorized from 1 to 10, 10 being the highest. A summery of the income variable's distribution across the three samples is included in table E.2.

Table E.2: Distribution of income variable

nonct-sample			ct-sample			oor-sample		
min	mean	max	min	mean	max	min	mean	max
1	4.74	9	1	5.31	10	1	5.31	10

Variable: school

School is given as the level of the respondents primary schooling split into four steps; grade 7 or less, grade 8/9, 10th grade or graduated gymnasium/HF. A summery of the school variable's distribution across the three samples is included in table E.3.

Table E.3: Distribution of school variable

nonct-sample			ct-sample			oor-sample		
min	mean	max	min	mean	max	min	mean	max
1	3.39	4	1	3.34	4	1	3.39	4

Variable: education

Education is given as the level of the respondents further education split into seven catagories; basic training, completed business training, higher education short (3 years), higher education medium (3-4 years), higher education long (5+ years), other or none. A summery of the education variable's distribution across the three samples is included in table E.4.

Table E.4: Distribution of education variable

nonct-sample			ct-sample			oor-sample		
min	mean	max	min	mean	max	min	mean	max
1	4.07	7	1	4.07	7	1	3.90	7

Variable: gender

Gender is given as a dummy indicating whether or not the respondent is a male. A summery of the gender variable's distribution across the three samples is included in table E.5.

Table E.5: Distribution of gender variable

nonct-sample			ct-sample			oor-sample		
min	mean	max	min	mean	max	min	mean	max
0	0.510	1	0	0.497	1	0	0.555	1

Variable: minkor

Minkor is given as the driving time in minutes that a respondent has to the nearest (present at the time of the questionnaire) off shore windmill site. A summery of the minkor variable's distribution across the three samples is included in table E.6.

Table E.6: Distribution of minkor variable

nonct-sample			ct-sample			oor-sample		
min	mean	max	min	mean	max	min	mean	max
5	68	241	4	60	239	5	66	239

Variable: minkor_pot

Minkor_pot is given as the driving time in minutes that a respondent has to the nearest potential (at the time of the questionnaire) off shore windmill site. A summery of the minkor_pot variable's distribution across the three samples is included in table E.7.

Table E.7: Distribution of minkor_pot variable

nonct-sample			ct-sample			oor-sample		
min	mean	max	min	mean	max	min	mean	max
9	73	179	10	71	208	10	74	185

Variable: turb_in_area

Turb_in_area is given as whether or not the respondent claims that there are wind turbines in the area they reside (1 = yes, 2 = no). A summery of the turb_in_area variable's distribution across the three samples is included in table E.8.

Table E.8: Distribution of turb_in_area variable

nonct-sample			ct-sample			oor-sample		
min	mean	max	min	mean	max	min	mean	max
1	1.44	2	1	1.49	2	1	1.48	2

F General Biogeme (2.3) code

General code used to run the spatial models in Biogeme 2.3. This piece of code runs the model on the full sample, to specify the sample further the `[Exclude]`-statement may be added to or adjusted. Furthermore, to run the basic model the lines with '`// Spatial`' should be ignored.

```
// Author: Casper Bjerregaard, University of Copenhagen 2016
[ModelDescription]
"General Mixed logit model"

[Choice]
choice

[Weight]
weight_adj

[Beta]
d8              0 -50    50  1
d18             0 -50    50  0
d50             0 -50    50  0
ASC23           0 -50    50  0
price           0 -50    50  0
price_x_kor     0 -10    10  0                      // Spatial
d50_x_kor       0 -10    10  0                      // Spatial
sigma_d18       0 -100  100  0
sigma_ASC23     0 -100  100  0

[Utilities]
1 alt1   one     d8 * d81 + price * price1_100
2 alt2   one     d18 [ sigma_d18 ] * d182
               + d50 * d502
               + ASC23 [ sigma_ASC23 ] * ASC232
               + price * price2_100
               + price_x_kor * price_x_minkor2    // Spatial
               + d50_x_kor * d50_x_minkor2        // Spatial
3 alt3   one     d18 [ sigma_d18 ] * d183
               + d50 * d503
               + ASC23 [ sigma_ASC23 ] * ASC233
               + price * price3_100
               + price_x_kor * price_x_minkor3    // Spatial
               + d50_x_kor * d50_x_minkor3        // Spatial

[PanelData]
id1
d18_sigma_d18
ASC23_sigma_ASC23

[Expressions]
choice = 1 * choice1 + 2 * choice2 + 3 * choice3
one = 1
price1_100 = price1 / 100
price2_100 = price2 / 100
price3_100 = price3 / 100
price_x_minkor2 = ( price2 / 100 ) * minkor_pot2  // Spatial
price_x_minkor3 = ( price3 / 100 ) * minkor_pot3  // Spatial
d50_x_minkor2 = d502 * minkor_pot2                // Spatial
d50_x_minkor3 = d503 * minkor_pot3                // Spatial
weight_adj = weight1 * 1

[Draws]
500

[Exclude]
( nonct1 != 1 ) || ( minkor_pot1 == 99999) ||
( minkor_pot2 == 99999) || ( minkor_pot3 == 99999)

[Model]
$MNL
```

G Spatial models with different sample weights

Table G.1: Estimation results of the spatial model (Samples weighed as ct-sample)

Variable	nonct sample Parameter estimate	Std. error	ct sample Parameter estimate	Std. error	oor sample Parameter estimate	Std. error
ASC	0.284	0.324	0.590*	0.345	0.476*	0.242
β_{d18}	0.423**	0.159	0.187	0.158	0.332**	0.153
β_{d50}	1.16***	0.229	0.905***	0.234	0.892***	0.218
$\beta_{\text{d50}\times\text{dist.}}$	-4.92E-3*	2.80E-3	-2.71E-3	2.90E-3	-2.37E-3	2.72E-3
β_{price}	-2.07E-3***	2.97E-4	-2.41E-3***	3.36E-4	-1.71E-3***	3.20E-4
$\beta_{\text{price}\times\text{dist.}}$	-6.80E-6*	3.81E-6	-7.25E-6*	4.29E-6	-2.14E-5***	4.56E-6
σ_{ASC}	5.39***	0.473	5.18***	0.438	3.72***	0.278
σ_{d18}	1.62***	0.193	1.52***	0.204	1.48***	0.193
Number of observations:		2172		1986		2070
Number of individuals:		362		331		345
Number of draws:		500		500		500
Final log-likelihood:		-1276.938		-1228.323		-1346.586

Note: *** = significant on 99% level, ** = significant on 95% level and * = significant on 90%.

Table G.2: Estimation results of the spatial model (Samples weighed as oor-sample)

Variable	nonct sample Parameter estimate	Std. error	ct sample Parameter estimate	Std. error	oor sample Parameter estimate	Std. error
ASC	0.247	0.308	0.645*	0.332	0.491*	0.237
β_{d18}	0.506***	0.155	0.183	0.152	0.357**	0.148
β_{d50}	1.02***	0.224	0.868***	0.226	0.884***	0.212
$\beta_{\text{d50}\times\text{dist.}}$	-3.64E-3	2.73E-3	-2.65E-3	2.83E-3	-2.66E-3	2.62E-3
β_{price}	-2.30E-3***	2.99E-4	-2.28E-3***	3.25E-4	-1.80E-3***	3.10E-4
$\beta_{\text{price}\times\text{dist.}}$	-4.87E-6	3.72E-6	-9.05E-6**	4.23E-6	-1.96E-5***	4.33E-6
σ_{ASC}	5.37***	0.451	5.17***	0.421	3.68***	0.270
σ_{d18}	1.64***	0.188	1.50***	0.196	1.44***	0.188
Number of observations:		2172		1986		2070
Number of individuals:		362		331		345
Number of draws:		500		500		500
Final log-likelihood:		-1364.806		-1312.453		-1391.160

Note: *** = significant on 99% level, ** = significant on 95% level and * = significant on 90%.

H Male spatial models with different sample weights

Table H.1: Estimation results of the spatial model (Male samples weighed as ct-sample)

	nonct sample		ct sample		oor sample	
Variable	Parameter estimate	Std. error	Parameter estimate	Std. error	Parameter estimate	Std. error
ASC	0.950*	0.525	0.410	0.488	0.483	0.330
β_{d18}	0.639***	0.227	0.174	0.234	0.369*	0.202
β_{d50}	1.48***	0.322	0.772**	0.316	1.01***	0.280
$\beta_{\text{d50}\times\text{dist.}}$	-8.06E-3**	4.00E-3	-7.03E-4	3.89E-3	-4.39E-3	3.64E-3
β_{price}	-1.83E-3***	3.93E-4	-1.58E-3***	4.08E-4	-1.34E-3***	4.06E-4
$\beta_{\text{price}\times\text{dist.}}$	-9.03E-6*	5.30E-6	-1.20E-5**	5.43E-6	-2.58E-5***	6.19E-6
σ_{ASC}	5.97***	0.776	5.27***	0.632	3.82***	0.383
σ_{d18}	1.76***	0.280	1.75***	0.288	1.44***	0.253
Number of observations:		1110		1008		1152
Number of individuals:		185		168		192
Number of draws:		500		500		500
Final log-likelihood:		-635.620		-659.076		-739.197

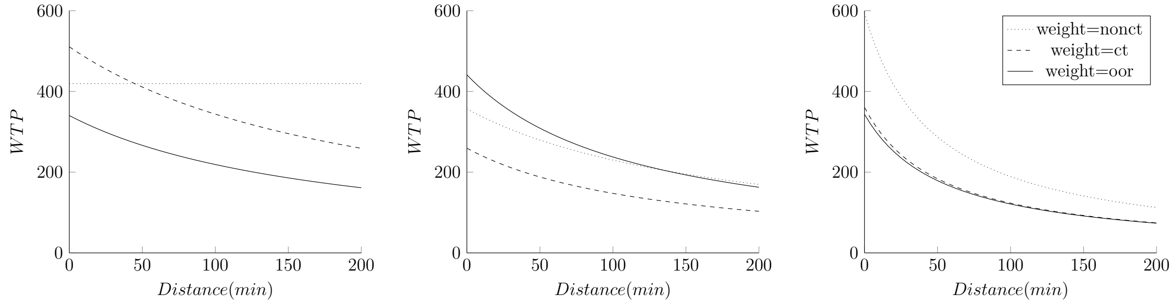
Note: *** = significant on 99% level, ** = significant on 95% level and * = significant on 90%.

Table H.2: Estimation results of the spatial model (Male samples weighed as oor-sample)

	nonct sample		ct sample		oor sample	
Variable	Parameter estimate	Std. error	Parameter estimate	Std. error	Parameter estimate	Std. error
ASC	0.657	0.464	0.675	0.434	0.460	0.323
β_{d18}	0.692***	0.222	0.086	0.236	0.393*	0.197
β_{d50}	1.48***	0.321	0.680**	0.309	1.04***	0.275
$\beta_{\text{d50}\times\text{dist.}}$	-8.30E-3**	4.06E-3	-4.18E-4	3.86E-3	-5.15E-3	3.56E-3
β_{price}	-1.93E-3***	4.15E-4	-1.53E-3***	4.00E-4	-1.34E-3***	3.92E-4
$\beta_{\text{price}\times\text{dist.}}$	-1.07E-5*	5.67E-6	-1.31E-5**	5.41E-6	-2.46E-5***	5.96E-6
σ_{ASC}	5.85***	0.707	5.58***	0.663	3.77***	0.373
σ_{d18}	1.72***	0.275	1.87***	0.281	1.41***	0.248
Number of observations:		1110		1008		1152
Number of individuals:		185		168		192
Number of draws:		500		500		500
Final log-likelihood:		-683.844		-695.682		-758.978

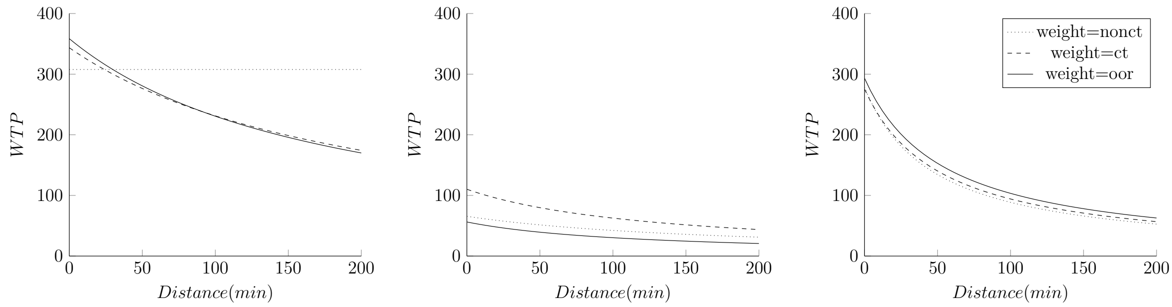
Note: *** = significant on 99% level, ** = significant on 95% level and * = significant on 90%.

I WTP with varying sample weights: male



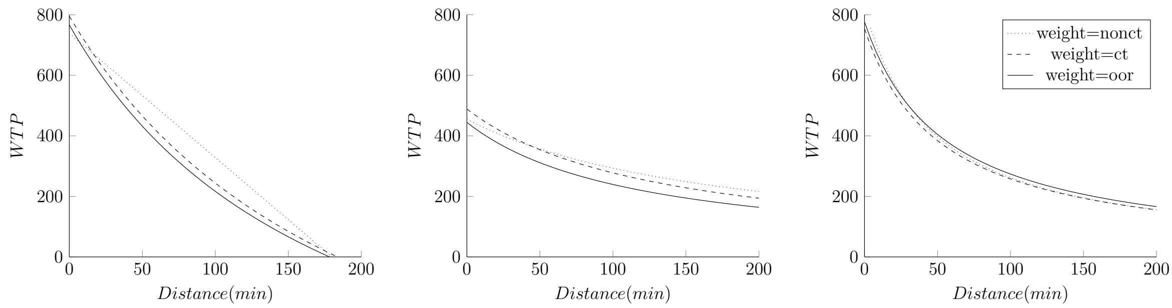
(a) nonct *(b)* ct *(c)* oor

Figure I.1: Willingness to pay estimates (in DKK) for the alternative-specific constant for male sample with different sample weights



(a) nonct *(b)* ct *(c)* oor

Figure I.2: Willingness to pay estimates (in DKK) for the 18 km attribute for male sample with different sample weights



(a) nonct *(b)* ct *(c)* oor

Figure I.3: Willingness to pay estimates (in DKK) for the 50 km attribute for male sample with different sample weights

J Female spatial models with different sample weights

Table J.1: Estimation results of the spatial model (Female samples weighed as ct-sample)

	nonct sample		ct sample		oor sample	
Variable	Parameter estimate	Std. error	Parameter estimate	Std. error	Parameter estimate	Std. error
ASC	0.180	0.422	0.533	0.475	0.559	0.346
β_{d18}	0.269	0.223	0.227	0.214	0.223*	0.238
β_{d50}	0.807**	0.331	1.04***	0.358	0.707***	0.348
$\beta_{\text{d50}\times\text{dist.}}$	-1.47E-3	3.99E-3	-4.84E-3	4.46E-3	4.74E-4	4.20E-3
β_{price}	-2.38E-3***	4.64E-4	-3.54E-3***	6.12E-4	-2.34E-3***	5.33E-4
$\beta_{\text{price}\times\text{dist.}}$	-4.50E-6	5.77E-6	-1.20E-5	7.33E-6	-1.55E-5**	7.13E-6
σ_{ASC}	4.81***	0.570	5.21***	0.620	3.47***	0.390
σ_{d18}	1.52***	0.281	1.16***	0.317	1.59***	0.302
Number of observations:		1062		978		918
Number of individuals:		177		163		153
Number of draws:		500		500		500
Final log-likelihood:		-635.952		-556.481		-601.746

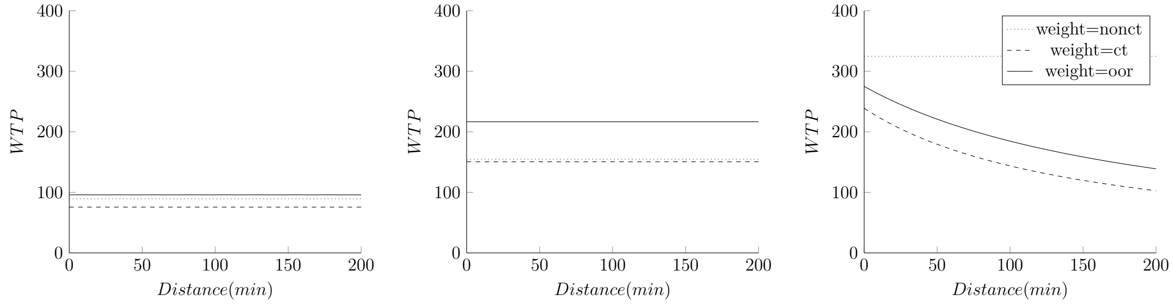
Note: *** = significant on 99% level, ** = significant on 95% level and * = significant on 90%.

Table J.2: Estimation results of the spatial model (Female samples weighed as oor-sample)

	nonct sample		ct sample		oor sample	
Variable	Parameter estimate	Std. error	Parameter estimate	Std. error	Parameter estimate	Std. error
ASC	0.256	0.416	0.702	0.485	0.718**	0.340
β_{d18}	0.341	0.216	0.239	0.197	0.243	0.230
β_{d50}	0.553*	0.319	1.07***	0.343	0.668**	0.340
$\beta_{\text{d50}\times\text{dist.}}$	-7.86E-4	3.74E-3	-6.26E-3	4.32E-3	5.99E-4	4.01E-3
β_{price}	-2.67E-3***	4.46E-4	-3.24E-3***	5.90E-4	-2.61E-3***	5.23E-4
$\beta_{\text{price}\times\text{dist.}}$	-1.40E-7	5.21E-6	-5.23E-6	7.33E-6	-1.28E-5*	6.76E-6
σ_{ASC}	4.84***	0.564	5.01***	0.580	3.50***	0.379
σ_{d18}	1.51***	0.264	0.97***	0.321	1.58***	0.284
Number of observations:		1062		978		918
Number of individuals:		177		163		153
Number of draws:		500		500		500
Final log-likelihood:		-674.710		-596.938		-624.587

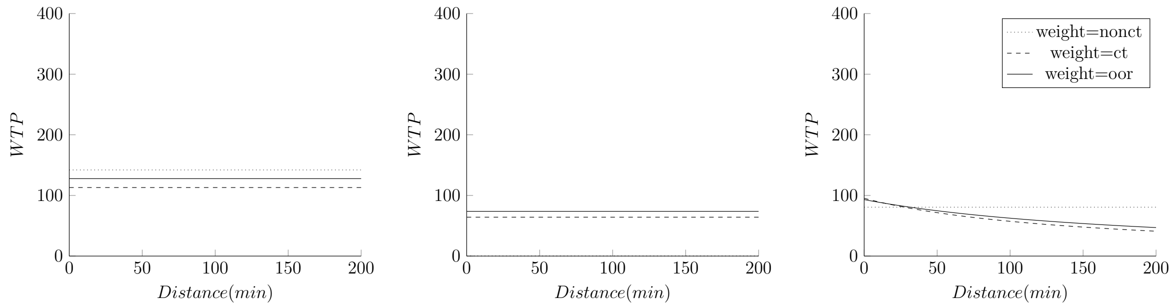
Note: *** = significant on 99% level, ** = significant on 95% level and * = significant on 90%.

K WTP with varying sample weights: female



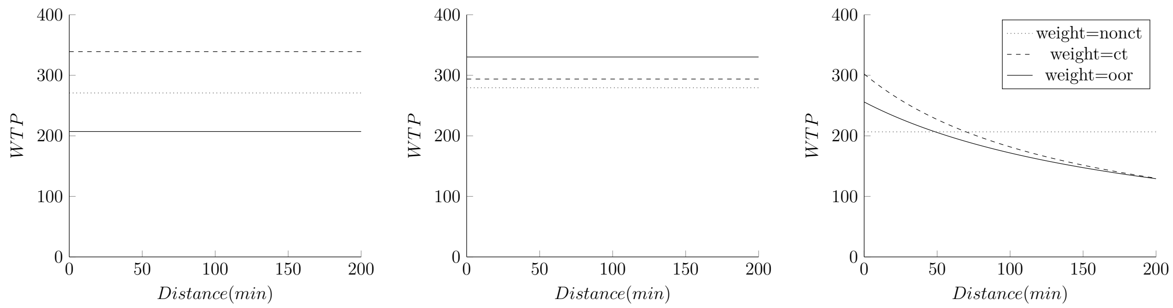
(a) nonct *(b)* ct *(c)* oor

Figure K.1: Willingness to pay estimates (in DKK) for the alternative-specific constant for female sample with different sample weights



(a) nonct *(b)* ct *(c)* oor

Figure K.2: Willingness to pay estimates (in DKK) for the 18 km attribute for female sample with different sample weights



(a) nonct *(b)* ct *(c)* oor

Figure K.3: Willingness to pay estimates (in DKK) for the 50 km attribute for female sample with different sample weights

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