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Behavioral Economics and Public Policy: The Case of Green Electricity Defaults

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Zurich, September 2017 Claus Ghesla

Disclaimer

In accordance with the 'Ordinance on Doctoral Studies at the Swiss Federal Institute of Technology Zurich' (Ordinance on Doctoral Studies ETH Zurich 414.133.1) this thesis was written as a *cumulative* doctoral thesis.

It includes one article of which the candidate is the sole author (Chapter 2), and two articles of which the candidate is the first author (Chapter 3 and Chapter 4). The individual contribution of the candidate to each of the articles has been declared in the authorship declaration and confirmation form, which has been handed to the rectorate upon registration for the doctoral examination. Please note that Chapters 1 (Introduction) and 5 (Conclusion) use the singular personal pronoun for consistent readability of the text. Nevertheless, I greatly acknowledge the co-authorship of Manuel Grieder, Jan Schmitz, and Renate Schubert in Chapter 3 and respectively Chapter 4.

Note that the three chapters have been separately prepared for the purpose of standalone scientific publication in specialist journals in the academic field of the thesis. It is, therefore, inevitable to have certain repetitions of content, arguments and citations, especially in the introductive and concluding parts of each chapter.

Eventually, as this research project was supported by the Swiss Federal Office of Energy (SFOE) under the research program 'Energy - Economy - Society (EES)' (contract number: SI/501109-01), the articles submitted in this thesis also form an integral part of a final technical report to the funding body. At the effective date of the submission of this doctoral thesis (29^{th} of September 2017), the final technical report has not yet been published. Hence, no direct citation of this report can be provided. The interested reader may access the technical reports of the SFOE at their web-page (Energy research database).

Abstract

This thesis examines the design, the distributional costs and benefits, and the behavioral consequences of applying 'green' electricity defaults in residential electricity markets. The three main chapters represent of a set of closely related, but self-contained papers that build on methods and concepts applied in behavioral and environmental economics. In particular, the analyses presented in this work are based on experimental and empirical approaches towards the different research questions at hand.

In Chapter 2, I analyze whether the choices imposed by defaults actually match with individuals' preferences in case of no default. Despite the wide-spread application of green electricity defaults, there is only a limited understanding under which conditions defaults actually help consumers to act according to their preferences. Using two design elements from existing electricity markets, costly opt out of contracts and green electricity certificates, preferences for specific electricity mixes were elicited in an incentivized laboratory experiment. This chapter then assesses whether choices in the absence of default options match with preferences in the presence of default options. It turns out that the alignment of default intentions and 'pure' individual preferences depends on the relative price of green electricity. Green electricity defaults at low relative prices, as currently applied in several electricity markets, do not match subjects' preferences. In contrast, green electricity defaults at high relative prices match with subjects' preferences. These findings offer some guidance on how to design green electricity defaults that could achieve a double dividend for public policy making: match individual preferences and increase the demand for green electricity.

Chapter 3 assesses whether green electricity defaults have diverging costs and benefits for different groups in society. There is limited evidence on which segments of a population

are particularly prone to stick to defaults and why. Given that defaults could potentially mismatch with people's preferences, the application of such behavioral interventions may have considerable impacts on individual welfare. This chapter reports results from a field study in which the authors contrast the choice for an electricity contract under a default regime (with the default being an intermediately green and intermediately expensive contract) with an active choice without any default. The results show that the default distorts decisions in different directions. On the one hand, the default leads pro-environmentally minded, but uninformed consumers to choose less environmentally-friendly contracts. On the other hand, it also leads poorer households to choose options that are more costly than the ones they prefer in the active choice. Both effects have important implications for consumer welfare. A simple cost-benefit analysis reveals that the green electricity default is successful at curbing greenhouse gas emissions. However, the costs for this emission abatement seem to be rather high when compared to some recent estimates of the social cost of carbon. These results highlight the need for a careful evaluation of nudging interventions applied in public policy making.

Chapter 4 aims at scrutinizing potential behavioral effects of choice defaults, which reach beyond their direct impact on decision making. Choice defaults are remarkably successful when judged by their effects on the targeted behaviors in isolation. However, there is scant knowledge about possible spillover effects of defaults on subsequent related choices. Theoretically, such behavioral spillover effects could amplify, eliminate or reverse initially positive effects of choice defaults. This chapter reports results from a laboratory experiment that explores the behavioral consequences of nudging people into a pro-social behavior via the use of choice defaults. The results show that a well-intended choice default stimulating initial 'good' behavior does not interfere with subsequent 'other' individual choices. These findings carry a positive message for policy makers fearing adverse consequences from the use of choice defaults.

The key contributions of this thesis highlight the issues of the applicability and of the design of green electricity defaults in residential electricity markets, examine potential costs and benefits of choice defaults, and address whether choice defaults may come with adverse consequences in subsequent behavior. The research presented here enhances the

understanding of potential benefits and ramifications of green electricity defaults from different perspectives, thereby adding to the growing literature in the field of behavioral environmental economics. Additionally, it carries important messages for an economically and socially tenable design and for the use of behaviorally-oriented instruments in public policy making.

Zusammenfassung

Die vorliegende Dissertation setzt sich mit der Ausgestaltung, den Kosten und Nutzen, sowie mit den Konsequenzen auf nachgelagertes Verhalten von 'grünen' Standardstromverträgen (so genannten *Defaults*) auseinander. Defaults werden vermehrt als Instrumente zur Verhaltensänderung von Konsumentinnen und Konsumenten in Endkundenenergiemärkten eingesetzt. Im Vordergrund stehen dabei zwei politische Ziele. Einerseits soll die Nachfrage nach 'grünem' Strom, d.h. Strom aus erneuerbaren Energiequellen erhöht werden, andererseits soll den Verbraucherinnen und Verbrauchern die freie Wahl eines Vertrages erhalten bleiben.

Die drei Hauptkapitel dieser Arbeit umfassen thematisch eng verwandte, jedoch unabhängige und in sich abgeschlossene Forschungsartikel, welche sich methodisch und konzeptionell an Arbeiten der Verhaltens- und Umweltökonomie anlehnen. Die Analyse der Forschungsfragen erfolgt dabei mittels experimenteller und empirischer Methoden.

Im ersten Forschungspapier (Kapitel 2) untersuche ich, ob sich die individuellen Präferenzen für einen Stromvertrag bei einer Wahl mit und ohne Standardstromvertrag unterscheiden. Viele Energieversorger bieten heutzutage grüne Standardstromverträge für ihre Kundinnen und Kunden an. Bislang ist jedoch unklar, welche Ausgestaltung eines solchen Standardvertrages für eine Übereinstimmung mit den Präferenzen der Verbraucherinnen und Verbraucher sorgen kann. In einem ökonomischen Laborexperiment werden die Präferenzen für einen Elektrizitätsmix, d.h. die Präferenzen für die Zusammensetzung des Stromes aus unterschiedlichen Produktionsquellen, mit und ohne Standardstromvertrag gemessen. Das experimentelle Design verwendet dabei reale Merkmale von Endkundenenergiemärkten: (1) es entstehen Kosten für das Wechseln des Standardvertrages und (2) nachgefragte Mengen an grünem Strom werden tatsächlich mittels so

genannter Herkunftsnachweise im Markt produziert. Die Analyse zeigt, dass Standardstromverträge je nach dem relativen Preis von Strom aus erneuerbaren Energiequellen unterschiedlich den Präferenzen der Teilnehmenden entsprechen. Insbesondere bei tiefen relativen Preisen entsprechen individuelle Entscheidungen welche in einer Wahl mit grünen Standardstromverträgen getroffen werden nicht den Präferenzen der Teilnehmenden gemäss einer aktiven Wahl. Die Befunde des zweiten Kapitels helfen eine sinnvolle Ausgestaltung von grünen Standardstromverträgen vorzunehmen, so dass diese sowohl den individuellen Präferenzen entsprechen, als auch einen hohen Anreiz für eine vermehrte Nachfrage nach Strom aus erneuerbaren Energiequellen mit sich bringen.

Im zweiten Forschungspapier (Kapitel 3) wird untersucht, ob sich grüne Standardstromverträge nachteilig auf unterschiedliche Gruppen der Gesellschaft auswirken können. Generell existiert nur sehr wenig Evidenz darüber, welche Individuen oder Gruppen besonders 'anfällig' dafür sind, bei einem Standardstromvertrag zu bleiben. Ebenfalls ist es unklar, weshalb diese Gruppen verstärkt im Default bleiben. Entsprechen grüne Defaults nicht den eigentlichen Präferenzen von Haushalten, kann die Verwendung solcher verhaltensorientierter Politikinstrumente negative Wohlfahrtseffekte haben. In einer Feldstudie mit Konsumentinnen und Konsumenten eines Schweizer Energieversorgers wird erhoben, ob die aktiv offenbarte Präferenz für einen Stromvertrag der gegenwärtigen Vertragswahl in einem Regime mit einem grünen Standardstromvertrag entspricht. Der Standardstromvertrag ist hierbei durch den Energieversorger in der Mitte der zur Wahl stehenden Verträge platziert, d.h. Konsumentinnen und Konsumenten können sowohl noch grünere (und teurere), als auch weniger umweltfreundlichere (und billigere) Verträge wählen. Die Analyse zeigt, dass der Standardstromvertrag die Entscheidungen von Haushalten in unterschiedliche Richtungen 'verzerrt'. Einerseits führt der Default dazu, dass Haushalte mit starken Präferenzen für die Umwelt, welche sich jedoch zu wenig über ihre Wahl informiert haben, nicht zu einem noch umweltfreundlicheren Vertrag wechseln. Anderseits führt der Default dazu, dass Haushalte mit geringem Einkommen und schlechter formaler Ausbildung im Standardstromvertrag bleiben, obwohl sie eine klare Präferenz für eine günstigere Option haben. Beide Befunde ermöglichen es erstmals, eine Diskussion über die Verteilungswirkungen von grünen Standardstromverträgen zu führen. In einer

einfachen Kosten-Nutzen-Analyse zeigt sich, dass der Default zwar, wie geplant, erfolgreich den Ausstoss von Treibhausgasen reduziert, jedoch wird diese Emissionsminderung, verglichen mit gängigen Schätzungen der gesellschaftlichen Kosten für diese Emissionen, zu einem relativ hohen Preis erkauft. Diese Resultate unterstreichen den Bedarf einer sorgfältigen Abschätzung der Kosten und Nutzen im Zusammenhang mit der Verwendung von verhaltensökonomischen Instrumenten im Bereich der Umweltpolitik.

Das dritte Forschungspapier (Kapitel 4) beschäftigt sich mit der Frage, ob Defaults Auswirkungen auf Verhaltensweisen haben können, welche nicht in direktem Zusammenhang mit der ursprünglichen Entscheidung stehen. Der Default-Effekt, d.h. dass viele Personen diejenige Option wählen, welche als Standard vorgegeben wird, ist in der Literatur sehr gut dokumentiert. Jedoch gibt es sehr viel weniger Erkenntnisse darüber, ob die Verwendung eines Defaults möglicherweise auch auf nachgelagerte Entscheidungen einen Einfluss haben kann. Diese so genannten 'spillover'-Effekte könnten—theoretisch—die positiven Wirkungen eines Defaults verstärken, eliminieren oder auch umkehren. Von besonderem Interesse in der Umweltökonomie sind vor allem negative 'spillovers', d.h. 'nachgelagerte' Verhaltensweisen, welche anfänglich positive Umwelteffekte in gesamt gesehen negative Effekte umkehren. In einem ökonomischen Laborexperiment werden die nachgelagerten Konsequenzen im Verhalten von Individuen erforscht, welche durch einen Default zu erhöhtem pro-sozialen Verhalten motiviert werden. Die Befunde zeigen, dass der Default den gewünschten positiven Effekt hat und dass keine negativen Auswirkungen auf nachgelagerte Entscheidungen erkennbar sind. Diese Resultate sind gegenüber einer stringenten Kontrolle von Einkommenseffekten und von Effekten aufgrund altruistischer Motive im Zusammenhang mit den nachgelagerten Entscheidungen stabil. Diese Befunde sind insoweit interessant, da nicht zu befürchten ist, dass die Verwendung von pro-sozialen Defaults zu nachteiligem nachgelagertem Verhalten führt.

Insgesamt gesehen, untersucht die vorliegende Dissertation die Ausgestaltungs- und Anwendungsmöglichkeiten grüner Standardstromprodukten in Endkundenenergiemärkten. Zusätzlich beleuchtet die Arbeit die Kosten und Nutzen einer verhaltensökonomisch motivierten Umweltpolitik für verschiedene Gruppen der Gesellschaft. Schliesslich addressiert der Text die Frage, ob in der Anwendung von Defaults mit nachteiligem Verhalten bei nachfolgenden Entscheidungen zu rechnen ist.

Die hier vorgestellten Forschungsarbeiten tragen zu einem besseren Verständnis möglicher Herausforderungen und Chancen grüner Standardstromprodukte bei. Diese Dissertation leistet damit einen Beitrag zur wachsenden wissenschaftlichen Literatur im Bereich der verhaltensorientierten Umweltökonomie. Die hier vorgestellten Forschungspapiere geben zudem Impulse für eine ökonomische und sozial verträgliche Ausgestaltung und Verwendung von verhaltensorientierten Massnahmen in der Umweltpolitik.

Chapter 1

Introduction

Under 'ideal' conditions free market economies provide institutions that lead to an efficient allocation of resources.[1] However, often markets fall short of this ideal. For instance, many goods and services that are provided by the natural environment classify as public goods, i.e., their consumption is non-excludable and non-rivalrous, such as clean air. The existence of public goods generates individual behavior that frequently hinders Pareto optimal market outcomes, i.e., there are failures in the efficient allocation of resources. These market failures call for governmental intervention and highlight the need for well-designed incentives to guide individual decision making.

The economic discipline has a rich tradition in informing public policy makers on how to design interventions that are capable of achieving Pareto optimal market outcomes. For instance, market failures could be addressed by command-and-control regulation, such as standards or mandates (e.g., 'The Clean Air Act' in the North America, US Environmental Protection Agency, 2017), or by priced-based solutions, such as Pigouvian taxes or tradeable permit schemes (e.g., 'Emissions Trading Scheme' in the EU, European Commission, 2017b). However, the success of such 'traditional' economic interventions commonly relies on rational and fully informed individuals to take actions.

Beginning with the advances of Simon (1959), Kahneman and Tversky (1979), and Thaler (1980), economic research has started to integrate psychological elements into its

[1] The ideal conditions relate to the first fundamental theorem of welfare economics, i.e., given fully competitive markets, fully informed agents, full issuance of property rights, no externalities, and no public goods, a market will reach an allocative equilibrium that is Pareto optimal; for a formalization of the theorem see Mas-Colell, Whinston, and Green (1995, Chapter 16: Equilibrium and Its Basic Welfare Properties).

models in order to account for potential limitations in rational human behavior. A vivid sub-discipline coined 'behavioral economics' has since developed and has gained attention by policy makers and the general public. In particular, the seminal contributions of Thaler and Sunstein (2003) ('Libertarian Paternalism')[2] and Camerer, Issacharoff, Loewenstein, O'Donoghue, and Rabin (2003) ('Asymmetric Paternalism') have paved the way for the application of 'libertarian' or 'soft' paternalism in addition to the traditional tools used in public policy making to correct for market failures. The central premise of these concepts is to help individuals to make a decision based on their own interests, without infringing on the initial choice set, therefore still preserving the freedom to choose.

Governments across the globe have been keen to set up 'behavioral insights teams' or 'nudge units', which seek to understand and guide decision-making of citizens by applying findings from behavioral sciences to the respective policy frameworks.[3] A favored and widely applied concept in the toolkit of policy instruments inspired by libertarian paternalism has been a default option in a choice set. Such defaults automatically assign a preset choice to individuals, unless they opt out and select another alternative. This simple alteration of the choice set has demonstrably powerful effects, as most individuals remain with the option presented as default. Evidently, choice defaults have proven to be effective—inter alia—for improving personal wealth by increasing saving rates in retirement schemes (for instance Choi, Laibson, Madrian, and Metrick, 2003; Cronqvist and Thaler, 2004; Madrian and Shea, 2001), for increasing organ donation rates (Johnson and Goldstein, 2003), for enhancing charitable giving (Altmann, Falk, Heidhues, and Jayaraman, 2014), or for stimulating elevated contributions to public goods (for instance Altmann and Falk, 2009; Carlsson, Johansson-Stenman, and Khanh Nam, 2015; Cappelletti, Mittone, and Ploner, 2014).

With pressing needs to curb natural resource depletion (United Nations Environment Programme (UNEP), 2012) and global warming (Intergovernmental Panel on Climate

[2] This contribution later culminated in a popular science book titled: *'Nudge - Improving Decisions About Health, Wealth, and Happiness'* (see Thaler and Sunstein (2008)).

[3] For examples of governmental intervention the interested reader may refer to annual reports of the 'Social and Behavioral Science Team' in the United States (Social and Behavioral Sciences Team, 2016), the annual report by the 'Behavioral Insights Team' in the United Kingdom (Behaviroal Insights Team, 2016), a summary report of the European Commission on applying behavioral insights to policy (Sousa Lorenco, Ciriolo, Rafael Rodrigues Vieira de Almeida, and Troussard, 2016), or the regulatory policy outlook of the Organization for Economic Co-operation and Development (OECD) (2015).

Change (IPCC), 2014), regulative policy has also started to make use of libertarian paternalism in order to enhance more environmentally friendly decision-making of households. The energy sector has attracted particular attention, as the emissions produced by heating/cooling and using electricity in general sum up to nearly 70% of the total emissions contributing to anthropogenic climate change (International Energy Agency (IEA), 2016). The guiding insight that lays the foundation for the application of behaviorally-oriented interventions is that there seem to be differences in what people state to prefer and what they actually do—a notion termed the 'value-action gap' (Ajzen, 1985; Eden, 1996). Specifically and most relevant for the topic of this thesis, people seem to have positive preferences for purchasing 'green electricity' (see e.g., Soon and Ahmad, 2015),[4] however, they often fail to choose greener electricity contract options. For this reason, policy makers, regulators and utilities have increasingly used green electricity default options to channel households' choices towards more environmentally friendly electricity contracts.[5]

The rationale for green electricity defaults is to increase the relative share of green power demand with respect to the total demand for electricity and, hence, generate socially beneficial effects on climate and energy policy targets, while "maintaining freedom of choice and respect for [heterogeneous consumer preferences]" (Sunstein and Reisch, 2014, p. 132). It is less debated whether a higher share of green electricity contracts paves the way for an internalization of negative externalities caused by producing electricity from fossil or nuclear sources. However, it remains unclear whether the benefits of a choice architecture involving green electricity defaults outweigh potential 'behavioral costs'. This latter question constitutes the prime subject matter of this thesis.

The analyses presented in this thesis specifically examine whether choices under a green electricity default regime actually match people's preferences, whether they have undesired distributional effects for different groups in societies and whether well-intended nudges may inadvertently spill over negatively into subsequent behavior. The studies presented here contribute to the literature in behavioral environmental economics addressing questions of

[4] Throughout this thesis green electricity is refereed to electricity predominately produced by renewable sources, such as wind, solar, hydro, or biomass. Conventional electricity is produced by fossil or nuclear sources.

[5] For instance, in Zurich (the city this thesis was written in) the local utility changed the default electricity contract to a 100% green option, thus affecting the choices of roughly 200,000 households.

the design, the appropriateness and the consequences of behavioral nudges for public policy making in residential electricity markets. Given that governments, regulators, and utilities increasingly make use of such instruments to guide decision-making of households in these markets,[6] this research tackles publicly salient issues and is highly policy relevant. The remainder of this chapter lays out the motivation and practical relevance of this research, provides a primer on the functioning of residential electricity markets and the explicit relevance of default options, introduces the main methods applied throughout the text, and outlines the key research questions as well as the structure of this thesis.

1.1 Relevance and motivation

It is "extremely likely" that anthropogenic greenhouse gas emissions, such as carbon dioxide, are the root cause for climate change inducing a rise in global surface temperatures (Intergovernmental Panel on Climate Change (IPCC), 2014, p. 4). Approximately 70% of the annually man-made emissions can be attributed to the sector of energy, which subsumes the combustion of fossil fuels for transport, heating/cooling, and electricity (International Energy Agency (IEA), 2016, p. 9, Figure 1). Recognizing the pressing need for intervention, the United Nations Framework Convention on Climate Change (UNFCCC) (2015) has recently concerted in a common agreement to keep global warming "well-below 2 degrees Celsius" in the 21^{st} century.[7] This agreement includes and requests, amongst other things, that governments need to develop policy measures, which are capable of de-carbonizing the energy sector, i.e., replacing the use of fossil energy with renewable energy sources, such as hydro, solar, or wind power.

In this vein, many countries have stipulated targets to increase the share of green electricity. For instance, in Switzerland regulators aim at quadrupling the current share of 4%

[6] It seems likely that the number of behaviorally-oriented interventions in energy markets will rise as, for example, the European Commission (2015, p. 2) has set out in a vision of a common energy strategy that "citizens take ownership of the energy transition", [...] and "participate actively in the market". Likewise, in the United States the Federal Government calls for more people centered policies, which "reflect our best understanding of how people engage with, participate in, use, and respond to those policies and programs", [...], and which help "accelerating the transition to a low-carbon economy" (The White House, 2015).

[7] The so-called 'Paris Agreement' is already in force since the 5^{th} of October 2016, as more than 55 countries, producing 55% of the total greenhouse gas emissions, have ratified the accord (United Nations Framework Convention on Climate Change (UNFCCC), 2016).

of *new* renewable electricity, i.e., electricity produced by wind, solar or biomass, by the year 2035 (Federal Department of the Environment, Transport, Energy and Communications (DETEC), 2017). In Europe the 'EU renewable energy directive' requires member states to meet a target of 33% or renewable electricity production by 2020 (European Commission, 2016b). In the United States, 29 states have adopted 'renewable portfolio standards' with differing requirements and time horizons to reach them (National Conference of State Legislatures (NCSL), 2017). Likewise, emerging economies such as China are heavily investing in programs to fuel their electricity grids with green electricity production sources (for an overview see Cherni and Kentish, 2007). Much of the efforts to change the existing production portfolio have been focused on supply side policy measures, like for instance, supportive feed-in-tariffs for renewable electricity production or the build up of funds to finance large scale renewable production sites.[8]

On the demand side, residential electricity markets have evolved since the 1990s from a 'one-contract-fits-all' system to more sophisticated markets with a profound level of retail choices for consumers (see e.g., Joskow, 2006). The choice of retailers has not only accomplished a vast amount of competition between utilities, but it has also mobilized direct consumers' interest for green electricity by offering different green electricity plans or contracts at different prices. The Agency for the Cooperation of Energy Regulators (ACER) (2014) reports, for example, that in the European Union 280 electricity suppliers offer more than 690 different green electricity tariffs. Likewise, in the United States 850 utilities have green electricity programs, which offer customers to replace their conventional shares of electricity with shares of green electricity (US Department of Energy, 2016).

Regardless of these steps towards liberalization, and although empirical evidence points towards an overall positive additional willingness-to-pay for green electricity (Soon and Ahmad, 2015), customers seem to be reluctant to 'execute' their preferences. Morey and Kirsch (2016, p. 63) point out, for instance, that most customers in the United States "remain with their incumbent utilities [and with their electricity contracts] regardless of competitors' efforts". The same state of inertia can be observed for European customers, as 90% of households have remained in their existing contracts with the same underlying

[8] The interested reader is referred to Gan, Eskeland, and Kolshus (2007) for an overview of supply side measures, as this topic is beyond the scope of this thesis.

electricity mixes (Agency for the Cooperation of Energy Regulators (ACER), 2014). This gap in people's intentions and their realized actions provides the 'playing field' for interventions and regulations in the sense of libertarian paternalism. Specifically, the choice of default contracts has become a decisive instrument in residential electricity markets. Considering the fact that today consumers' choice for electricity is very often surrounded by a default contract,[9] the electricity mix of the default contract, i.e., how much conventional and green electricity is used, significantly influences whether a higher share of green electricity is demanded.

Recent experimental evidence underscores this point by showing that the switch from a conventional electricity default contract to a green default electricity contract has a considerable positive impact on the demand for green electricity (Ebeling and Lotz, 2015; Pichert and Katsikopoulos, 2008).

However, even this putative simple switch of defaults may pose challenging questions in terms of whether the nudge fulfills all of its policy goals. Precisely, switching to a green default seems largely to be based on two distinctive argumentative channels. The first argument makes clear that green electricity defaults have the potential to successfully raise green electricity demand and may thus be capable of mitigating external damages from carbon emissions. There is little debate about this argument. The second argument suggests that the switch to a green default is of additional use to consumers as it helps them overcome their inertia to switch towards their preferred alternatives (Rebonato, 2014). To speak in the words of Thaler and Sunstein (2008, p. 5, italics in original), nudges are used to "make choosers better off, *as judged by themselves*". It remains unclear whether and under which conditions this latter rationale holds. This question forms the motivational starting point for the work presented in this thesis.

Chapter 2 of this text aims at scrutinizing whether green electricity defaults may bring along the assumed implicitness of being beneficial for the public good, while simultaneously also matching consumers' preferences. By conducting a economic laboratory experiment, I show that this preference match is not unequivocally granted and that the number of

[9] By law most regulators, such as for instance in the European Union (European Commission, 2016a) or the United States, grant access to the electricity grid without households necessarily having to choose a contract.

preference-matching decisions crucially depends on the relative price for green electricity. Based on the results obtained, this chapter provides insights for a design of defaults in green electricity markets that would indeed foster a match in preferences, while also enhancing the share of renewable electricity. The current application of relatively low-priced green default electricity contracts does not seem to meet these premises.

When applying behavioral insights to public policy, it is implicitly assumed that a complaisant and well-informed choice architect is able to design nudges in a way that all people may equally benefit from an intervention.[10] Naturally, every regulation, whether it is behaviorally-oriented or not, has redistributive effects. Certain groups or individuals in society may be more or less favored by a given policy. While economics has a rich tradition in assessing such redistributive effects for 'traditional' tools of regulation, like, for instance, different taxation strategies (see e.g., Judd, 1985), there is to this date scant knowledge of the distributional effects of green electricity defaults for different consumer groups. Chapter 3 of this thesis (co-authored with Manuel Grieder and Renate Schubert) seeks to fill this void. Taking the findings from Chapter 2 to a representative field setting, I examine whether different groups in society are affected differently when applying a green electricity default. The findings in this chapter demonstrate that choice defaults may indeed pose negative consequences for individual welfare. Given an intermediately green and intermediately expensive default contract, I find that the behaviorally-oriented intervention distorts decisions in different directions. On the one hand, the default leads pro-environmentally minded, but uninformed consumers to choose less environmentally friendly contracts. On the other hand, it also leads poorer households to choose options that are more costly than the ones they would actually prefer. Both effects have important implications for consumer welfare. A simple cost-benefit analysis reveals that the green electricity default is successful at curbing greenhouse gas emissions. The costs for this emission abatement, however, seem to be rather high when compared to some recent estimates of the social cost of carbon. These results highlight the need for a careful evaluation of the use of choice defaults in public policy making in residential electricity markets.

[10] The term choice architect in this case may subsume decision makers in an incumbent utility, or a government agency having oversight over electricity tariffs.

The literature on behaviorally inspired policy interventions has so far been mainly focused on the effectiveness of behaviorally motivated interventions in isolation of any preceding or subsequent behavior. Chapter 4 (co-authored with Manuel Grieder and Jan Schmitz) extends this limited view and aims at addressing potential behavioral consequences if people have been successfully defaulted into a pro-social, i.e., a 'good' behavior. Generally, the consequences of an initial behavior on a subsequent behavior have been termed 'spillover effects' (Dolan and Galizzi, 2015; Truelove, Carrico, Weber, Raimi, and Vandenbergh, 2014).[11] In the literature there seems to be an empirical ambiguity about the direction of the effect; whereas some argue that there is evidence for 'consistent' behavior, i.e., initial behavior is fortified in subsequent behavior (Cialdini, Trost, and Newsom, 1995), others claim the opposite, i.e., initial good behavior leads to self-licensing of subsequent iniquitous actions (Monin and Miller, 2001; Conway and Peetz, 2012). I show in a tightly controlled economic laboratory setting that being nudged initially into pro-social behavior does not lead to subsequent adverse behavior—a fear that many behavioral environmental economists have. This evidence carries a tentatively positive message for behaviorally guided policy making: tenable designs of choice defaults can be focused on the behavior in question only and do not cause negative spillover effects.

1.2 Methodological approach

Behaviorally motivated regulation and legislation has embraced an evidence-based approach to policy making (see e.g., Sousa Lorenco et al., 2016; Organization for Economic Co-operation and Development (OECD), 2015). This implies that policy makers and researchers design interventions, test them in markets, assess their outcomes, and re-design the interventions based on their stipulated effectiveness.

This mode of testing and re-testing the effectiveness of treatment variables, i.e., experimentation, is also a relatively recent addition to methods used in economics. In this

[11] Note that the term 'spillover effect' is not used unequivocally in the literature. In this thesis I narrow down spillover effects to the effect of an initial behavior on a related subsequent behavior (such as in Dolan and Galizzi, 2015). Others have embraced the term to explain a 'backfiring' of a policy on the same initial behavior (Schultz, Nolan, Cialdini, Goldstein, and Griskevicius, 2007), to interpret rebound effects, i.e., behavioral alterations due to a change in relative prices (Alcott, 2005) or to elaborate on 'adaptive learning', in which effects of an initial behavior on a subsequent same behavior are considered (Fudenberg and Levine, 1998).

vein, my thesis follows an experimental and empirical approach towards the assessment of potential benefits and costs of a choice architecture involving green electricity defaults.[12]

Roth (1986, pp. 245-246) outlines three purposes of experimentation in economic analysis. First, the method could be used as a means of "testing and modifying formal economic theories". Second, experimentation can be used to deliver "direct input into the policy making process". Third, experiments are also useful in order to "collect data on interesting phenomena and important [economic] institutions".

While much of the theory in (traditional) environmental economics offers rich advice on how to regulate markets in which externalities occur due to individual or collective choices,[13] there is much less theoretical groundwork that generates insights to be used in *behavioral environmental economics*. The reason is that often cognitive or emotional processes behind some observed behavior are not well-understood or compete against each other. This makes the identification of optimal behavioral policy instruments a complex task, as it involves developing non-standard theories for behavior in the first place (see for instance Salant and Rubinstein, 2008; Bernheim, 2009). This thesis, therefore, does not attempt to test economic theories related to environmental decision making.

However, methodologically, this thesis relates to the two latter purposes of economic experimentation. Specifically, I use two economic laboratory experiments (Chapters 2 and 4) to collect data on important mechanisms and institutions when using defaults; plus I use an additional field study (Chapter 3) to examine the potential benefits and costs of green electricity defaults to provide direct input for policy makers in residential electricity markets.

Studying individual preferences (which may later be used to formulate policies) in economic laboratory experiments follows well-established procedures. The founding insight for conducting these types of experiments was formulated by Smith (1976, p. 274, quotation marks in original) stating that "laboratory experience suggests that all of the characteristics of "real world" behavior [...] arise naturally [...] in experimental settings." For letting this happen, we have to adhere strictly to certain rules in order to get people to make

[12] Experimentation in the discipline gained first wide-spread recognition with the contributions of—for instance—2002 memorial Nobel laureate Smith (1976, 1982) or Plott (1979). For a display of the evolution of experimentation in economics see Chapter 1 in the book by Bardsley, Cubitt, Loomes, Moffatt, Starmer, and Sugden (2010).

[13] The interested reader may be referred to the seminal contributions of Pigou (1920), Coase (1960), Baumol and Oates (1971), and Weitzman (1974).

'real' decisions. First and foremost, choices need to have consequences. Monetary incentives that mirror trade-offs between choices are the backbone of economic experimentation. Second, random assignment and stringent control conditions ensure the identification of causal effects, i.e., the impact of a treatment on subject behavior can be unmistakably identified when compared to a behavior in a control condition. Eventually, detailed instructions and the usage of control questions make sure that subjects have understood the tasks and procedures. Deceptive elements are not allowed. Hence, a typical economic experiment provides a decision environment, i.e., a monetary reward structure for each decision. It further sets out institutions, i.e., rules of actions or a decision framework, such as which information is received or which kind of game participants engage with. Varying the reward structure or the rules of actions systematically allows identifying the causal effect of changes in the variables of interest.

In the two laboratory studies presented in this thesis, I use decision environments and institutions that allow scrutinizing individual preferences for pro-environmental and pro-social acts under the influence of a default choice. Specifically, I use Dictator games to assess the marginal rate of substitution for the purchase of a unit of green electricity at different prices (Chapter 2) or to assess subsequent pro-sociality after having been nudged into pro-social behavior (Chapter 4). A Dictator game is a simple institution in an experimental setting, consisting of two types of agents: a proposer and a recipient. The proposer has a certain amount of money and decides how to split this amount between herself and a recipient who cannot put a veto on the proposer's action. Recipients can be other persons within the same experimental session, may constitute charitable organizations (in this frame the experimenter can investigate the pro-social preferences of the proposer), or may represent green electricity funds (in this setting the experimenter can investigate the pro-environmental preferences of the proposers).

Laboratory studies usually employ student samples. The experiments reported in Chapters 2 and 4 of this thesis make use of a participant pool consisting of students both from the University of Zurich and the Federal Institute of Technology in Zurich. Clearly, and especially for studies wanting to inform policy makers, the used student samples pose several questions towards the external validity of the results. Therefore, both Chapters

2 and 4 feature extensive discussions of the experimental design in order to address and potentially alleviate these concerns.[14]

In view of the representativeness of subject pools, the field study presented in Chapter 3 is an important addition to the laboratory work. In this field study I examine choices of a representative set of households of a Swiss city. The analysis presented in chapter 3 is much in line with the standard stream of applied research in environmental economics. Traditionally, this type of research has targeted questions on people's preferences for differing environmental policies via stated-preference research, for instance via conjoint analysis or choice modeling approaches.[15] As I am left unable to incentivize choices in the field study, I made use of a best-practice approach taken from the literature in environmental economics in order to address the concerns arising with hypothetical choices. The study presented in Chapter 3 aims at putting more demographic context to the results obtained in Chapter 2. Yet, the 'price' I pay is the loss of choice incentivation. I specifically discuss this issue in the respective chapter.[16]

Summing up, the analyses presented in this thesis make use of revelatory preference identification when applicable, and use stated preference elicitation when necessary. I aim at providing a balanced selection of methods and data sets that are capable of illustrating my main research questions. The data analysis is carried out with straightforward econometric approaches and models using parametric and non-parametric inference to identify effects. More advanced econometric models are directly introduced in the chapters, when needed.

1.3 Deciding by default

The main premise of a libertarian paternalistic intervention is to make decision makers better off according to their own view. Thus, a choice default shall help individuals to choose their most preferred alternative. The individual behavioral responses to a choice default, therefore, seem to be trivial at first sight. Either individuals stay with the default

[14] See Section 2.3.2 (Chapter 2) and respectively Section 4.3.3. (Chapter 4) for more details.

[15] A detailed discussion of the depth and breadth of literature using these methods is beyond the scope of this thesis. The interested reader may be referred to the methodological manual by Bateman, Carson, Day, Hanemann, Hanley, Hett, Jones-Lee, Loomes, Mourato, Ozdemiroglu, Pearce, Sugden, and Swanson (2002).

[16] See Section 3.4.2 (Chapter 3) for more details.

option and are better off, or individuals select an option that is more preferable to them because the default induces them to opt out.

In order to decide for one of the two behavioral responses, individuals must perform a cost-benefit analysis. On the one hand, staying with the default could come with individual losses due to not selecting an option that is more preferable. On the other hand, opting out of the default could entail decision costs for searching a more suitable alternative choice. Thus, individuals need to trade off the losses incurred by staying with the default, with the costs of choosing an option that is more eligible to them.

The decision rule for either staying or opting out of the default is simply given by the fact that when the decision costs are larger than the losses incurred from staying with the default, individuals stay with the default. Vice versa, if the decision costs are smaller than the loss occurred through a default choice, individuals choose a more suitable option for them (see e.g., Chesterley, 2017; Bernheim, Fradkin, and Popov, 2015). Therefore, theoretically, through the imposition of a default two groups of decision makers emerge, as explained by the two behavioral responses outlined above. The aim of the choice architect is to minimize the losses of the first group (i.e., those who stay with the default), while also minimizing decisions costs for the second group (i.e., those who actively choose another option) in order to design a nudge in the spirit of libertarian paternalism.

Upon closer examination, however, matters become more complex. The reason is that both, the costs to choose a more suitable alternative and the losses of staying with the default could be heterogeneous across individuals. This is because choice defaults seem to matter for individual decision-making for various reasons, which could, on the one hand, have an impact on how individuals perceive the costs for opting out. On the other hand, these reasons could also affect how individuals perceive the losses from staying with the default.

There exist several explanations why green electricity defaults work. A first hypothesis is the relevance of transaction costs, i.e., the costs for searching and identifying other electricity contracts that suit the preferences of the individual (e.g., Schwartz and Scott, 2003). Transaction costs likely differ among individuals. Some may be acquainted with the options available to them, while others need more effort to attain a clearer picture of the market.

A second hypothesis concerns the effects of procrastination, i.e., displacing a decision to an unspecified time in the future. Electricity is an utterly homogeneous good with which customers tend to get rarely involved, which then could lead some individuals to delay their choices. Hence, they will be prone to incur losses from staying with the default rule.

A third explanation centers around the issue that some individuals may not pay sufficient attention to their choice possibilities. Limited attention may be due to high cognitive costs in order to compare different, but similar electricity contracts. Limited attention could also be explained by a reduced salience of a green electricity default. Often, electricity providers do not explicitly advertise the default contract, i.e., individuals could overlook the default option, which might have negative effects on their decision making.

A fourth hypothesis is that individuals tend to stick to the electricity mix they currently possess (so called 'status-quo bias', e.g., Samuelson and Zeckhauser, 1988). The reasons for such behavior is that, given the long-time existence of incumbent utilities and their significant influence on the market structure in residential electricity markets, individuals perceive the default as an implicit recommendation, or as a supposedly 'good' choice (McKenzie, Liersch, and Finkelstein, 2006).

Summing up, given this multitude of explanations why defaults matter for individual decision-making in electricity markets, individuals could struggle to identify the correct behavioral response to a default, i.e., analyzing the 'correct' trade off between staying with or opting out of the default. In turn, choice architects may face difficulties to come up with designs of choice defaults that truly help people to decide in their own interests.

In this respect, Chapter 2 of this thesis assesses whether and under which conditions individuals' active choices for an electricity mix match with the choices taken under a default regime. This analysis provides first evidence on how to design green electricity defaults that let people effectively decide between the two behavioral responses outlined above. A central empirical message of that chapter, which is in-line with the predictions of Chesterley (2017), is that a green electricity default which is costly to the individual,[17] leads to more choices that correspond with the choices in an active choice frame.

[17] In the sense that the default is significantly more expensive than other alternative contracts.

Chapter 3 mainly focuses on the extent of losses that decision makers may be confronted with when staying in the default electricity contract. Concretely, the field evidence presented in this chapter shows that different households may face different forms of losses (monetary and non-monetary ones) due to the default.

Eventually, Chapter 4 assesses whether setting a default with negligible costs to opt out may affect subsequent decision-making. In this way, I can assess whether a nudge that should, theoretically, foster behavioral responses in line with individual preferences,[18] has potential adverse effects on untreated subsequent behavior.

1.4 Structure of the thesis and contribution to research

At the core of this thesis are three closely related, yet independent and self-contained papers (Chapters 2 to 4), followed by a concluding chapter (Chapter 5). Chapters 1, 2, and 5 are the sole contribution of the candidate. Chapters 3 and 4 have been written in co-authorship. Throughout the three separate main chapters, the decision by default and its implications on preference matches, distributional effects, and subsequent behavior is the running theme of the analysis. The corresponding research questions and contributions of the individual chapters are introduced below. Figure 1.1 provides a graphical overview of the main chapters of the thesis.

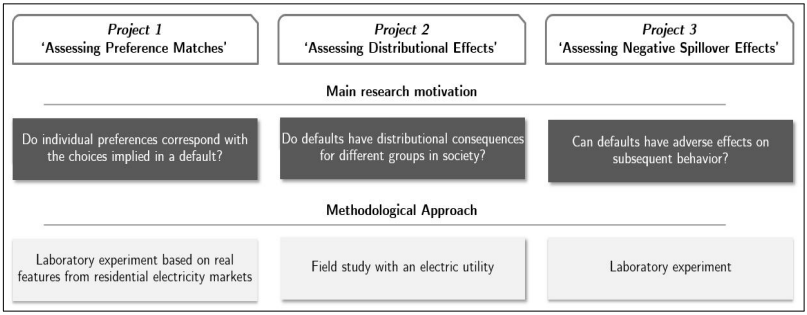

Figure 1.1: Main chapters of the thesis

[18] Note that if the choice architect is able to reduce decision costs to a minimum, individuals should not have difficulties to choose their 'right' behavioral response to the default (Chesterley, 2017).

Chapter 2: Defaults in Green Electricity Markets: Preference Match not Guaranteed

As indicated above, green electricity defaults in residential electricity markets seem to follow at least two intentions. On the one hand, defaults should steer individual choices to environmentally friendly alternatives (Sunstein and Reisch, 2014), thereby changing the demanded electricity mix and reducing harmful emissions. On the other hand, the use of defaults is advocated to help individuals make better decisions from their own point of view (Thaler and Sunstein, 2008), i.e., the use of defaults aims to reach a match with individuals' preferences for electricity mixes. While the former intention receives a positive approval since it helps curbing carbon emissions, the latter intention is much more debated. Proponents of green electricity defaults argue that these behavioral instruments may help overcome systematic biases in individual decision-making. Assuming that individuals have preferences for green electricity, green defaults help households to decide according to their preferences and thus increase welfare. Thus, whether decisions made in a regime with a green electricity default actually match individual preferences, may play an important role for the design of these behavioral instruments. However, despite the wide-spread application of green electricity defaults, there is very limited evidence on whether green defaults in electricity markets lead to choices which actually match individual preferences and thus help improve individuals' welfare. The objective of Chapter 2 is therefore to experimentally investigate whether currently used designs of defaults in residential electricity markets actually correspond with a core principle of libertarian paternalism, i.e., whether the use of defaults leads to choices that match preferences in an active choice frame. Along this line of research, I further address the issue of pricing in green electricity markets. This is of relevance because, as of today, green electricity defaults are only marginally more expensive than other, conventional electricity contracts. However, given that policy makers need to switch to new renewable production technologies, such as solar, wind, or biomass, it seems likely that prices for green electricity defaults increase. Therefore, it is important to address the implications and consequences of relatively higher priced green electricity defaults. In brevity, the guiding questions of Chapter 2 are:

Research Question 1.A: Do currently applied green electricity defaults match individual preferences in residential electricity markets?

Research Question 1.B: What are the implications for the design and the application of defaults when relative prices for green electricity increase?

To address these questions, I use an economic laboratory experiment, which provides a revelatory choice environment in presence and absence of a default. With this setting it is possible to compare choices of fully-optimizing agents in a no default situation with individual choices when a default is present (see Chetty, 2008, for a similar framework). Using two design elements from existing electricity markets, namely costly opt out of contracts and green electricity certificates, I add relevant context to the decision environment and thus re-create a realistic decision setting that grants 'parallelism' of incentives and institutions of an actual residential electricity market (Smith, 1982). The obtained results show that the alignment of default intentions and preferences crucially depends on the relative price for green electricity. Green electricity defaults at low prices, as currently applied in many electricity markets, do not match subjects' preferences. At higher prices, the effects of green electricity defaults are more complex, as they differ depending on the aims of the choice architect. If the choice architect is interested in increasing the share of green electricity, rather than increasing the uptake of 100% green electricity contracts, a higher priced green electricity default fulfills both intentions of a choice default: it provides incentives to switch to a greener electricity mix *and* it matches with individual preferences. The findings in Chapter 2 of this thesis are relevant for the design and application of green electricity defaults in practice and provide one of the first experimental assessments in the literature of whether revealed preferences actually match with choices implied in a default regime in the context of the choice of an electricity contract.

Chapter 3: Costs and Benefits of Green Choice Defaults – A Field Study in the Residential Electricity Market

Chapter 2 offers insights on the general design and applicability of green electricity defaults. Yet, the costs and benefits that come with the use of green electricity defaults at a consumer and at the societal level are not specifically addressed. Brown, Farrell,

and Weisbenner (2016) are among the few authors in the current literature who assess which groups in society are particularly prone to stick to defaults. Their analysis focuses, however, on individual decisions for retirement savings. Brown et al. (2016) find that individuals with low education levels and low decision-specific literacy stay with a default more often than others. Additionally, inadequate information about a choice situation, procrastination or perceiving the default as an implicit recommendation also leads people to choose default pension plans more often. Interestingly, the authors also acknowledge that those who stay with the default plan are the least content with their choice. Thus, it may be hypothesized that individuals who regret their default choice do so because the default reduces their economic pension outcome in comparison to any other plan. Hence, the use of defaults may have considerable impacts on individual welfare. In this vein, taking a similar route of analysis, I aim at assessing the reasons why some people tend to stay with the default more often than to opt out and choose a different contract. Given an intermediately green and intermediately expensive choice default in the field setting, I explore whether the nudge impacts certain groups in society differently. Finally, I assess whether the effects for individual welfare should concern policy makers. In essence, I focus on three guiding research questions:

> **Research Question 2.A:** Why do people stay with green electricity defaults?
>
> **Research Question 2.B:** Does a default distort choices of certain groups in society differently?
>
> **Research Question 2.C:** What are the effects on individual welfare?

In order to analyze these questions, I design a field study with a representative set of households of a Swiss city. This study is administered via an on-line and paper-pencil questionnaire sent out to a randomly selected sample of customers of the local utility. The empirical procedure contrasts the current contract choices under a default regime (using historical data provided by the utility), with a hypothetical active choice of an electricity contract. I use the rich evidence in the applied environmental economics literature on the effectiveness of 'cheap-talk-scripts' (for instance Cummings and Taylor, 1999) to alleviate a potential social-desirability bias on behalf of the respondents. In doing so, I am able to

assess the match and mis-match potential of green electricity defaults with respect to an active choice. Combining this data with the responses of who sticks to a default and why sheds light on the highly policy relevant, but so-far under-researched issue of distributional differences for different groups of households as well as on the costs and benefits of green electricity defaults.

The findings offer multiple novel insights that are important for policy makers in residential electricity markets. First, demographics are only a weak predictor of electricity contract choice. Much more variation in different choices can be explained by personality characteristics, such as the importance of nature conservation or altruistic motivations. Additionally, several reasons for choice, such as information related problems or procrastination, help to explain how people make choices for electricity contracts. Thus, as most of these variables are not directly observable for a choice architect, it may prove challenging to provide 'better' defaults based on readily available demographic data only. Second, the green electricity default used in this field setting has different costs and benefits at a consumer and at a societal level. On the one hand, it hinders pro-environmental, but uninformed households to move to even more environmentally friendly contracts. On the other hand, households with lower socio-economic status in terms of education and income more often forgo to choose a cheaper contract due to the choice default. Both distortions have substantial negative effects on consumer welfare. Third, while the green electricity default successfully curbs greenhouse gas emissions, the emission abatement seems to be rather costly when compared to some recent estimates of the social cost of carbon.

The findings in Chapter 3 carry highly policy relevant messages and help to put the results of Chapter 2 into a wider context. Specifically, they open up the discussion among choice architects and policy makers on how green electricity defaults should be designed in order to moderate potential distributional concerns for 'weaker and poorer' groups in society.

Chapter 4: Nudge for Good? Choice Defaults and Negative Spillover Effects

The forth chapter of this thesis scrutinizes potential behavioral effects, which reach beyond the direct impact of choice defaults. Evidently, defaults are very effective policy

instruments for promoting 'good' causes, such as fostering pro-environmental consumption (Ebeling and Lotz, 2015; Egebark and Ekstroem, 2016; Brown, Johnstone, Hascic, Vong, and Barascud, 2013), increasing retirement savings (Choi et al., 2003; Cronqvist and Thaler, 2004), or raising charitable giving (Altmann et al., 2014). Even though there is an active debate touching upon the ethics of nudging (Bovens, 2009; Hausman and Welch, 2010; Desai, 2011; Sunstein, 2015), their distributional effects (Löfgren, Martinsson, Hennlock, and Sterner, 2012; Brown et al., 2016, or Chapter 3 in this thesis) or whether choice defaults correspond to their libertarian paternalistic intentions (Carroll, Choi, Laibson, Madrian, and Metrick, 2009; Keller, Harlam, and Loewenstein, 2011, or Chapter 2 in this thesis), the application of defaults is prevalent, as their outcomes are remarkably successful when judging their effects on the targeted behavior in isolation.

Chapter 4 aims to widen the scope of the behavioral analysis of choice defaults by also looking at whether their use may inadvertently spill over to subsequent and related behavior. Backed up by empirical data, one may expect two different directions of spillover effects. Cialdini et al. (1995), for instance, describe that individuals behave 'consistently' with their initial decisions, which is classified as a positive spillover of initial on subsequent behavior. Others, like for instance, Meritt, Effron, and Monin (2010) argue that individuals often use initially 'good' behavior to 'license' adjacent immoral behavior, which constitutes a negative spillover. Given these ambiguous findings from the previous literature, Chapter 4 of this thesis follows competing research questions. Evidently, if pro-environmental or pro-social choice defaults lead to positive spillover effects, this provides a strong argument for the effectiveness of such instruments. On the other hand, if choice defaults lead to negative spillovers, such a finding may severely weaken the potential of defaults and it may provoke a critical discussion on the wider suitability of these instruments for sustainable public policy making. The final research question in this thesis thus addresses the following issue:

> **Research Question 3:** Do pro-socially set choice defaults stipulate subsequent beneficial or adverse behavior?

To assess this question, I design an economic laboratory experiment that explores the subsequent behavioral consequences of nudging people into initial pro-social behavior.

Incentives and decision framework of the design are closely aligned with comparable experiments in the literature that aim at assessing positive or negative spillovers in general (see, for instance, Mullen and Monin, 2016). Additionally, the experimental design ensures that I am able to separate income effects, i.e., subsequent behavior that is purely driven by different levels of income after a first decision, and altruistic utility, i.e., subsequent behavior that is purely driven by knowing that something 'good' was done in a first decision, from 'true' spillover effects in subsequent decision-making. I find that, even in very short-lived laboratory sessions, individuals' subsequent decision making seems to be unaffected by an initial choice default. Hence, while a pro-social default fulfills its intention to stimulate more 'good' behavior, it does not seem to interfere with subsequent, untreated individual choices.

Chapter 5: Conclusion

Finally, in Chapter 5 I illustrate the main results of the previous chapters, summarize their contributions and discuss their role for the literature in behavioral environmental economics. I also discuss the direct practical relevance of the presented results and provide some recommendations for policy makers in residential electricity markets. Chapter 5—and this thesis—end with an outline of potential further research topics.

Chapter 2

Defaults in Green Electricity Markets: Preference Match not Guaranteed*

Abstract

Green electricity defaults should steer individual choices to environmentally friendly alternatives and provide a match with individuals' preferences for an electricity mix. It is debatable whether green electricity defaults conform to this latter intention. Using two design elements from existing electricity markets, costly opt-out of contracts and green electricity certificates, preferences for an electricity mix are elicited in an incentivized laboratory experiment. This study then assesses whether preferences in the absence of default options match with preferences in the presence of default options. It turns out that the alignment of default intentions and preferences depends on the relative price for green electricity. Green electricity defaults at low price premia, as currently applied in several electricity markets, do not match subjects' preferences. The findings are relevant for the design of green electricity defaults.

Keywords: defaults, green electricity, choice architecture, nudging
JEL Classification: C91, D03, H41, Q48

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2.1 Introduction

Default options have a strong influence on individual decision making. Changing the default from an opt-in to an opt-out frame has—for example—a positive effect on organ donation rates (Johnson and Goldstein, 2003). Likewise, individual decisions for retirement savings largely differ depending on the default contribution rate (Carroll et al., 2009; Choi et al., 2003; Cronqvist and Thaler, 2004). Experiments manipulating default printer (Egebark and Ekstroem, 2016) or thermostat settings (Brown et al., 2013) provide evidence for the default effect in simple choice situations, in which the costs of opting out of a default are negligible. Defaults also seem to work in more strategic environments, like increasing contributions to public goods (Cappelletti, Mittone, and Ploner, 2014; Carlsson, Johansson-Stenman, and Khanh Nam, 2015) or elevating donations to charities (Altmann et al., 2014).

Due to the success of defaults in these domains, electric utilities and policy makers have vested interests in whether these effects are conferrable to residential electricity markets. The prime reason for this interest is to identify measures that may enhance the share of green electricity,[1] thereby reducing harmful emissions of carbon dioxide. These emissions have been identified to be a prime driver for anthropogenic climate change (Intergovernmental Panel on Climate Change (IPCC), 2014), and nearly 70% of these emissions are attributable to the energy sector (International Energy Agency (IEA), 2016).

In the United States, 29 states have issued 'renewable portfolio standards', which require utilities to reach predefined shares of green electricity (National Conference of State Legislatures (NCSL), 2017). Likewise, in Europe the 'EU renewable energy directive' requires member states to meet a target of 33% of renewable electricity production by 2020 (European Commission, 2016b). In order to finance these changes in the production portfolio, several supply side policy measures, like for instance, feed-in-tariffs or renewable funds exist (for an overview Gan, Eskeland, and Kolshus, 2007). On the demand side, utilities may mobilize consumer interest for green electricity by offering different electricity contracts with differing shares of renewable electricity to buy. In the United States

[1] Throughout this paper green electricity is refereed to electricity predominately produced by renewable sources, such as wind, solar, hydro, or biomass. Conventional electricity is produced by fossil or nuclear sources.

approximately 850 utilities have green pricing programs in which providers sell renewable electricity at different price premia per unit (US Department of Energy, 2016). In the EU member states, the Agency for the Cooperation of Energy Regulators (ACER) (2014) reports 280 electricity suppliers that offer more than 690 different green electricity tariffs. These numbers indicate that voluntary green electricity markets are actively promoted and used.[2] However, despite that overall willingness-to-pay for green electricity is positive (for an overview Soon and Ahmad, 2015), people are still reluctant to switch contracts: for instance, only 10% of consumers in EU electricity markets have switched contracts (Agency for the Cooperation of Energy Regulators (ACER), 2014). This is where the use of a default may come into play. Recent experimental evidence reports that green electricity defaults can considerably increase demand for green electricity (Ebeling and Lotz, 2015). Thus, several utilities have started to explore the effectiveness of defaults by promoting 100% green electricity contracts as the default option.[3]

The intentions of using a green electricity default seem to be twofold. On the one hand, these defaults may channel individual choices to more environmentally friendly alternatives (Sunstein and Reisch, 2014). Hence, green electricity defaults may be an attractive instrument for choice architects who aim at substituting large parts of conventionally generated electricity with more environmentally friendly green electricity. From the viewpoint of internalizing external damages from carbon emissions, the quest for arguments for or against the positive approval of green defaults could stop here, as said defaults address negative externalities from electricity consumption. However, on the other hand, the use of green electricity defaults is argued to be of additional use since they help to overcome systematic flaws in individual decision making and make sure that individuals decide according to their 'true' preferences. If individuals have positive preferences for green electricity, but shy away from the effort to switch contracts, a green default may increase their welfare. This argument relies on the idea of libertarian paternalism, which claims to use defaults to "make choosers better off, *as judged by themselves*" (Thaler and Sunstein, 2008, p. 5, italics in original). Hence, public acceptance, i.e., whether a de-

[2] In voluntary green electricity markets consumers can voluntarily purchase green electricity for their consumption needs.

[3] For instance, the local provider in Zurich has changed the default to 100% green electricity, which may affect contract choices of up to 200,000 households.

fault matches preferences, may also play an important role in designing and developing default regimes. For instance, the 'Executive Order – Using Behavioral Science Insights to Better Serve the American People' specifically states that "the Federal Government should design its policies and programs to reflect our best understanding of how people engage with, participate in, use, and respond to those policies and programs" (The White House, 2015). Despite growing prevalence of green electricity defaults, it is not clear that green defaults match individual preferences for an electricity mix and thus help improve individual decision making. In fact, this paper provides evidence that green electricity defaults do not lead to preference-matching decisions if relative prices for green electricity are low, which may undermine the often assumed implicitness that defaults bring along a win-win situation.

To analyze whether green defaults match private households' electricity mix preferences, a measurement strategy for the elicitation of their baseline preferences is needed. Following the framework of Chetty (2008), I assume that individuals act as fully-optimizing agents when no default is present.[4] However, the general regulations of electricity markets do not allow for contract choices in absence of a default. For instance, if a household does not choose an electricity contract, utilities are required by law to supply a default contract, i.e., it is made sure that households receive basic services.[5] Thus, observed contract choices in electricity markets do not provide a suitable indicator of actual preferences over an electricity mix. Furthermore, considerable shares of customers do not switch their contracts due to a multitude of reasons (European Commission, 2010b). These circumstances may severely impede the elicitation of field data for my purposes. By contrast, a laboratory setting "offers possibilities to control decision environments in ways that are hard to duplicate with the use of naturally occurring settings" (Falk and Heckman, 2009, p. 536), and serves as a useful complement to field and survey data. Therefore, I use a laboratory experiment to elicit subjects' baseline preferences. Throughout this paper the baseline is called the 'active choice', as subjects actively choose their electricity mix in

[4] This assumption is also guided by the framework of Bernheim (2009), in which active and consistent choices are deemed relevant for individual welfare. In other words, if the choice situation is fully salient (no presence of a default), agents choose their optimal allocation of goods.

[5] For instance, the European Union foresees that all households must have a right to be supplied with electricity. The local network provider is responsible to grant this right (European Commission, 2016a).

the absence of a default option. I will show under which conditions preferences in active choice and in default regimes coincide or diverge.[6]

Furthermore, the experiment addresses the important issue of pricing in green electricity markets. Currently used green electricity defaults are often only marginally more expensive than other available contracts.[7] This is due to the fact that in most cases these green electricity defaults are sourced by depreciated hydropower sources, which are largely cost-competitive with other, more conventional sources of power generation (International Energy Agency (IEA), 2015b). Despite overall decreasing generation costs for renewable electricity, (renewable) technologies other than hydropower still have significantly higher price premia. For instance, the National Renewable Energy Laboratory (NREL) (2011, p. 22) reports that price premia for green power products typically range between 1¢ per kilowatt hour (kWh) to 2.5¢ per kWh, but some "are priced as high as 10 to 20¢ per kWh." Although there may be potential for a further widening of capacity in hydropower, the International Energy Agency (IEA) (2015b) points out that financing and developing new capacity involves considerable risk and cost. Thus, retailers in voluntary electricity markets will need to turn to other technologies, such as solar or wind, which have higher costs than their conventional counterparts (at least in the short and medium term).[8] The experiment addresses this potential shift in technology support by investigating whether subjects react differently to relatively higher priced green defaults.

In the experiment subjects assume the role of private households consuming electricity. In five different tasks subjects have to decide which electricity mix to purchase for a given electricity consumption. An electricity mix consists of varying shares of green and conven-

[6] Importantly, this experiment is based on a student sample. This may limit generalizability of results. Nevertheless, I will distinctively discuss which characteristics still render this sample particularly relevant for messages to policy makers in this context.

[7] For instance, Ebeling and Lotz (2015) provide a randomized controlled trial on the effect of a green electricity default when the extra cost of one unit of green electricity is EUR0.003 as opposed to one unit of conventional electricity.

[8] It is assumed that policy makers continue to support binding goals for shares of renewable electricity with respect to total consumption. For instance, the EU currently discusses to increase this share from "21% [...] to at least 45% in 2030" (European Commission, 2014, p. 6). Furthermore, remaining with the country example of the EU, current hydropower generation is reported to be roughly 300 Terrawatt hours (TWh) per annum, which amounts to 10% of the total electricity generation (Eurostat, 2016). Residential electricity demand is estimated at approximately 900 TWh per annum (European Environment Agency, 2013). Hence, even if it is assumed that the complete hydropower generation is taken to satisfy residential demand, the policy target may not be reached. Therefore, countries aiming at higher shares of renewables may turn to other (more costly) production technologies that are capable of fulfilling this demand.

tional electricity. In each task subjects have to select one out of six different contracts with different electricity mixes. The price per unit of green electricity is fixed within each task and varied exogenously across the tasks. One unit of green electricity is always assumed to be more expensive than one unit of conventional electricity. Subjects are endowed with a budget out of which they have to pay for their electricity costs. Endowment spent on green electricity is used by the experimenter to buy so-called certificates of 'Guaranteed Origin' (GOs). These certificates grant the production and insertion of green electricity to the market. Relative prices for green electricity are chosen to conform to current prices of GOs of different technological sources. As argued above, relative prices for green electricity may differ according to the respective source and technology used to produce green electricity.

Subjects are randomly allocated to three treatment conditions: a condition in which an active choice of an electricity mix is required (denoted as 'ACTIVE'), a condition in which the default electricity mix is 100% green electricity (denoted as 'GREEN'), and a condition in which the default electricity mix is 100% conventional electricity (denoted as 'GREY'). Opting out of either default condition is costly in terms of the effort required for switching.

The main results are as follows. In all treatments, subjects show positive preferences for green electricity, and these preferences are declining with increasing relative prices for green electricity. Subjects in GREEN and GREY often stick to electricity contracts that are presented to them as the default. These findings are in line with previously reported evidence from the field.[9]

However, it is ambiguous whether choices under default options correspond to revealed preferences or not. I examine this issue from two angles: (1) the percentage of participants choosing 100% green electricity, and (2) the mean demand for green electricity units. These indicators are assessed at two price levels: (1) low relative prices for green electricity, (2) high relative prices for green electricity.

At *low relative prices* for green electricity, subjects' preferences elicited in GREEN do not match subjects' preferences elicited in the absence of a default option (ACTIVE).

[9] See Soon and Ahmad (2015) for a review on willingness to pay for green electricity; see Ebeling and Lotz (2015) and Pichert and Katsikopoulos (2008) for the effect of default electricity contracts.

There is neither a match in the percentage of choices for 100% green electricity nor in mean demand for green electricity. For GREY however, subjects' preferences match their preferences elicited in the absence of a default option (ACTIVE).

At *high* relative prices for green electricity, the results are more diverse. Subjects' preferences elicited in GREEN match subjects' preferences elicited in the absence of a default option (ACTIVE) when assessing mean demand for green electricity. They do not match, however, in choice percentages for 100% green electricity. Subjects' preferences elicited in GREY match subjects' preferences elicited in the absence of a default option for 100% green electricity choices. They do not match, however, in mean demand for green electricity.

These results encompass three important messages. First, green electricity defaults at low relative prices for green electricity, as currently applied in several electricity markets, do not correspond to the intention of matching subjects' preferences. Hence, on average, they tend to reduce subjects' welfare as compared to a situation of active choice. On the contrary, conventional electricity defaults at low relative prices match subjects' preferences as elicited in the baseline.

Second, the implication of green electricity defaults at high relative prices for green electricity are more complex. From the perspective of a regulator interested in enhancing mean green electricity demand *and* matching subjects' preferences, an opt-out regime (GREEN) seems a favorable design.[10] If a regulator is interested only in raising the adoption rate of 100% green electricity choices, hereby matching subjects' preferences, an opt-in regime (GREY) seems a favorable design.

Third, using defaults is unavoidable in real electricity markets. Factual active choices, in the sense that no default is present in the choice situation, are unfeasible. Therefore, based on the results, if green electricity defaults are used, it may be advocated to set these defaults at higher relative prices for green electricity. This may guarantee a match in preferences, while also contributing to new renewable production capacity.

This paper proceeds as follows. The related literature is briefly reviewed in Section 2.2. The experimental design, treatments, and hypotheses are explained in Section 2.3. Section 2.4 presents the experimental results. Section 2.5 discusses major findings and concludes.

[10] Here the term regulator is used in a general sense. It could stand for a governing body having oversight on electricity contracts and prices, or for an incumbent utility.

2.2 Related literature

Menges, Schroeder, and Traub (2005) are among the few to use a revealed preference approach for assessing preferences for green electricity. They employ an artefactual field experiment with actual monetary incentives and use donations to environmental charities as a proxy for spending money on a green electricity contract. This paper presented here adds to the literature by also designing an experiment using an incentivized method for eliciting preferences for green electricity. However, the used approach differs from the previous methods in several ways. First, my experimental procedure mimics a real choice situation for an electricity mix. In particular, subjects choosing units of green electricity purchase certificates of 'Guaranteed Origin' (GOs). GOs guarantee the production of green electricity in the electricity market. Hence, possible embedding effects of public goods may be avoided within the experiment (Kahneman and Knetsch, 1992).[11] Second, I introduce default options in the choice situation. Opting out of the default is costly. Subjects need to follow time-consuming procedures to be able to actively select another electricity mix. This design is based on the literature that a default effect occurs due to costs of effort (Dhar and Nowlis, 1999). Complexity of tasks and time-consuming activities (Sitzia, Zheng, and Zizzo, 2015), less experience with a task (Löfgren et al., 2012), or the sheer number of options (Iyengar, Jiang, and Huberman, 2003) may increase these effort costs. Third, Pichert and Katsikopoulos (2008) and Ebeling and Lotz (2015) provide field evidence that a green default has a positive effect on the choice of a 100% green electricity contract. Both studies focus on the (binary) choice to either select a 100% green or a 100% conventional electricity contract. Price differences between the green and the conventional electricity contract are low and fixed for each choice situation. In contrast, the analysis in this paper is not constrained to binary choices of electricity contracts. Subjects can choose between different electricity mixes. Furthermore, in my experiment relative prices for green electricity vary according to the relative costs of production sources. This enables me to analyze whether choices under default regimes correspond to revealed preferences and also to study how increases in the relative price of green electricity interact with subjects'

[11] An embedding effect may occur if subjects can not clearly determine the effect of their public good contribution. For instance, choosing the label 'contribution to the environment' may elicit a higher WTP than 'contribution to green electricity', as the latter is embedded in the former concept.

decisions. Such an analysis is connected to other recent advances in the literature that have targeted the question of welfare implications of nudges (see Chetty, Looney, and Kroft (2009) for salience of taxes; see Carroll et al. (2009) and Bernheim, Fradkin, and Popov (2015) for pension plan defaults).

2.3 Experimental design

2.3.1 Environment, treatments and default options

In my experiment subjects assume the role of electricity consumers. They have to choose their preferred electricity mix for a fixed and given amount of kilowatt hours (kWh). Subjects face five choice situations in which they select such a mix. In each choice situation subjects receive a budget with which they need to cover a fixed consumption of 100 electricity units (kWhs).[12] Units of electricity can be either green or conventional. Conventional electricity has always a cheaper per unit price than green electricity. In the experiment, relative prices for green electricity are chosen to correspond to real-world prices of certificates of 'Guaranteed Origin' (GOs) for different production technologies, like, for instance, hydro or solar power. According to the respective source and technology the price per unit varies. The link between relative prices and production technology is not disclosed to participants. They are informed, however, that the money they use to purchase units of green electricity is spent by the experimenter to buy GOs. These certificates guarantee the production of kilowatt hours of green electricity and are subsequently owned by the respective participants.[13] Prices for green electricity are fixed within and varied randomly across the choice situations in order to avoid order and learning effects.[14]

The experimental design is built on a 'Dictator Game' (Kahneman, Knetsch, and Thaler,

[12] Certainly, in terms of external validity of the results, it would be desirable to have subjects endogenously select their own electricity consumption. However, lack of knowledge and data availability may severely impede the use of actual demand figures. Therefore, as in Menges et al. (2005), I opted to choose a proximity measure for individual electricity consumption. Specifically, I chose to use a standard consumption profile (H1-profile) used by utilities to forecast electricity demand. This profile estimates a personal electricity consumption of between 100 to 130 kilowatt hours per person per month.

[13] Certificates are bought via a certified Swiss power retailer and made accessible for participants online. This approach reflects the actual practice of power companies when they offer green electricity contracts. Utilities need to buy GOs to cover consumer demand for green electricity. They are then required by law to report the purchase of GOs on an annual basis.

[14] Prices and budgets are denoted in real money in order to ease the understanding of the decision environment. At the time of the experiment 1 CHF $\approx$ 0.95 USD.

1986). Choosing conventional electricity is identical to keeping the entire budget which leads to a maximization of individual monetary payoffs (selfish dictator). Choosing green electricity is equal to giving (parts of) the budget away to a recipient (benevolent dictator) and accepting lower monetary payoffs.[15] I use three treatment conditions to study the decision making in the presence or absence of default options (see Table 2.1 for an overview of conditions, relative prices and sample sizes).

In the ACTIVE treatment, subjects have to select their preferred mix of green and/or conventional electricity units actively; they do not receive a default electricity product. The provided budgets are sufficient to purchase any combination of green and/or conventional electricity for the given amounts of kWhs. In ACTIVE subjects choose their preferred electricity mix by moving a slider to decide on their desired electricity mix.[16]

In the GREY treatment, subjects receive 100 units of conventional electricity in each choice situation as their default electricity supply. Additionally, for each of the five choice situations, they receive specific budgets, which they can use to replace units of conventional electricity by units of green electricity. The available budgets always allow for a complete transformation of 100 units of conventional electricity into 100 units of green electricity. For the transition from the default (i.e., 100% conventional electricity) to another electricity mix, five additional contracts are offered[17]: 10% green electricity, 40% green electricity, 60% green electricity, 90% green electricity and 100% green electricity.[18]

In the GREEN treatment, subjects receive 100 units of green electricity in each choice situation as their default electricity supply. Since every unit of green electricity is always more expensive than the respective unit of conventional electricity, subjects are able to substitute green by conventional electricity units and - in addition - build up a monetary budget. If participants decide to buy conventional instead of green electricity units, their

[15] This setup is closest to an actual contract selection in an electricity market. Assuming that an initial contract is always in place, subjects decide on whether to keep this contract or replace it with a different one. See Figure 2.A.1 in the appendix for a translated example of a decision screen in GREEN.

[16] The position of the slider was invisible to the subjects, i.e., they had to actively click in order to make a decision. See Figure 2.A.2 in the appendix for a translated example of a decision screen in ACTIVE.

[17] Note that the remaining percentage in each contract is covered by conventional electricity.

[18] The ACTIVE treatment was elicited chronologically before the two default treatments. Subjects showed significant anchoring to the contract values offered in the two default treatments (60% of choices are very close (+- 3%) to these contracts). Therefore, I opted to use these six contract categories, rather than allowing for a continuous choice. Moreover, there is no statistical difference in means of purchase decisions that fall into one of the six contract categories and purchase decisions for the full set of active choices. Hence, comparability of results is ensured.

Table 2.1: Treatment conditions

Treatment (between)	N	Relative prices for green electricity (within)				
ACTIVE	53	p1	p3	p10	p15	p20
GREY	54	p1	p3	p10	p15	p20
GREEN	54	p1	p3	p10	p15	p20

Note.— Subjects are randomly allocated to treatments. In each of the five choice situations, relative prices for green electricity are randomly varied. Prices p1 to p20 denote the extra cost of one unit of green electricity as opposed to one unit of conventional electricity. For instance, at *p10* a unit of green electricity costs an additional amount of *10 Swiss Rappen (100 Rappen = 1 CHF)*. Relative prices correspond to the actual prices of the certificates of 'Guaranteed Origin' (GOs).

monetary payoff will increase as their expenses for electricity decline. For the transition from the default (i.e., 100% green electricity) to another electricity mix, five additional contracts are offered: 0% green electricity, 10% green electricity, 40% green electricity, 60% green electricity and 90% green electricity.

Implementing a default in an experimental setting is a challenge. Bovens (2009, p. 209) points out that nudges "work better in the dark". Moreover, the experimental design needs to account for the fact that participants come to the laboratory to *do* something. As this study is concerned with *how* rather than *why* defaults work, the implementation of default options in the laboratory is based on costs of effort.[19] Subjects who did not want to stay with the default had to follow specific procedures. At first, they had to actively deselect the default electricity contract in order to be able to choose other contracts. Secondly, they had to justify their choice by indicating why they wanted to change their contract.[20] Finally, participants needed to refer to so-called data-sheets, which were distributed at the beginning of the experiment in order to make an informed decision

[19] In the industrial organization literature it has been recognized very early that switching or effort costs play a dominant role for individual decision making in electricity markets (Klemperer, 1987). Some may argue that nudges are always costless alterations of the choice environment. However, in reality, and explicitly in the case of electricity markets, this premise may not hold. Switching away from a default electricity contract is more or less costly. Hence, I argue along the lines of Thaler and Sunstein (2003, p. 177) that as long as "no choice is forbidden" a nudge may be implemented even if there are transaction costs of choosing another alternative.

[20] In particular, they needed to write sentences with a minimum of 25 characters. This is similar to an (informal) letter of cancellation required by some power suppliers. This approach enabled the collection of anecdotal evidence with regards to specific decision behavior.

between different electricity contracts. For each of the five decision situations a separate data-sheet was provided. These sheets listed the current electricity product and relative price, the available options, i.e., the contract options specified in the description of the default treatments, their prices and some personal information, like a generic and randomly generated customer and contract ID. The participants' task was then to search and decide for their preferred option, and to indicate the corresponding product ID, customer ID and contract ID.[21] If those steps were completed successfully, the participants got an overview over their new contract choice, their monetary budgets and their chosen amount of green electricity. Subjects finally needed to confirm their choice, before proceeding to the next decision task.

2.3.2 Discussion of the experimental design

Incentives and institutions Since revealed preferences of electricity contract choices do not provide a suitable indicator for the elicitation of preferences for an electricity mix, a laboratory setting with tighter possibilities for controlling the decision environment offers a reasonable choice for experimentation (Falk and Heckman, 2009). However, naturally the informative value of a laboratory study relies on the transferability of results to the field. In his seminal work, Smith (1982) argues that transferability of results is granted by a precept, which he calls 'parallelism'. This precept states that for transferability of results to be granted "propositions about behavior of individuals and performance of institutions [...] apply also to nonlaboratory microeconomies where similar [...] conditions hold" (p. 936).

Hence, Smith (1982) claims that used incentives and institutions in the laboratory must *qualitatively* lead to the same outcomes and directional effects as in the field. Falsification of incentives and institutions must be done with field data. Therefore, it is worthwhile to briefly reflect on the incentives and market institutions used in this experiment.

Voluntary green electricity markets are at the core of this experiment. In such markets consumers can choose to 'upgrade' their current electricity consumption with a more sustainable consumption. Utilities enable this choice by offering different contracts or con-

[21] In reality, customers also need to go through several steps before they can change an electricity contract. For instance, the Office of Gas and Electricity Markets (OFGEM) (2016) provides an illustrative and comprehensive guide on how to switch suppliers in the UK.

sumption plans with different shares of green electricity. When purchasing green electricity the utility must buy certificates of 'Guaranteed Origin' (GOs) in order to demonstrate that consumer demand has an actual impact on the overall electricity mix. Usually, GOs sell at different relative prices depending on the production technology of green electricity. Some utilities have set a 100% green contract as the default. Hence, if consumers forgo a change, they will receive green electricity. Changing contracts is usually costly in terms of effort.

The presented experiment tries to capture the institutions and incentives of such a voluntary green electricity market as closely as possible. First, I allow subjects to exercise preferences for green electricity at various relative prices (unlike in stated preference studies, these choices are incentivized). Second, purchases of green electricity lead to the issuance of certificates of 'Guaranteed Origin', which directly impact the contribution to the public good of emission avoidance. Third, default options are implemented based on cost of effort, which are analogous to the often complicated switching procedures of electricity contracts. Therefore, I argue that incentives and institutions of the experimental voluntary green electricity market should demonstrate parallelism with the actual market. Potential falsification of the used incentives and market institutions with field data is deferred to Section 2.4.1 and 2.4.2. In these sections I will examine whether the direction of willingness-to-pay estimates and the effect of defaults qualitatively hold relative to field data.

The subject pool A second, and equally important issue in terms of transferability of the results is whether there is *quantitative* support for the data. Generally, laboratory studies work with student samples, which may bound the representativeness of the obtained results. Hence, in order to present meaningful conclusions of this study, "exploiting complementaries" in subject pools from the field and from the laboratory might present a viable strategy to better inform the reader on the applicability of the results (Falk and Heckman, 2009, p. 537).

As a starting point, willingness-to-pay for green electricity in this experiment can be mirrored with estimates of more representative samples. Investigating more into the de-

cisive characteristics of subject pools from the field, the following effects seem to apply: younger and better educated people have a significantly higher preference for choosing green electricity; more income makes it possible to support green electricity to a higher extent; and preferences for the environment also lead to higher appreciation of green power (see for instance Bigerna and Polinori, 2014; Borchers, Duke, and Parsons, 2007). Therefore, given that the used sample mainly consists of young and educated subjects with rather low incomes,[22] it may be expected that these characteristics may have countervailing effects with respect to the overall willingness to pay for green electricity. Section 2.4.1 provides evidence whether the same basic effects of these characteristics in representative samples also apply in this sample.

Sample characteristics may influence the overall effect on WTP and green electricity choice, but could also interact with treatment effects. Unfortunately, the only field experiment exploring the effect of defaults on electricity choice is unable by design to report whether individual characteristics actually mattered on the treatment level (Ebeling and Lotz, 2015). Evidence from other domains shows that socio-demographics may influence the choice of a 401(k) pension plan under a default regime. Lower education and lower decision domain specific knowledge lead to a higher acceptance of default options (Brown et al., 2016). Löfgren et al. (2012) find that experienced and knowledgeable people are not influenced by default options. To further investigate this issue, I assess in Section 2.4.2 a subsample of subjects who are more knowledgeable about green power markets and study whether this characteristic has an influence on the strength of a default effect.

Moreover, it could be argued that individuals in real electricity markets are much more used to such contract decisions, and that the obtained results may not replicate in the field. To further question this claim, it is important to examine which characteristics determine a 'typical decider' in voluntary green electricity markets, and whether these characteristics can also be identified in the used sample. Choice of suppliers and products in residential electricity markets is a rather new feature. For instance, in the European Union first liberalization directives were adopted in 1998, with the latest changes to make more retail choices feasible being effectively implemented in the year 2009.[23] Similar

[22] See Table 2.A.1 in the appendix for further characteristics.
[23] See EU Directive 2009/72/EC.

market liberalization efforts can be observed for the US electricity market (Morey and Kirsch, 2016).[24]

Thus, switching rates of contracts or electricity plans provide a suitable proxy of how actively individuals engage with the decision of choosing an electricity contract. However, even though most markets offer significant opportunities to switch and provide structured access to information, only 6.2% of customers have switched contracts in the EU between 2008 and 2010 (European Commission, 2010b, p. 77). Therefore, it may be fair to argue that most households or individuals are inexperienced when it comes to choosing an electricity contract. This ignorance is further facilitated by too infrequent billing, long term contracts issued by the incumbent utility, and the general low interest in the low-involvement good electricity. In fact, the European Commission (2010a, p. 9) comes to the conclusion that "individual consumers are not experts" and that they "are not well aware of many aspects of the market" (p. 13). Survey evidence from the Office of Gas and Electricity Markets (OFGEM) (2014, p. 3) suggests that even if complete liberalization of the electricity markets has been achieved, "most consumers remain disengaged from the energy market."

Laboratory experiments are often criticized for using subjects who are not used to the specific choice environment, or who have only little expertise with the domain as such. However, in fact, based on the arguments brought forward, I may claim that the sample reflects well the inexperience with the decision task in the actual market. Inexperience with the decision or weak knowledge about a market may be decisive determinants for the results in the field as well as in the laboratory. Moreover, there is an additional insight which may render this sample particularly interesting for policy makers: Hortacsu, Madanizadeh, and Puller (2015) show that households in residential electricity markets who have not been exposed to the power to choose, show significant inertia once they are able to choose. This implies that most individuals get involved with choosing an electricity contract only when they move or a when a major change in life-circumstances occurs. Looking at survey and census data, differences in moving numbers are especially connected to age and education. Younger and more educated people move more often (Pew

[24] In Switzerland electricity supplier choice is not liberalized; households can only choose between contracts of the incumbent utility. Thus, choice is even more limited.

Research Center, 2008). Hence, it is sensible to advocate that these types of consumers are also more often exposed to the decision of utility contracts.

Potentially, there is no such concept as the 'typical decider' in an electricity market. However, I argue that market exposure and experience with the decision task may be not as advanced in the general population as it is generally assumed to be. Furthermore, younger and more mobile customers may make more out of the advantages of liberalized electricity markets. Therefore, while I acknowledge that there are certain limits with regards to the generalizability of the subject pool, I also provide well-founded reasons that this sample of the population is particularly relevant to study and provides potentially interesting results for policy makers.

2.3.3 Procedures

A 5x3 mixed factorial experimental design over five different relative prices for green electricity (*randomized and within* subjects) and three treatment conditions (*between* subjects) was used. The experiment was run at the ETH Decision Science Laboratory in Zurich in June and September 2015.[25] A total of 214 participants were recruited using ORSEE software (Greiner, 2015). After pretesting, 161 participants remained for analysis. There were three sessions for ACTIVE and three sessions for the default treatments, in which subjects were randomly assigned to one of the two default treatments. The experiment was fully computerized using the software Unipark™.[26]

When checking in for the experimental sessions, subjects were randomly assigned to computer-equipped cubicles. Subjects received printed and on-screen instructions and an envelope with the data-sheets.[27] Before they started the experiment, the experimenter read aloud common rules for conducting the experiment. Questions were answered individually and in private. After reading the printed instructions, participants were guided through examples of the decision task on the computer screen. In order to check the comprehension of the instructions, subjects needed to answer control questions. Without completing the control questions correctly, they were not able to proceed with the

[25] ACTIVE was elicited in June; Default treatments were run in September.
[26] Program Version: EFS Survey, 10.7. Questback GmbH. Published 2015.
[27] Examples of translated experimental material can be found in the appendix.

experiment. Subjects then faced five individual decisions in random order. Upon completing this task, they were asked to fill in supplemental questionnaires evaluating their principles of decision making, their energy contract literacy, their preferences concerning the environment and its protection, and sociodemographic variables. A session lasted on average 44 minutes. Earnings consisted of three parts: a participation fee, the monetary budget remaining after the decisions and the value of the GO-certificate. Average earnings excluding the value of the GO-certificate were 26.90 Swiss Francs.[28]

2.3.4 Behavioral predictions

Green electricity can be classified as an impure public good (for theoretical frameworks see Andreoni, 1990; Cornes and Sandler, 1994). Impure public goods deliver two 'characteristics' to the consumer. Consumers who purchase green electricity receive on the one hand a private good: electricity for private consumption. On the other hand, purchasing green electricity provides a public good: a reduction in harmful emissions.

Consumers' utility functions depend on the valuation of both, the private and the public good characteristics (Lancaster, 1966). If individuals show positive preferences for the public characteristic, they have a positive willingness-to-pay for green electricity. Such positive preferences may stem from different motivations like altruism, social norms, fairness considerations, internalized norms or intrinsic motivation (for an overview Frey and Stutzer, 2006).

In my experiment, the formation of subjects' preferences for the impure public good differs between treatments. Consider Figure 2.1 for an illustration of the choice situations. Subjects' task in ACTIVE is as follows. Subjects obtain a fixed electricity consumption that is purely private in its characteristic.[29] They are asked on their willingness to replace

[28] One choice situation was chosen randomly to be relevant for payoff. As the certificates could only be ordered ex-post by the experimenter, participants received an official letter stating when and where they could access their GO-certificate. Average student wage is 30 Swiss Francs per hour.

[29] Note that this consumption may produce negative externalities, which I refer to as 'baseline externalities'. However, the crucial point is that the consumption does not lead to *additional* negative external effects. This is reasonable to assume, as a potential increase in demand for conventional electricity most probably will not lead to a change in electricity mix (utilities will not demount existing renewable infrastructure). Therefore, Kotchen (2005) assumes in his model that this baseline consumption has only a private characteristic. This is true, as long as one thinks in the dimension of additional harm done. For voluntary green electricity markets to function, however, baseline externalities must exist. Otherwise, there is no need for choosing green electricity.

purely private units of this consumption with units of green electricity containing also a public good contribution (denoted as G^*).[30] Purchasing units of green electricity is costly and reduces subjects' monetary payoffs (M^*). Subjects' budgets (denoted as M) are sufficient to completely replace the purely private consumption with a mixed private and public good consumption. At the extreme ends, subjects are either completely selfish (replacing zero private consumption units with green electricity, $G^* = 0$) or completely benevolent (replacing the complete private consumption with green electricity, $G^* = \bar{K}$).[31] Previous empirical and theoretical research points to positive, but non-uniform preferences for impure public goods, i.e., for green electricity. With an increase in the relative price per unit of green electricity, the demand for the impure public good is expected to decrease (Eriksson, 2004; Kotchen, 2005; Menges et al., 2005). For the sake of consistency with the preceding literature, I presume to observe similar patterns in my experiment.

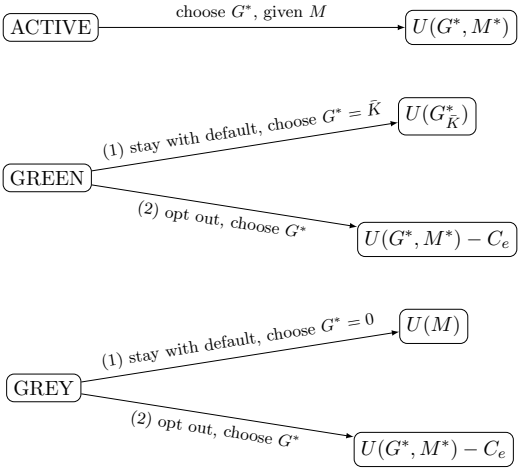

Figure 2.1: Choice situations

[30] Characters with a * denote choice variables.
[31] $\bar{K}$ denotes 100% consumption of green electricity.

Hypothesis 1 Preferences for green electricity

Subjects in ACTIVE show positive preferences for green electricity leading to a positive demand for green electricity. This demand decreases as the relative price of green electricity increases.

A main interest of this paper is to investigate whether subjects maximize their utility differently when confronted with the choice of purchasing units of the impure public good under a default regime or in a situation of active choice. According to libertarian paternalistic intentions there should be no differences in preferences under both regimes.

In GREEN subjects have two choices: (1) stay with the default and receive utility from $G^* = \bar{K}$; (2) opt out of the default and receive utility from their preferred G^* and M^*. Additionally, subjects incur costs of effort, C_e, to opt out of the default.[32] In GREY, subjects also have two choices: (1) stay with the default and receive utility from M; (2) opt out of the default and receive utility from their preferred G^* and M^*. Additionally, subjects incur costs of effort, C_e, to opt out of the default. First, I consider the differences in percentages of 100% green electricity choices.

Hypothesis 2A Choice Match ACTIVE | GREEN

The percentage of subjects in GREEN who do not opt out of 100% green electricity is not higher than the percentage of participants choosing 100% green electricity in ACTIVE. There are equal proportions of 100% green electricity choices in ACTIVE and GREEN.

Hypothesis 2B Choice Match ACTIVE | GREY

The percentage of participants in GREY who choose 100% green electricity is not lower than the percentage of participants choosing 100% green electricity in ACTIVE. There are equal proportions of 100% green electricity choices in ACTIVE and GREY.[33]

[32] Subjects will prefer choice (2) if and only if the net utility of opting out is larger than its cost of effort to do so. Cost of effort to opt-out of a default is identical in GREEN and GREY. Note that the default effect may not be attributed singularly to effort costs and that other effects might play a role, for instance implicit recommendation (McKenzie et al., 2006) or psychological biases (Ritov and Baron, 1992; Samuelson and Zeckhauser, 1988; Thaler, 1980).

[33] Note that the dependent variable of interest is choice of 100% green electricity. However, the reported results in Section 2.4 (Table 2.2) also show data when the dependent variable is choice of 100% conventional electricity.

Second, the effect of a default on mean demand for green electricity is examined. Prior research suggests that even if subjects do not choose the default option, i.e., 100% green or conventional electricity, they may use defaults as a reference point to make their choices (for instance Dhingra, Gorn, Kener, and Dana, 2012). As argued, a well-set default should not result in choices deviating from subjects' preferences, i.e., mean demand should not differ between ACTIVE and the default treatments. The following two hypotheses apply.

Hypothesis 3A Mean Demand Match ACTIVE | GREEN

Mean demand for green electricity is not positively influenced by the presence of a green electricity default option. Subjects' mean demand for green electricity in GREEN is not higher than in ACTIVE. There is an equal mean demand for green electricity in ACTIVE and GREEN.

Hypothesis 3B Mean Demand Match ACTIVE | GREY

Mean demand for green electricity is not negatively influenced by the presence of a conventional electricity default option. Subjects' mean demand for green electricity in GREY is not lower than in ACTIVE. There is an equal mean demand for green electricity in ACTIVE and GREY.

2.4 Results

The 161 participants in the experiment were university students, and to a large extent undergraduates (71%). 58% of the participants were women and the median age was 22 years.[34]

2.4.1 Green electricity demand

Result 1 Preferences for green electricity

Hypothesis 1 *not rejected by the data.*

Subjects in ACTIVE show positive preferences for green electricity. Demand for units of green electricity decreases as the relative price for green electricity increases. In other

[34] For additional characteristics of the sample see Table 2.A.1 in the appendix. As argued in Section 2.3.2, the influence of the characteristics will be discussed in the presentation of the results.

words, subjects act more selfishly when the relative prices for the impure public good, i.e for green electricity, increase.

As argued above, subjects in this experiment may show benevolent behavior triggered by motivations like altruism or environmental concern. This experiment is no exception to this observation. Table 2.2 provides an overview of the experimental data. Several observations apply:

(1) The top row in panel ACTIVE in Table 2.2 indicates that subjects have a positive demand of units of green electricity, G^*. Such behavior may be rationalized by evidence of the questionnaire, which finds that many subjects have pro-environmental preferences and positive altruistic motives.[35]

(2) The demand for an additional unit of green electricity declines as the relative price for green electricity increases.[36] Subjects keep more money privately as the price per unit increases (pairwise comparisons across price premia for green electricity, t-tests, Mann-Whitney-Wilcoxon tests (MWW), all $p < .001$).

(3) Different types of demand behavior can be observed when the relative price for green electricity increases. More than half of the subjects in ACTIVE behave completely benevolent in terms of choosing $G^* = \bar{K}$ at p1 for green electricity. Complete benevolence decreases with increasing relative prices for green electricity. Hence, the degree of acting environmentally friendly depends on its cost. Additionally, a quite stable share of strictly selfish subjects always selects $G^* = 0$. Based on evidence from the supplemental questionnaire, these subjects do not have strong environmental and altruistic preferences, which may be an indication why they act in a self-interested way.

(4) The comparison of willingness-to-pay estimates from the field with the values elicited in this experiment shows that the results are similar, both qualitatively and quantitatively. Soon and Ahmad (2015, p. 882) report a mean monthly willingness-to-pay for green electricity of USD7.16 (CI 95% 6.66-7.66) based on a meta-regression of 137 estimates in stated preference studies. Equivalently, subjects in ACTIVE are willing to pay

[35] These indicators are based on the level of stated agreement to 5-point Likert-scale questions.

[36] Note that for each relative price for green electricity subjects receive a corresponding budget. These budgets vary across prices. For higher relative prices for green electricity, the budgets are also higher. This may explain why mean green electricity demand does not reduce by much between p10, p15, p20.

on average CHF6.38 (USD6.47).[37] Thus, in terms of the precept of parallelism, the used incentives and institutions not only lead to qualitatively comparable results, but also the obtained estimates seem to generalize to more representative subject pools. Additionally, as observed in more representative samples, positive preferences for the environment and future generations also increase demand for green electricity in this experiment.[38]

Table 2.2: Experimental data

ACTIVE (n=53)	Relative prices of green electricity				
	p1	*p3*	*p10*	*p15*	*p20*
Mean green electricity demand [%]	67	57	29	27	25
Mean budget privately kept [CHF]	0.33	1.29	7.07	10.88	14.95
% of subjects choosing 100% green elec.	55	36	6	6	2
% of subjects choosing 100% conventional elec.	11	19	19	25	26

GREY (n=54)	Relative prices of green electricity				
	p1	*p3*	*p10*	*p15*	*p20*
Mean green electricity demand [%]	71	56	23	16	15
Mean budget privately kept [CHF]	0.29	1.31	7.74	12.60	16.96
% of subjects choosing 100% green elec.	65	37	4	4	4
% of subjects choosing 100% conventional elec.	22	26	54	52	50

GREEN (n=54)	Relative prices of green electricity				
	p1	*p3*	*p10*	*p15*	*p20*
Mean green electricity demand [%]	86	73	39	34	31
Mean budget privately kept [CHF]	0.14	0.82	6.11	9.97	13.70
% of subjects choosing 100% green elec.	83	59	24	17	20
% of subjects choosing 100% conventional elec.	11	17	24	28	31

Note.— Panels for each treatment ACTIVE, GREY, GREEN are shown. Mean green electricity demand (in %) is reported for each treatment and for respective relative prices of green electricity. Correspondingly, the percentages of subjects choosing 100% green and 100% conventional electricity is shown. Prices refer to the additional costs of one unit of green electricity as opposed to one unit of conventional electricity. For instance, at *p10*, a unit of green electricity costs an additional *10 Swiss Rappen*.

[37] This calculation assumes a measured WTP of CHF0.029 per unit of green electricity for an average Swiss two-person household with a monthly consumption of 220 kWh.

[38] See Table 2.A.2 in the appendix for further insights on treatment effects, when including predictors from the questionnaire.

2.4.2 The default effect

Behavioral patterns in the two default treatments show diverging results with respect to ACTIVE. The levels of benevolence and selfishness differ in GREY and respectively in GREEN.

Two explanations why this could be the case can be ruled out by the experimental data and evidence from the questionnaire. First, it could be argued that being in one or the other treatment leads to different reactions in terms of green electricity demand at different relative prices for green electricity. However, treatments do not influence the general reaction to relative price changes of green electricity (pairwise comparisons across treatments, t-tests, Mann-Whitney-Wilcoxon tests (MWW), all $p > .050$), and thus price elasticities of green electricity demand are of equal sign across treatments. Second, environmental and altruistic preferences, which may rationalize benevolent or selfish behavior, do not differ across treatments (Env. pref., $Chi^2 = 4.45$, $p = .814$; Alt. pref., $Chi^2 = 8.74$, $p = .365$). Thus, acting benevolent or selfish in one or another treatment is not influenced by an uneven distribution of these preferences across the full sample.

Nevertheless, subjects in GREY choose $G^* = 0$ more often and demand less G^* with increasing relative prices for green electricity relative to subjects in ACTIVE. They behave in more selfish ways. Such behavior may be rationalized by a default effect. If acting more self-interested is the default, subjects do so, irrespective of their price-sensitivity or their environmental preferences. Evidence from the questionnaire provides further insights how subjects may rationalize this selfish behavior. Higher stated agreement with the notion *'I have the impression that nowadays everyone feels obliged to buy green electricity in order to correspond to societal expectations'*, leads to fewer choices of 100% green electricity – acting against a perceived social norm seems to rationalize selfish behavior. In GREEN, more subjects show complete benevolent behavior. Subjects choose $G^* = \bar{K}$ more often and demand more G^* than subjects in ACTIVE. As environmental and altruistic preferences do not differ across treatments, this behavior may again be rationalized by a default effect. If acting more benevolent is the default, subjects do so, irrespective of their price-sensitivity or their environmental preferences.

Both of these observations of the default effect equally apply to data elicited in the field. For instance, Ebeling and Lotz (2015) find that a green default has a tremendous effect on choices for green electricity contracts irrespective of green preferences.[39] The same basic treatment effects apply in this experiment. Defaults have a significant effect on choices. Green or altruistic preferences do not influence these treatment effects.

Digression: Does knowledge about the decision domain attenuate default effects? Interesting effects apply when analyzing a subsample of subjects that are more knowledgeable with respect to green power markets. In line with other evidence from the field (Brown, Farrell, and Weisbenner, 2011; Brown et al., 2016; Löfgren et al., 2012), conventional and green default effects diminish.[40] This suggests that knowledge and expertise in a market seem to be potential drivers for making choices that match baseline preferences. Hence, in terms of the precept of parallelism, it seems that implementing the default in the experiment yields the same qualitative effects as in the field.

Having established that individual behavior in GREY and GREEN is potentially affected by a default, the following sections more closely examine situations when defaults may lead to matches and mismatches with preferences elicited in ACTIVE.

2.4.3 Match in choices

In this section, I analyze the effects of defaults on the proportion of 100% green electricity choices, in order to investigate whether subjects maximize their utility differently under a default regime or in a situation of active choice.

Result 2A Choice Match ACTIVE | GREEN

Hypothesis 2A *is rejected by the experimental data.*

The overall percentage of subjects in GREEN who choose 100% green electricity is significantly higher than the percentage of participants in ACTIVE across all relative prices for green electricity. Obviously, a green electricity default has a decisive impact on the choice of 100% green electricity. Using proportion tests, significant statistical differences are

[39] Ebeling and Lotz (2015) use voting outcome at different postal codes as a proxy for green preferences.
[40] Pairwise proportion and t-tests neither yield significant differences at the level of binary contract choice nor at the level of mean green electricity demand.

found in the selection of 100% green electricity between ACTIVE and GREEN (proportion test, all p-values $p \leq .050$).[41] This behavior can be rationalized by the well-established default effect. Subjects choose 100% green electricity more often when it is the default.

Result 2B Choice Match ACTIVE | GREY

Hypothesis 2B *is not rejected by the experimental data.*

The overall percentage of subjects in GREY who choose 100% green electricity is not significantly lower than the percentage of participants in ACTIVE across all relative prices for green electricity. This means that a conventional electricity default has no decisive impact on the choice of 100% green electricity. Using proportion tests, no significant statistical differences are found in the selection of 100% green electricity between ACTIVE and GREY (proportion test, all p-values $p > .050$).[42]

Figure 2.2 highlights results 2A and 2B. Panel A of the figure shows 100% green electricity choices between ACTIVE and GREEN. Prices for green electricity are depicted on the abscissa. Percentages of 100% green electricity choices are denoted on the ordinate. Correspondingly, Panel B of the figure shows choices of 100% green electricity between ACTIVE and GREY.

A linear probability model is run to control for price and treatment effects of subjects' choices. The dependent variable is a binary indicator of whether subjects choose 100% green electricity or not. The results are reported in Table 2.3. Increasing relative prices for green electricity reduce the probability of choosing 100% green electricity. Treatment contrasts on the right hand side of Table 2.3 show the match in preferences between the default treatments relative to ACTIVE at different relative prices for 100% green electricity choices. These results correspond with the behavioral patterns outlined above. Subjects in GREEN are more inclined to choose 100% green electricity relative to subjects in ACTIVE. The probability of choosing 100% green electricity is not significantly different between GREY and ACTIVE.[43]

[41] At p15 the differences are marginally significant at a 10%-level.

[42] Note that if the dependent variable is 100% conventional electricity choices, the default in GREY decisively impacts behavior. Subjects in GREY choose more often 100% conventional electricity than in ACTIVE at high relative prices for green electricity.

[43] The order in which relative prices for green electricity were shown to subjects did not affect these likelihoods. Further robustness checks with predictors from the post-experimental questionnaires can be found in the appendix in Table 2.A.2.

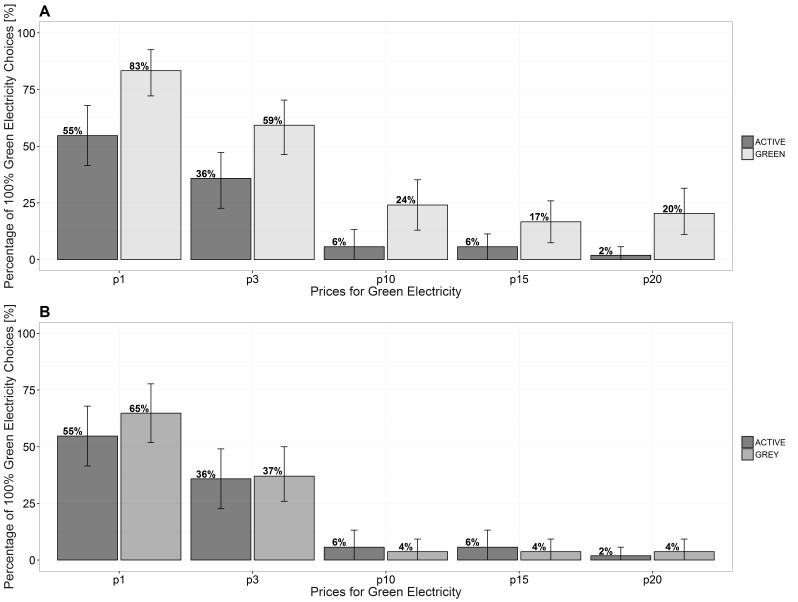

Figure 2.2: 100% green electricity choices

Note.— Panel A shows matches for ACTIVE and GREEN with respect to choosing 100% green electricity. Panel B show matches for ACTIVE and GREY with respect to choosing 100% green electricity. Percentages of 100% green electricity choices are denoted on the ordinate. Prices for green electricity are depicted on the abscissa. Error-bars denote bootstrapped 95% confidence intervals.

Table 2.3: Linear probability model (including contrasts) | Choice of 100% green electricity

LPM		Treatment Contrasts (base: ACTIVE)				
Choice 100% green electricity		p1	p3	p10	p15	p20
Intercept	0.547***					
	(0.069)					
GREEN	0.286***	0.286***	0.234**	0.184***	0.110*	0.185***
	(0.086)	(0.086)	(0.095)	(0.067)	(0.060)	(0.058)
GREY	0.100	0.100	0.012	−0.020	−0.020	0.018
	(0.095)	(0.095)	(0.094)	(0.041)	(0.041)	(0.032)
p3	−0.189***					
	(0.054)					
p10	−0.491***					
	(0.070)					
p15	−0.491***					
	(0.070)					
p20	−0.528***					
	(0.069)					
Interactions	Yes					
Observations	805					
Subjects	161					
Adjusted R^2	0.316					

Note.— *p<0.1; **p<0.05; ***p<0.01
Linear probability model; the dependent variable of the linear probability model (LPM) is a binary choice of selecting 100% green electricity or not. The interactions between price premia and treatment are all insignificant at standard significance levels. The intercept denotes choices at p1 in ACTIVE. Treatment contrasts (Wald-tests) denote differences between ACTIVE and GREEN/GREY at different price premia. Additionally, the contrast differences between GREEN and GREY are all significant at conventional levels. Subjects in GREEN choose 100% green electricity more often at each relative price for green electricity with respect to subjects in GREY. Robust standard errors are in parentheses. Standard errors clustered at subject level.

2.4.4 Match in mean demand

In this section, I analyze the effect of defaults on mean green electricity demand.

Result 3A Mean Demand Match ACTIVE | GREEN

Hypothesis 3A *is partly rejected by the experimental data.*

Subjects in GREEN demand significantly more green electricity than subjects in ACTIVE at low relative prices (p1, p3) for green electricity (t-tests, all $p < .050$; MWW, $p < .030$). At high relative prices (p10, p15, p20) there is no difference in subjects' demand for green electricity between GREEN and ACTIVE (t-tests, all $p > .050$; MWW $p > .050$). This means that there is a match in mean demand for green electricity at higher prices (p10, p15, p20) between ACTIVE and GREEN. However, demand mismatches at low relative

prices for green electricity. Reviewing Table 2.2 and Figure 2.1, this 'over-reaction' in demand may be rationalized with the corresponding utility of the choice situation. For instance, 83% of subjects in GREEN value the utility of staying with the default at p1 higher, $U(G^*_K)$, than choosing G^* actively. Thus, opting out of the default is deemed too costly in turn for the gain in additional utility of selecting $U(G^*)$. With rising gains in additional utility, i.e., rising M^*, more subjects opt out from a 100% green electricity choice and choose levels of G^* that correspond to levels of G^* chosen in ACTIVE.

To complement Result 3A, the effect of the default on the matching of preferences can be separated into extensive and intensive margin effects. Three effects are of particular interest. First, defaults may affect choices at the intensive margin, i.e., subjects choose other contracts less often at the 'cost' of choosing the default. Second, defaults may affect choices at the extensive margin, i.e., subjects 'drop out' completely and choose the exact opposite of the default. Third, it can be analyzed whether for a subsample of subjects who depart from the default, the effects of the intensive and extensive margin analysis still prevail or if these subjects recover the baseline elicited in ACTIVE.

As expected, the results of the analysis depend on the price premia for green electricity. At price premia of p20, p15, p10 per unit of green electricity, choosing a default operates completely at the intensive margin. Subjects choose the default at the cost of fewer other contracts relative to ACTIVE (pairwise proportion tests, all $p < .050$).[44] There is no effect on the extensive margin, i.e., subjects do not choose more 100% conventional electricity contracts relative to ACTIVE (pairwise proportion tests, all $p > .050$). Analyzing a subset of subjects who depart from the default, I find that the baseline in ACTIVE is always recovered. There are no differences in distributions (pairwise proportion tests, all $p > .050$). This leads to the conclusion that the match in preferences between ACTIVE and GREEN at high relative prices is attained, because the impact of the default choices is countervailed by the effects on the intensive margin. At price premia p3, and p1 per unit of green electricity, choosing a default operates again at the intensive margin. Subjects choose fewer other contracts relative to ACTIVE (pairwise proportion tests, all $p < .050$). Moreover, there is no effect at the extensive margin (pairwise proportion tests, all $p >$

[44] At p15 the default effect itself is only significant at the 10% level, hence the effect on the intensive margin is also attenuated, $p = .180$.

.050). Analyzing again a subset of subjects who depart from the default, I find that at p3 and p1 the baseline is recovered (proportion tests, $p > .050$). This leads to the conclusion that the default choices are decisive for the mismatch in preference. While the effect on the intensive margin is significant, it is not strong enough to offset the effect of the default on 100% green electricity choices.

Result 3B Mean Demand Match ACTIVE | GREY

Hypothesis 3B *is partly rejected by the experimental data.*

Subjects in GREY do not demand significantly less green electricity than subjects in ACTIVE at low relative prices (p1, p3) for green electricity (t-tests, all $p > .050$; MWW, $p > 0.050$). At high relative prices (p10, p15, p20) there is a significant difference in subjects' demand for green electricity between GREY and ACTIVE (t-tests, all $p < .050$; MWW $p < .050$).[45] This means that there is a match in mean demand for green electricity at lower relative prices (p1, p3) between ACTIVE and GREY and a mismatch at higher relative prices. Reviewing Table 2.2 and Figure 2.1 again, this 'under-reaction' in demand may be rationalized with the corresponding utility of the choice situation. For instance, 54% of subjects in GREY value the utility of staying with the default at p10 higher, $U(G^* = 0)$, than choosing G^* actively. Thus, opting out of the default is deemed too costly in turn for the gain in additional utility of selecting $U(G^*)$. At low price premia for green electricity, most subjects seem to opt out of the default as the utility of selecting $U(G^*)$ is higher than $U(M)$. This behavior can be rationalized by positive pro-environmental and altruistic preferences as elicited in the questionnaire. As long as acting benevolent is not too costly, subjects will do so. With decreasing gains in additional utility of selecting G^*, i.e., decreasing M^*, subjects do not opt out and stay with the conventional default.

Again, Result 3B can be complemented by separating the impact of the default into extensive and intensive margin effects. At p20, p15, p10 a conventional default significantly affects choices at the intensive margin. Subjects in GREY stay with the default at the cost of selecting fewer other contracts relative to ACTIVE (pairwise proportion tests, all $p < .050$). There are no effects on the extensive margin, i.e., subjects do not choose

[45] At p10 this effect is only directional with $p = .230$.

more 100% green electricity contracts relative to ACTIVE (pairwise proportion tests, all $p > .050$). Analyzing a subset of subjects who depart from the default, I find that the baseline in ACTIVE is always recovered (pairwise proportion tests, all $p > .050$). This leads to the conclusion that the default choices are the prime driver for the mismatch in preferences of mean demand for green electricity, and that the effects at the intensive margin are not strong enough to offset this behavior. At p3, there is no effect at the intensive and extensive margins, as there is no effect of the default treatment. Likewise, subjects who departed from the default recover the baseline in ACTIVE. At p1, the default choices have an effect at the intensive margin, i.e., subjects in GREY choose fewer other contracts relative to ACTIVE (proportion test, $p < .050$). However, assessing a subsample of those who departed from the default, I find that there is an effect at the extensive margin. Subjects who depart from the conventional default choose 100% green electricity more often than comparable subjects in ACTIVE (proportion test, $p < 0.050$). Thus, although the effect of the default is strong, the countervailing effect on the extensive margin for those who opt out leads to an overall match of preferences.

Figure 2.3 shows the results 3A and 3B graphically. Mean demand for green electricity is shown on the abscissa, relative prices for green electricity are shown on the ordinate. Panel A and B show the match – mis-match situation in mean green electricity demand between ACTIVE and GREEN or GREY respectively.

An OLS model is run to control for price and treatment effects of subjects' mean demand for green electricity.[46] The dependent variable is mean green electricity demand. Results of the model are reported in Table 2.4. Mean green electricity demand declines as price premia for green electricity increase. Treatment contrasts on the right hand side of Table 2.4 show the match in preferences between the default treatments relative to ACTIVE at different relative prices for green electricity. These results correspond with the behavioral patterns outlined above. At low relative prices for green electricity, p1 and p3, demand in GREEN mismatches relative to ACTIVE, while demand in GREY matches relative

[46] I also ran a two-limit Tobit specification that captures the corner solution characteristics of the data, i.e., the pile-ups at 0% green electricity and 100% green electricity. The adjusted estimates of the Tobit specification are comparable in sign, significance and magnitude to the model reported in Table 4. Additionally, I also ran an ordered Probit model, which also estimates results comparable in sign, significance and magnitude to the model reported in Table 4. Therefore, I chose to report the OLS only in order to grant parallelism with the preceding analysis.

to ACTIVE. At p10, the results from the linear model are ambiguous with respect to the match in preferences. In GREEN, mean demand approaches the baseline from above, while in GREY it is approached from below. At p15 and p20, demand in GREEN matches relative to ACTIVE, while demand in GREY is significantly lower relative to ACTIVE.

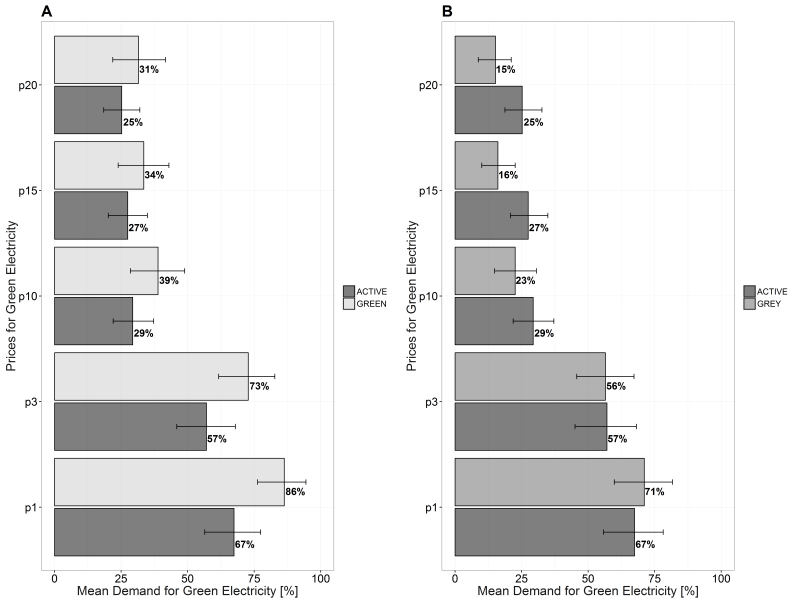

Figure 2.3: Match – mis-matches in mean green electricity demand

Note.— For both panels mean demand for green electricity is depicted on the abscissa. Relative prices for green electricity are depicted on the ordinate. Panel A shows the mean demand for green electricity for ACTIVE and GREEN. Panel B shows the mean demand for green electricity for ACTIVE and GREY. Error-bars denote bootstrapped 95% confidence intervals.

Table 2.4: Linear model (OLS) (including contrasts) | Demand for green electricity

OLS		Treatment Contrasts (base: ACTIVE)				
Mean green electricity demand		p1	p3	p10	p15	p20
Intercept	67.434***					
	(5.637)					
GREEN	18.862***	18.862***	15.702**	9.549	6.047	6.255
	(7.189)	(7.189)	(7.853)	(6.616)	(6.291)	(6.322)
GREY	3.677	3.677	0.594	−6.747	−11.361**	−10.041**
	(8.117)	(8.117)	(8.115)	(5.613)	(5.010)	(4.833)
p3	−10.359**					
	(4.055)					
p10	−38.094***					
	(5.345)					
p15	−39.962***					
	(5.536)					
p20	−42.208***					
	(5.782)					
Interactions	Yes					
Observations	805					
Subjects	161					
Adjusted R^2	0.280					

Note.— *p<0.1; **p<0.05; ***p<0.01
Linear model (OLS); the dependent variable of the linear model (OLS) is a continuous indicator of mean green electricity demand. The interactions between price premia and treatment are all insignificant at standard significance levels, and are thus not indicated specifically. The intercept denotes mean demand at p1 in ACTIVE. Treatment contrasts (Wald-tests) denote differences between ACTIVE and GREEN/GREY at different price premia. Additionally, the contrast differences between GREEN and GREY are all significant at conventional levels. Subjects in GREEN demand more green electricity at each relative price compared to subjects in GREY. Robust standard errors are in parentheses. Standard errors clustered at subject level.

2.5 Discussion and conclusions

Employing design elements from existing electricity markets, this study uses a controlled laboratory experiment to elicit preferences for an electricity mix in the absence and the presence of default options. It has been argued that the use of defaults in green electricity markets follows two intentions. On the one hand, green electricity defaults may steer choices to more environmentally friendly alternatives (Sunstein and Reisch, 2014). Hence, green electricity defaults may be appealing to choice architects in need of providing the respective public good. On the other hand, the use of green electricity defaults has been proposed to facilitate choices matching individuals' preferences (for instance Kaenzig, Heinzle, and Wuestenhagen, 2013). Thus, green electricity defaults may exploit behavioral biases to the advantage of individuals. The core issue of this paper is to investigate this latter intention.

The experimental results are summarized and illustrated in Table 2.5. My experimental design allows to test for a match of baseline preferences with preferences revealed in default conditions. I analyze two types of impacts of defaults: (1) the percentage of 100% green electricity choices and (2) the mean demand for green electricity (rows in Table 2.5). Prices for green electricity are clustered around (1) low relative prices (p1, p3) and (2) high relative prices (p10, p15, p20) (columns in Table 2.5).

Table 2.5: Summary of results

	Preference Match with ACTIVE			
	Low relative prices		High relative prices	
	GREY	GREEN	GREY	GREEN
Choice 100% green electricity	Match	Mismatch	Match	Mismatch
Mean green electricity demand	Match	Mismatch	Mismatch	Match

Note.— The table shows the match of choices in default treatments with ACTIVE across low (p1, p3) and high (p10, p15, p20) relative prices for green electricity. The first row corresponds to matches in 100% green electricity choices (see Results 2A and 2B). The second row corresponds to matches in mean green electricity demand (see Results 3A and 3B).

2.5.1 Defaults at low relative prices for green electricity

Preferences for an electricity mix elicited in GREEN do not correspond to preferences elicited in ACTIVE at low relative prices for green electricity. The reverse is confirmed for preferences elicited in GREY. These results hold when assessing matches of 100% green electricity choices and mean green electricity demand.

Given that many utilities currently apply low priced 100% green electricity defaults, this first set of results has important implications. Subjects seem to 'overreact' to a green default. As opposed to an active choice, they demand more green electricity and choose more often 100% green electricity. Given that subjects in ACTIVE reveal their actual preferred choices of green electricity, G^*, green defaults do not match subjects preferences. On the other hand, subjects in GREY choose levels of green electricity that correspond to those in ACTIVE. Apparently, regulators in the electricity market face a trade-off between enhancing contributions to the public good and matching people's preferences.

The intentions of libertarian paternalism are not met by a green electricity default at low relative prices.

2.5.2 Defaults at high relative prices for green electricity

Defaults at higher price premia for green electricity are more complex in their effects. If the intention of setting a default is to match preferences for a choice of 100% green electricity, then subjects in GREY have a better match with preferences revealed in ACTIVE than subjects in GREEN. If the intention of setting a default is to match preferences for mean green electricity demand (most likely the intention of higher practical relevance), then subjects in GREEN have a better match with preferences revealed in ACTIVE than subjects in GREY.

Given that relative prices for green electricity differ by technology,[47] this second set of results has interesting implications. For solar and wind the relative price per unit of electricity is still considerably higher as compared to conventional technologies. In order to stimulate demand for these higher priced alternatives, utilities may offer higher relatively priced green electricity contracts as a default. Such defaults may correspond to the intentions of libertarian paternalism as long as subjects have a possibility to choose the percentage of green electricity they want to have. Consider—for example—two possibilities of how a high priced 100% green electricity contract can be offered as default: (1) choice between 100% green electricity or 100% conventional electricity,[48] (2) choice between 100% green electricity or lower amounts of green electricity. Based on my results, a green default at high relative prices does not match preferences of subjects in GREEN for the first possibility. Subjects overreact and choose more 100% green electricity than in ACTIVE. On the other hand, if subjects opt out of the default, but can still choose positive values of G^*, they match the mean demand in ACTIVE. Therefore, regulators should be cautious about offering only a 100% green electricity default at high relative prices, when the single alternative is a 100% conventional electricity contract. 'All-or-nothing' contract regimes do not seem to be helpful for matching subjects' preferences.

[47] There is a range of studies assessing levelized cost of electricity, i.e., the present value cost of the production of one unit of electricity for a specific source over its lifetime (see International Energy Agency (IEA), 2015a).

[48] This choice situation is used for example in the field experiment by Ebeling and Lotz (2015).

2.5.3 A note on welfare analysis

Evaluating the welfare implications of policy measures that are motivated by insights from behavioral economics is an important issue. Beginning in the introduction, this paper argues that green electricity defaults present an attractive tool for policy makers to stimulate demand for green electricity in order to reduce negative externalities from electricity consumption. Likewise, defaults are capable of correcting for potential biases in individual decision making. A default could, for instance, help a person with preferences for green electricity to overcome the effort of the actual choice. To the contrary, a default could provoke an 'overcorrection' of the individual status-quo bias, leading a person to overestimate the true utility of choosing green electricity. The identification of how well a default may correct for individual biases, i.e., whether individual welfare is improved or not, cannot be answered easily. In fact, based on recent attempts to study welfare implications of behaviorally guided policies (Allcott and Taubinsky, 2015; Bernheim et al., 2015), I argue why a welfare analysis is not feasible with the current experimental set-up.

The difference between preferences in a choice situation with a default and a baseline without a default (in which true preferences are assumed to be revealed) is termed "average marginal bias" (Allcott and Taubinsky, 2015, p. 2503). It is the valuation mistake of marginal individual choices due to the change in the choice environment. Given the reasoning of libertarian paternalism, a default should not lead to this bias, i.e., individuals should not make valuation mistakes when a default is present in the choice environment. Being able to estimate the average marginal bias at each point of the demand curve is essential for deriving sufficient statistics to evaluate a behavioral policy (Chetty, 2015).

An approach to identify the magnitude of the average marginal bias in a *between* subject setting, like the presented experiment in this paper, is to use a method called the equivalent price metric (EPM) (Chetty et al., 2009). The EPM provides the average bias of a subject in a default regime as compared to a subject in an active choice as a measure of equivalent willingness-to-pay due to the default. The issue with EPM is that it is bounded to assess the average marginal bias only under the (restrictive) assumption that this bias is homogeneous across individuals. Unfortunately, and as argued above, biases in decision making with respect to the default are likely to be heterogeneous across individuals, in

which case this metric will lead to very wrong conclusions. For instance, it will not capture whether persons with preferences for conventional electricity are more or less elastic to the green default in their demand response as compared to persons with preferences for green electricity. In this vein, Allcott and Taubinsky (2015) present another identification strategy of the average marginal bias, which is able to handle heterogeneity in bias. However, this estimation requires a *within* subject design. While there are good reasons why this experiment was designed as a between-subjects-design,[49] it leaves me unable to estimate the average marginal bias for heterogeneous preferences.

Furthermore, as the electricity consumption path in the experiment was given exogenously, there may be welfare effects that cannot be captured by the presented design. Specifically, if households with green preferences are assumed to be more energy efficient, purchases of units of green electricity are less expensive for them. Hence, the monetary consequences of a default are less pronounced. To the contrary, if households with green preferences use less electricity, their purchase of fewer units of green electricity will also result in smaller overall environmental benefits. It remains an empirical question which of these effects dominates, but potentially they may impede an analysis of individual welfare implications of a green default in this setting.[50]

Nevertheless, this research makes an important first contribution towards the positive question of how individual preferences are affected by electricity defaults, and whether there is a match with an active choice baseline on an overall level. Estimating the effectiveness of defaults for the potential of correcting a behavioral biases in a within-subject design must be deferred to future research. Recent evidence shows that this poses indeed a challenging avenue for future work, as results for or against a recommendation of a behaviorally motivated policy may vary depending on the nature of (psychological) biases (Allcott and Taubinsky, 2015) and the welfare perspective that the policy maker adopts (Bernheim et al., 2015).

[49] For instance, between-designs do not pose risks of influencing subjects' decisions by exploiting frame changes or reference points, which is potentially very relevant when choosing actively versus deciding under the presence of a default (for a discussion Charness, Gneezy, and Kuhn, 2012)

[50] I thank an anonymous reviewer for pointing out this issue.

2.5.4 Conclusion

In conclusion, the obtained findings add a new facet to the discussion of green electricity defaults. Electricity network providers are required by law to provide basic supply of electricity to households. Therefore, there is always a default contract in place. Factual active choices (as in ACTIVE) are not possible. Based on the insights of my experiment, currently applied green electricity defaults at low price premia do not match subjects' preferences. If the utility and the regulator are interested in promoting green electricity *and* matching consumers' preferences, it seems, however, to be recommendable to set green electricity defaults at higher relative prices for green electricity. This suggestion is in line with the results by Carroll et al. (2009) who advocate to force active decisions by setting defaults at very noticeable levels. Nevertheless, such interventions may require a rich variety of electricity contracts, mature markets and informed customers. Conveying adequate market information to the customer seems to be of vital importance for enabling informed decision making when defaults are present in the choice environment. It seems that more knowledgeable individuals are not affected by default effects and that they are able to choose in line with their actual preferences. However, results from my post-experimental questionnaires indicate that energy-literacy seems to be rather low.[51] Moreover, consumer research in electricity markets identifies complicated contract selection procedures still as a main barrier of switching (see European Commission, 2010b). This points to the need of a more universal understanding of consumers concerning the functioning of (green) electricity markets.

While I argue that treatment effects and implications of characteristics in this sample parallel choices in more representative environments, and while this sample matches with policy relevant and interesting characteristics of 'typical decision-makers' in voluntary electricity markets, like, for instance in terms of inexperience with the decision task, further field research is needed to analyze the functioning of green electricity defaults. The fact that revealed preferences in electricity markets do not provide a suitable indicator of actual preferences may open up interesting possibilities for further lab-in-the-field experiments.

[51] For instance, half of the subjects are either unsure whether or falsely indicate that purchased green electricity physically flows directly to their sockets.

Specifically, in terms of potential assessments of welfare implications there is a growing need to look at decisions at a more personalized rather than an average level to judge how choices can indeed become 'better'.

A future line of research may also explore to which extent green defaults are beneficial for the additional provision of the public good. The experiment presented here allows for some speculation on how green defaults affect the demand for the certificates of 'Guaranteed Origin' (GOs).[52] Obviously, there is more demand in terms of units of green electricity for low priced GOs than for high priced GOs. This demand, however, may largely be satisfied with certificates from pre-existing renewable energy plants.[53] Thus, the demand of these certificates may not necessarily provide direct investment signals for new renewable capacity. On the other hand, in terms of overall money spent, subjects in GREEN purchase GOs worth seven times the value when prices are high as compared to when relative prices for green electricity are low. This may suggest that, while green electricity defaults at high price premia lead to less overall demand for green electricity, they may be still more useful in terms of investment signals for new renewable production capacity.

This paper is a first contribution pointing to possible caveats in the current use of green electricity defaults. Overall, the results indicate that green electricity defaults—especially at low relative prices—may generate a considerable social cost by not matching people's preferences. Nevertheless, they may contribute to the provision of a public good like stopping or reducing global climate change which constitutes a welfare increase. Given these two welfare effects of defaults, it seems worthwhile to investigate more into a design that could indeed guarantee a double dividend: preference matches for individual decision makers *as well as* contributions to a public good. The often assumed implicitness that defaults bring along this win-win situation has to be put in question.

[52] Note: In order to not confound the decision task, subjects do not know the respective technologies behind the different relative prices for green electricity. Thus, it cannot be determined whether some green electricity production technologies might be more favored than others. Individuals may have pro-environmental preferences, as long as policies do not interfere with their own personal space. This phenomenon, called 'not in my backyard', might have relevant (negative) interaction effects with higher priced green electricity demand.

[53] GOs at low relative prices (p1,p3) are all sourced from depreciated hydropower plants. GOs at high relative prices (p10,p15,p20) are all sourced from *new* renewable production (for instance solar, wind, or biomass).

Appendix

2.A Appendix

2.A.1 Additional data

Table 2.A.1: Summary statistics of selected sample variables

Variable	Description	Mean	SD	Min	Max
Price	Used relative prices for green electricity in each of the three treatments; ranging from 1 to 20 Swiss Rappen	9.80	7.14	1	20
ACTIVE	Indicator of active choice treatment; 1=ACTIVE, 0=GREY or GREEN	0.33		0	1
GREY	Indicator of GREY; 1=GREY, 0=ACTIVE or GREEN	0.34		0	1
GREEN	Indicator of GREEN; 1=GREEN, 0=ACTIVE or GREY	0.34		0	1
Gender	Indicator of female	0.58		0	1
Age	Indicator of age in years	22.69	4.41	18	53
Income	Binary indicator of net income below 3,000 Swiss Francs, 1=Yes, 0=No	0.92		0	1
Education	Binary indicator of completed A-levels, 1=Yes, 0=No	0.98		0	1
Environmental motives	Likert-scale question whether green elec. benefits environment	4.48	0.84	1	5
Altruistic motives	Likert-scale question on concern about future generations	3.31	1.02	1	5
Perception of social norms	Likert-scale question on feeling olbiged to buy green elec.	2.47	1.10	1	5
State regulation	Likert-scale question on whether the state should intervene in elec. markets	3.28	1.08	1	5
Knowledge	Binary indicator of correct green electricity system knowledge	0.47		0	1

Note.— Likert-scale questions range from 'do not agree at all' (1), to 'totally agree' (5). Specific questions concerning green electricity are bound to the Swiss electricity market.

Table 2.A.2: Linear probability model; Linear model (OLS) | Extended control

LPM		OLS	
Choice 100% green elec.	Estimate	Mean green elec. demand	Estimate
Intercept	0.566***	Intercept	59.689***
	(0.098)		(9.370)
GREEN	0.291***	GREEN	21.262**
	(0.083)		(6.811)
GREY	0.134	GREY	7.702
	(0.090)		(7.312)
p3	−0.188***	p3	−10.306**
	(0.056)		(4.150)
p10	−0.486***	p10	−37.819***
	(0.069)		(5.423)
p15	−0.485***	p15	−39.621***
	(0.070)		(5.618)
p20	−0.522***	p20	−41.827***
	(0.070)		(5.878)
Female	0.025	Female	6.139
	(0.042)		(4.464)
Age	0.013*	Age	0.780
	(0.006)		(0.620)
Income	0.011	Income	4.702
	(0.070)		(6.367)
Education	−0.053	Education	−0.519
	(0.070)		(6.367)
Env. motives	0.065**	Env. motives	9.138**
	(0.031)		(4.574)
Env. motives x GREEN	−0.070	Env. motives x GREEN	−6.963
	(0.059)		(5.956)
Env. motives x GREY	−0.037	Env. motives x GREY	−2.504
	(0.045)		(6.414)
Altruistic motives	0.066*	Altruistic motives	6.514*
	(0.038)		(3.656)
Altruistic motives x GREEN	0.003	Altruistic motives x GREEN	1.685
	(0.053)		(5.128)
Altruistic motives x GREY	−0.012	Altruistic motives x GREY	1.395
	(0.048)		(4.617)
Social norms	−0.005	Social norms	−2.737
	(0.031)		(3.415)
Social norms x GREEN	−0.030	Social norms x GREEN	1.600
	(0.046)		(4.519)
Social norms x GREY	−0.086**	Social norms x GREY	−7.135*
	(0.039)		(4.204)
State regulation	−0.030*	State regulation	−3.778*
	(0.017)		(1.927)
Knowledge	−0.101***	Knowledge	−8.249**
	(0.038)		(3.901)
Order	0.011	Order	0.695
	(0.007)		(0.617)
Interactions	Treatment Yes	Interactions	Treatment Yes
Observations	805	Observations	805
Subjects	161	Subjects	161
Adjusted R^2	0.394	Adjusted R^2	0.389

Note.— *p<0.1; **p<0.05; ***p<0.01
Linear probability model, linear model (OLS); dependent variables as indicated, the basis for the intercepts is p1 in ACTIVE. Age, environmental motives, altruistic motives, social norms and state regulation are mean centered. The interactions between price premia and treatments are all insignificant at standard significance levels. Sign, significance and magnitude of contrasts reported in the main text equally apply for the models presented here. Robust standard errors are in parentheses. Standard errors are clustered at subject level.

2.A.2 Experimental instructions

Remark: This set of translated instructions was used for subjects in GREEN.

General Explanations for Participants

Welcome to the experimental laboratory. Today you are taking part in a scientific experiment, in which you can earn a certain amount of money. How much money you earn, is dependent on your decisions. Therefore, please read these instructions carefully.

The set of instructions is for your private use only. Please do not communicate with other participants during the experiment. If you have questions, give a hand signal and the experimenter will come to your desk to answer your questions. Non-observance of this rule will lead to the exclusion of the experiment. During the experiment you will receive information on paper as well as on your computer screen. You take your decisions with keyboard and mouse. Your inputs are completely anonymous. The experimenter knows your identity, however we are not able to relate your decisions with your identity.

Please only use the buttons within the experimental window. Do not use the 'Back'-button of the browser. With the button 'Continue' and respectively 'Back' you are able to change between the next and the previous page. If the button 'Continue' is not visible, you will need to scroll at the bottom of the page. Please note that pages might have longer loading times. Do not press buttons repeatedly and do not refresh your browser by yourself.

In the course of the experiment you will need to take five individual decisions. One of these decisions will be randomly selected by the computer for your payment. Therefore, decide in each of the five decisions as if it is relevant for your final payoff. Finally, you need to fill in a brief supplemental questionnaire.

As soon as each participant has finished the experiment, we will proceed with the individual pay-out. You will be called separately by your seat number and will be paid out in private by an assistant. Please bring along the material you have received.

You will receive a standard participation fee and dependent on your decisions an additional amount of money. Expected processing time for the experiment is set to a maximum of 60 minutes.

Experimental Procedures

You will presume the role of an electricity consumer for the entire experiment. As an electricity consumer you need to decide which electricity product you want to buy.

Electric utilities use different sources for the production of electricity. For this experiment, we distinguish between two sorts of electricity products. Conventional electricity is produced from nuclear and fossil resources. Green electricity is produced from renewable sources, like wind, solar or water.

Within this experiment you have an electricity consumption of 100 kilowatt hours (kWh) in each of the five decisions you make. At the beginning of each decision, you will receive a budget, which exactly clears the costs for 100 kWh of the standard product. If you select a product that differs to your standard product, your expenses will be reduced.

Each decision starts with the choice between two options. As an electricity consumer you can either stay with your standard product or you can select between various other product options.

The standard product of your utility is 100% green electricity. If you choose Option 1 (= the standard product), you have no additional effort and pay the depicted price (see Figure 2.A.1). The outcome of your decision will be shown to you directly and you can move on to the next decision.

If you wish to select between different electricity products, you need to choose Option 2. Your utility offers electricity products with lower shares of green electricity at certain prices per kWh. Prices vary between different situations. Buying less green electricity leads to lower costs that are needed to be covered with your budget.

If you choose Option 2 (= selection of green electricity contracts), you need to run through several steps before you can finally choose another contract. As a *first step* you need to write a small statement why you prefer the choice between other electricity products. This is to be seen as a letter of cancellation of the standard product. Please write a complete sentence and click then on 'Continue'.

As a *second step* you need to select your preferred electricity product. The available options are listed on data-sheets, which are enclosed with your set of instructions. On the next page, you will find an example of such a data-sheet (see Figure 2.A.3).

Additionally, on the data-sheet you find a contract number, an account number, your current standard product and its invoice price.[54] In the lower third of the data-sheet, your utility lists other options of possible contracts. These options have different shares of green electricity and different invoice prices.

For each of the five decisions you have received a separate data-sheet, which differs in the price per unit of green electricity. Therefore, for each decision you need to search for the right data-sheet, before you can proceed with the selection of contracts.

As a *final step* you need to enter product, contract, and account number to choose your electricity product. If you click on 'Continue', we will summarize your decision, stating demanded green electricity and invoice price (see Figure 2.A.4). You have the option to go back and change your selection of contracts. If you click on 'Continue', your decision will be implemented.

[54] Remark: These details are not displayed in Figure 2.A.3.

Figure 2.A.1 was only shown in the instructions for GREEN; Figure 2.A.2 was only shown in the instructions for ACTIVE.

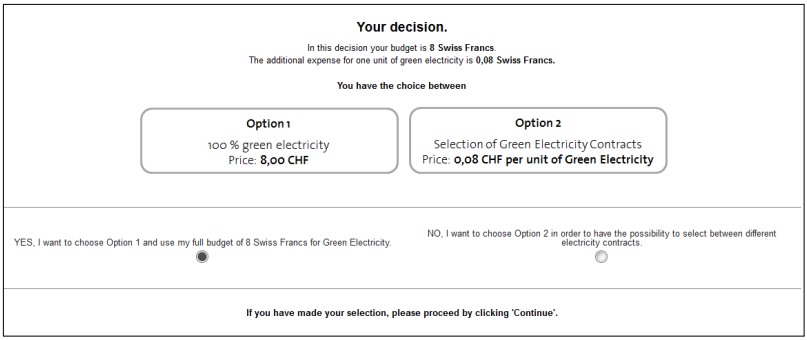

Figure 2.A.1: Sample screen of a decision task in GREEN.

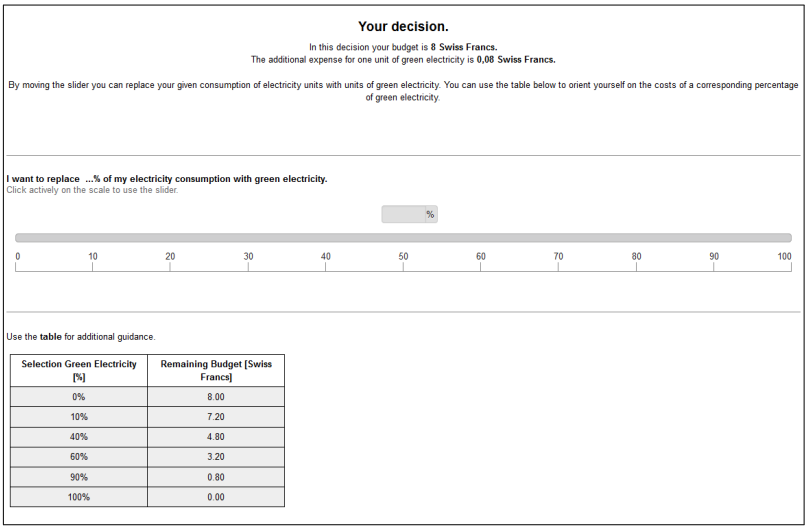

Figure 2.A.2: Sample screen of a decision task in ACTIVE.

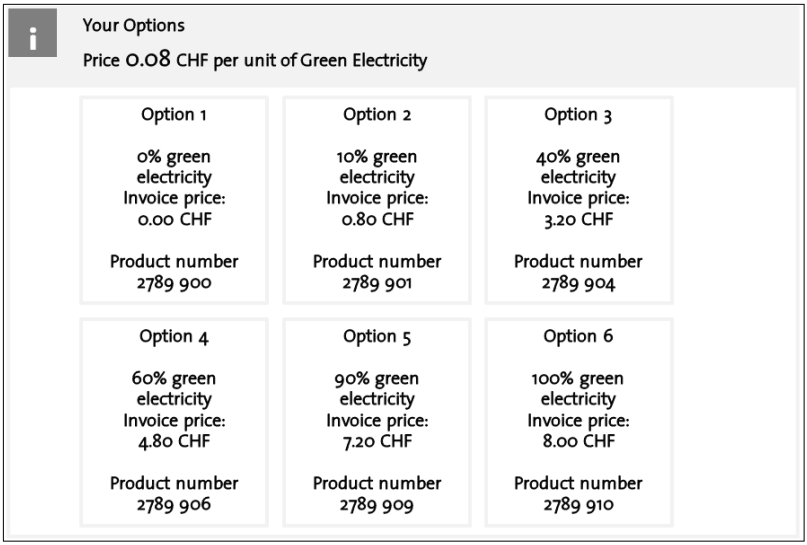

Figure 2.A.3: Options displayed on the sample data-sheet.

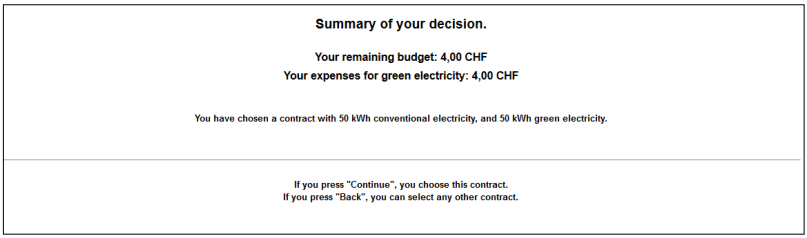

Figure 2.A.4: Summary of a sample decision.

Payoff

Please note that at the end of the experiment, one of your five decisions is randomly chosen to be relevant for your payoff. Your payoff consists of two parts:

(1) You will receive your remaining budget and your participation fee at the end of the experiment in cash.

(2) Your expenses for green electricity will be used by the experimenter to buy so-called certificates of 'Guaranteed Origin'. Such a certificate confirms that the demanded quantity of green electricity is actually produced in the Swiss electricity system. Therefore, according to your decisions you will finance the actual production of green electricity. The purchase of certificates of 'Guaranteed Origin' is directly

granted with an official letter of the institute, which is conducting the experiment. The certificates will be obtained subsequently to the experiment via the national power distributor. The letter explicitly states your demanded quantity and provides an on-line link where you will be able to access the certificates.

Start of Experiment

Please enter your seat number now on your computer screen. The number corresponds to the randomly assigned seat card. For a better comprehension of these instructions, the experiment will begin with a sample decision, in which your options will be explained in detail again. Subsequently, you need to answer some questions in order to check the comprehension of the task.

If you have questions, please give a hand signal. We will discuss your questions in person at your desk.

Example of a certificate of Guaranteed Origin (GO) for green electricity

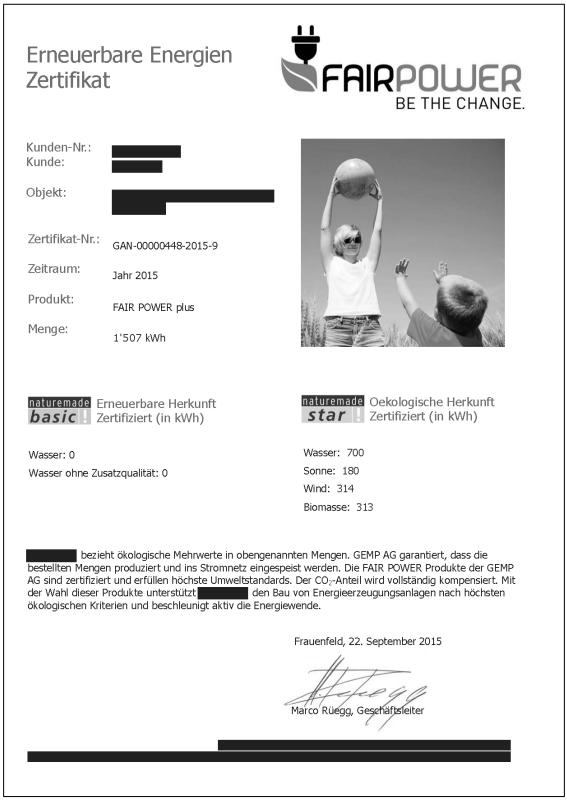

Figure 2.A.5: Example of a certificate of Guaranteed Origin for green electricity.

Note.— Certificates were ordered on a cumulative basis, as normally GOs are handled in magnitudes of GWh, rather than kWh. Subjects received a letter with their payment, indicating the amount of green electricity they purchased. Hence, they could easily identify their share.

Chapter 3

Costs and Benefits of Green Choice Defaults – A Field Study in the Residential Electricity Market*

Abstract

The use of choice defaults for influencing decisions of consumers and citizens to foster certain policy goals (e.g., the uptake of green electricity) has become increasingly popular. Yet little is known about the distributional consequences of such nudging interventions for different groups in society. We report results from a field study in which we contrast the choice of an electricity contract under a default regime with an active choice without any default. The default is an intermediately green and intermediately expensive contract. We find that this default distorts decisions in different directions. On the one hand, the default leads pro-environmentally minded, but uninformed consumers to choose less environmentally-friendly contracts. On the other hand, it also leads poorer households to choose options that are more costly than the ones they prefer in the active choice. A simple cost-benefit analysis reveals that whereas the green electricity default is successful at curbing greenhouse gas emissions, there are considerable downsides in terms of consumer welfare. Our results highlight the need for a careful evaluation of nudging interventions applied in public policy making.

Keywords: choice defaults, green electricity, nudges, consumer welfare
JEL Classification: D12, D31, D61, D63, H23, M38, Q48

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3.1 Introduction

Public policy makes increasing use of behaviorally-guided interventions (Chetty, 2015; Madrian, 2014). Choice defaults are particularly attractive as they have proven not only to have strong effects on behavior, but because they are also straightforward to implement, cheap to administer and do not infringe upon people's freedom of choice. Choice defaults have, for instance, been used to increase organ donation rates (Johnson and Goldstein, 2003), to foster retirement savings (Choi et al., 2003; Cronqvist and Thaler, 2004), or to fund public goods in the social or environmental domains (Carlsson et al., 2015; Ebeling and Lotz, 2015).

Whereas the effects of choice defaults on behavior are strong and well documented (see, e.g., Dhami, 2016, Chapter 22), much less is known about the potential distributional consequences of such interventions. Usually, the same choice default is equally applied to all people in society, which could—theoretically—benefit some groups, but may at the same time harm others. Hence, the implications of choice defaults on the welfare of different groups in society are much less clear—and much less researched—than their unambiguous behavioral effects.

Recent contributions in the behavioral economics literature stress the importance of evaluating policy intervention nudges not only on the basis of their effects on individual behavior, but to apply succinct cost-benefit analysis in order to analyze potential welfare implications (Allcott and Kessler, 2015; Bernheim et al., 2015). We contribute to this emerging work by providing novel empirical insights on the distributional effects of a choice default for the uptake of green electricity. In particular, we analyze how a choice default distorts the decisions of different groups of consumers compared to their preferences.

Residential electricity markets offer several interesting characteristics with respect to an analysis of choice defaults and their distributional consequences. First, the choice of electricity sources involves trade-offs between private and social benefits, which opens up an important 'playground' to apply behavioral nudges to increase social welfare (see for instance Allcott and Kessler, 2015; Ebeling and Lotz, 2015, for the application of nudges in energy markets). Second, consumers have sufficient opportunities to optimize their decision away from the choice default, both in terms of choosing even more environmentally-

friendly electricity contracts, but also in terms of choosing substantially cheaper options than the choice default. Hence, the availability of different electricity contracts ensures that consumers should be able to find a contract matching their preferences, thereby granting a choice environment in a libertarian paternalistic sense. Third, choice defaults that nudge consumers into an increased uptake of green electricity follow the intention of avoiding detrimental greenhouse gas emissions. Comparing the potential distributional costs of choice defaults with the amount of harmful emissions avoided allows for an evaluation of the cost-effectiveness of emission abatement of these behaviorally-guided interventions.

Four major research questions guide our empirical analysis. First, we investigate who is more likely to opt out of a default electricity contract and why. The answers to this question have an appealing practical relevance, as they help to identify general drivers of default behavior, which may go beyond observable demographic characteristics, but could be rooted in individual preferences and attitudes. Second, we assess the question of who prefers which electricity contract and why, when a choice has to be made in absence of a default. Combining these two steps, we can then study whether there are distortions in decision-making when contrasting an active choice of an electricity contract (without a default) with the currently held contract. By analyzing the frequency and the nature of choice distortions for different groups in society, we assess the potential distributional consequences of choice defaults. Such an analysis provides relevant insights in terms of how well a choice default attains an overall optimization of choices, and additionally permits statements on whether different groups in society incur different costs and benefits due to the intervention.

Finally, we ask whether the green electricity default applied in this study is cost-effective in curbing harmful greenhouse gas emissions. Contrasting potential losses in consumer welfare due to the choice default with the amount of harmful emissions avoided renders a clearer picture on the expenses of curbing externalities via a choice default.

For our research, we collaborated with an electricity utility that is a local monopolist in a Swiss city. The utility implements a choice default that provides an ideal field setting to answer our research questions. Households can choose between five different electricity contracts, ranging from very green (more environmentally friendly, more expensive) to

very grey (less environmentally friendly, cheaper) contracts. The choice default is placed on an intermediately green and intermediately expensive option and is the same for all customers.

Customers receive the default product whenever they do not make an active choice for a contract. They can opt out of the default simply by contacting the utility via phone, e-mail, letter, or on-line. Four years after the implementation of the green choice default in 2013, we administered a survey asking a representative set of households on their actually preferred electricity contracts in a well-tested elicitation format. Moreover, we examined consumers' reasons for their current contract choice, measured a range of personality characteristics, and collected data on socio-demographic variables. Our analysis of 1,362 survey responses offers multiple novel and policy relevant findings.

First, demographic data is only a weak predictor of opting-out of a green electricity default. Insights on the reasons for making choices generate a much clearer picture of why some respondents stay with the default, while others opt out. Second, preferences for an electricity contract largely depend on personal attitudes, such as political party affiliation or the level of importance towards nature conservation. Additionally, demographical predictors such as higher income or better formal education can predict to some extent preferences for greener electricity contract choices.

Second, the choice default in our setting distorts choices in several, distinct ways. It both prevents greener, more environmentally-friendly choices, and hinders choices to less expensive, greyer contracts. The first direction of distortion especially affects households who have pro-environmental preferences, but forgo a choice away from the default due to informational problems. The second direction of distortion concerns poorer households with lower socio-demographic status, especially in terms of education. Both distortions have negative effects on consumer welfare.

Third, a straightforward cost-benefit evaluation of the effectiveness of this choice default—using annual electricity consumption data, the carbon-intensity, and price differences of different electricity contracts—shows that, indeed, the green electricity default reduces externalities. However, this emission abatement comes at a considerable cost for consumers, which seems to be higher than at least some recent estimates of the social cost of carbon.

The results illustrate the impact of green electricity defaults on the welfare of different groups in society. As defaults seem inevitable in residential electricity markets,[1] choice architects need to be aware of these effects and take them into account when designing choice environments. As our findings demonstrate, the default's intended effect of curbing emissions from electricity consumption is achieved—to a considerable extent—at the cost of poorer households who would actively choose electricity contracts that are less costly to themselves. Our findings thus unfold the potential caveats of choice defaults in electricity markets and stipulate further discussions on how socially tenable behavioral interventions can be designed to achieve public policy goals.

The remainder of this paper is organized as follows. Section 3.2 summarizes the related literature concerning the evaluation of nudging interventions. Section 3.3 introduces and describes the importance of consumer choice in retail electricity markets. Section 3.4 details our empirical strategy, before Section 3.5 outlines and discusses our major results in more depth. Section 3.6 concludes.

3.2 Related literature

Since the publication of the popular article by Thaler and Sunstein (2003), the policy concept of 'libertarian paternalism', i.e., addressing societal problems with behavioral nudges that do not infringe with people's freedom to choose, has seen an unprecedented influence in public policy making (see, e.g., Lunn, 2014 or Sousa Lorenco et al., 2016 for policy examples and Chetty, 2015 or Madrian, 2014 for an overview of the academic literature on nudges and their influence on public policy). While the general attitudes of the public seem to be rather positive towards these tools of state intervention (Arad and Rubinstein, 2016; Sunstein, Reisch, and Rauber, 2017), the academic debate has evolved, with some abstraction, around three different sub-topics.

First, and of prime interest for this study, there is a growing strand of literature that aims at evaluating nudges beyond their mere effects on behavioral change, thus assessing the wider distributional and welfare consequences of applying behaviorally-guided interven-

[1] Legislations in many countries stipulate binding rules for utilities that even if no choice is made, households must receive a basic supply of electricity, which is of course tantamount to inevitably setting a default.

tions. Bernheim and Rangel (2005) are among the first to discuss how such interventions, in general, can be evaluated in terms of an economic welfare analysis. More recently, some authors have started to look into the welfare consequences of, for example, feedback on electricity consumption using social norms (Allcott and Kessler, 2015), of energy efficiency advisory programs for homeowners (Allcott and Greenstone, 2017), of energy efficiency standards and subsidies in the light-bulb market (Allcott and Taubinsky, 2015), of reminders wishing to attain higher fund-raising rates (Damgaard and Gravert, 2017), or of salience of tax displays (Chetty et al., 2009).

For the specific nudge of choice defaults, Bernheim et al. (2015) have looked into the welfare effects of default options in 401(k) pension saving plans in the United States. They show that the allocation of an optimal default, in the sense that the nudge should enhance overall welfare, is challenging and decisively depends on the welfare perspective of the choice architect, as well as on the reasons why people stay with a default. In connection to this work, Brown and Weisbenner (2014) examine who chooses which pension contribution plan and why. Inter alia, they find that the pension plan choice can be predicted with demographic factors, such as income or education. However, they also claim that usually unobservable characteristics in administrative data such as personal attitudes, beliefs on plan outcomes, or financial literacy can considerably increase predictive power. Thus, they argue that the scope of administrative data alone may not allow for a precise prediction where to possibly set a default, yet alone to judge the overall effectiveness of a nudge.

In addition, in a recent study, Brown et al. (2016) find, using comprehensive survey data from the State Universities Retirement System (SURS), that default options for pension plan choice can have complex consequences for individual welfare. Specifically, the authors argue that individuals who have been defaulted into a plan later regret their 'choice', leading to potentially negative consequences for individual welfare. Our study closely relates to the general advances of Brown et al. (2016) and Brown and Weisbenner (2014), yet our approach differs in several aspects. First, the context of our study is green electricity defaults, which means that the main purpose of the nudge is not necessarily to optimize individual decision-making (as in the case of retirement savings), but rather to foster a public good by creating a higher demand for green electricity. Second, while we

are also interested in the question of who chooses which (electricity) contract and why, we do not use a measure of regret to assess potential distributional consequences. In our empirical approach we deem that regret, as measured in Brown et al. (2016), is unsuitable because of the low consumer involvement and the moderate monetary stakes associated with decisions about electricity contracts. Instead we rely on asking respondents which contract they actually prefer in an active choice type of setting, thus uncovering their 'true' preferences for an electricity contract.[2] Third, our data contains the annual electricity consumption of each of our respondents. Assessing the match-mismatch relation between the actually preferred contract and the contract consumers currently hold, enables us to make static predictions about individuals' costs and benefits of choosing an electricity contract in a decision environment with a choice default. Moreover, we are also able to evaluate whether the green electricity default used in this setting is an efficient instrument (in terms of monetary costs caused and environmental benefits generated) to curb harmful emissions from electricity consumption.

A second related field of the literature is concerned with the question whether nudge-style interventions actually correspond to the criteria of libertarian paternalism, i.e., whether they help consumers attain a fit with their preferences (Camerer et al., 2003; Carroll et al., 2009; Choi et al., 2003; Keller et al., 2011), as well as with how well aspects of nudges mesh into a political economy framework of potentially self-interested choice architects (Schnellenbach, 2012; Schubert, 2017; Sugden, 2013). Our paper contributes to this literature by adding substantive field evidence that a prime criterion of libertarian paternalistic choice defaults, i.e., the inhibition of inconsistent choices, does not necessarily hold. In this way, we confirm and extend the laboratory findings of Ghesla (2017). Additionally, our results allow for more insights into the political economy aspect of setting a default, thus adding to the discussion of whether self-interested choice architects can actually use nudges to the overall benefit of a society.

[2] This approach is in line with a key assumption in the framework of Chetty et al. (2009, p. 1170) that given full saliency of taxes, "the agent chooses the same allocation as a fully-optimizing agent." This implies that an active choice without a default reveals fully-optimizing choices on behalf of respondents. We will use this assumption in Section 3.5.4 to guide our cost-benefit evaluation of the green electricity default.

At last, there is also a rich debate on the broader ethical and philosophical implications of using nudges, as well as on their societal consequences (see, e.g., Bovens, 2009; Desai, 2011; Gigerenzer, 2015; Hausman and Welch, 2010; Oliver, 2015; Rebonato, 2014; Sunstein, 2015). Our results may be of interest in the light of this discussion, as we provide an empirical illustration of who may benefit and who may lose when policy makers apply nudging interventions.

3.3 Consumer choice in residential electricity markets

The generation, transmission, distribution, and retailing of electricity have long been considered 'vertical' processes, i.e., tightly linked back-to-back steps of getting power from producers to consumers (Jamasb and Pollitt, 2005). Traditionally, state-owned power monopolies were considered best suited to deal with the vertical integration of the industry, shouldering the large investments in electric infrastructure, such as central power plants and country-spanning grid-lines. However, with the advent of modern information and computer technologies in the last 25 years, there has been a consistent reduction of monopolistic regulation in infrastructure industries, such as in air and rail travel, telecommunications, or postal services (Schneider and Jäger, 2003). The industry for the provision of electricity is no exception to this trend. De-regulation or liberalization of said industry is nowadays an accepted and promoted strategy in public policy making (Joskow, 2006). For instance, approximately half of the federal states in the U.S. have liberalized their power markets offering substantial consumer choice of power suppliers (American Coalition of Competitive Energy Suppliers (ACCES), 2017). Likewise, similar advances of opening up markets in order to strengthen opportunities for consumer choice for different electricity products have been brought forward in the EU (European Commission, 2017a), the United Kingdom and Australia (for an overview see International Energy Agency (IEA), 2005).

Consumer choice entails that households are not any longer mere recipients of electricity provided by an incumbent supplier, but that they have a choice of different electricity products. Differentiation of electricity products occurs in at least two, often interlinked, dimensions. First, competitive retailers may offer electricity at different prices per unit

of electricity, which may differ between retailers depending on their cost structure, their possibilities to produce their own electricity, and their need to acquire electricity demanded on wholesale markets.[3]

A second important dimension of differentiation are the underlying production sources for each unit of electricity that can be demanded by consumers. It is important to highlight that with the de-regulation of monopolistic electricity markets, consumers do not any longer receive the electricity mix provided by the incumbent utility, but can actively shape the composition of power sources used to satisfy their demand. However, in order to recognize and appreciate this dimension of differentiation, it is vital to understand the concept of 'additionality'. Naturally, consumers cannot differentiate between different sources for electricity supplied to them. Hence, physical flows of electricity may not necessarily encompass the electricity sources demanded by a consumer. Therefore, in de-regulated electricity markets the physical production of electricity and potential benefits of using different power sources, such as a reduction of greenhouse gas emissions, are de-coupled. Taking a simple example: If a retailer markets a green electricity product sourced from 100% wind power, then a consumer accepting this offer does not necessarily physically obtain 100% of her demand with wind-powered electricity. A purchase of such a contract just entails that the retailer is obliged to obtain a guarantee that anywhere in the power system the demanded wind electricity has actually been produced. Obtaining such guarantees of origin constitutes the backbone of the concept of additionality. It is the *additional* environmental value of the produced power that is actually used to sell electricity contracts stemming from different sources. Thus, while consumers continue to be physically supplied with the electricity mix from the local grid, their choices of contracts have an upstream effect on the general composition of power sources used to produce electricity. Or to put in other words, the choice of consumers of differentially sourced electricity contracts provides investment signals for producers of renewable electricity.[4]

[3] As this chapter has an introductive character, we omit a thorough discussion of different pricing strategies; the interested reader may be referred to, e.g., Joskow and Tirole (2006) for a more nuanced discussion. Also, the electricity market in which we carried out this study uses only a fixed form of time-variant pricing, i.e., different, but uniform per-unit prices for electricity during day and night times.

[4] Note that while prices and environmental value are deemed the most important dimensions of consumer choice in electricity markets, there may be also other dimensions of differentiation, like for instance the level of customer service, the variety of billing options, or additional services provided with the

Mobilizing consumer interest for the dimension of additionality of an electricity contract is a prime interest of many governments around the world. For instance, the European Commission (2015, p. 2) advocates in its vision for an Energy Union that "citizens (...) participate actively in the market," thus claiming that consumer engagement to increase green electricity demand is an important pillar for environmental policy. Green electricity production from sources such as wind, solar, or biomass is assumed to produce less greenhouse gas emissions than conventional sources of electricity production. Thus, as a consensus develops that anthropogenic emissions, including emissions from electricity production, cause climate change (Intergovernmental Panel on Climate Change (IPCC), 2014), consumers opting for and demanding more green electricity may help governments attain important policies targets, such as for instance, renewable portfolio standards in the United States (National Conference of State Legislatures (NCSL), 2017) or the 33% share of renewable electricity production by 2020 in the European Union (European Commission, 2016b).

In the United States, the US Department of Energy (2016) reports that there are roughly 850 utilities offering green electricity from different sources and varying prices. Additionally, there are also other schemes, such as 'Community Choice Aggregation (CCA)', which have been adopted in several federal states, bundling consumer choice on a community-wide level and offering green electricity.[5] Likewise, the Agency for the Cooperation of Energy Regulators (ACER) (2014) estimates that in the European Union there are at least 280 electricity suppliers that offer more than 690 different tariffs that include shares of green electricity production. Evidently, the liberalization of electricity retail markets has brought forward significant opportunities for consumer choice.

However, the share of new renewable electricity production, i.e., production from wind, solar, biomass, and geothermal sources, amounted to only about 15% of total production of the United States net power generation of renewables in 2016 (Energy Information Administration, 2017) and to 24% in the EU (Eurostat, 2017). This lack in demand seems to speak against the generally assumed positive willingness-to-pay for green electricity

electricity contract, such as the bundling of products for heating and electricity use.

[5] As of today, more than 5% of the U.S. population are offered an electricity choice under CCA-schemes (Local Power Inc, 2017).

(see Soon and Ahmad, 2015, for an overview). Upon closer examination, however, it becomes clear that although liberalization opens up electricity retail markets for choice, most consumers remain inactive when it comes to actually choosing an electricity contract (Agency for the Cooperation of Energy Regulators (ACER), 2014).[6] In fact, according to numbers of the European Consumer Organization (2016) less than 10% of consumers in the EU have switched contracts or supplier in 2013. Thus, it seems that liberalization alone may not deliver the push in demand for green electricity that was initially hoped for.[7]

In this vein, with the recent advances in behavioral economics and their implications for policy design (Chetty, 2015; Madrian, 2014), retailers in electricity markets have made increasing use of behaviorally-guided policy instruments to nudge people in socially desirable ways (e.g., Allcott, 2011; Costa and Kahn, 2013; Momsen and Stoerk, 2014; Newell and Siikamäki, 2014; Sunstein and Reisch, 2014). One of the most common nudges is the use of green electricity defaults, which sets the standard contract to a 'green' option. Thus, if households want to choose any other contract, they need to actively opt-out of the default. As the active engagement of consumers within the electricity retail market is rather low, defaults have a strong effect on green electricity uptake (Ebeling and Lotz, 2015). While it is important to note that choice in retail electricity markets almost always involves that a default option is present, the precise positioning of these defaults towards more expensive green options may involve diverging distributional consequences for different consumers groups, which constitute the prime subject matter of this paper.

3.4 Study design and sample composition

Our study makes use of a field setting in which a specific green electricity default has been imposed on all residential consumers of a Swiss utility. Issuing a mail and web-based survey four years after the default has been set, we analyze whether the preferences for an

[6] The Agency for the Cooperation of Energy Regulators (ACER) (2014) lists several potential reasons for this dis-engagement on behalf of the consumers in the market, such as lack of trust or perceived complexity of the choice.

[7] Note that here we focus on the demand side; governments also make use of a range of supply side measures to incentivize sustainable electricity production, such as feed-in-tariffs or renewable funds (for an overview Gan et al., 2007).

electricity contract, elicited in an active-choice format, correspond to the current contracts held by the households. Thereby we identify the mismatches between currently held and actively preferred contracts.

3.4.1 Survey method and procedure

We surveyed residential electricity consumers in our field setting in March and April 2017 and randomly selected 12,000 households who had a valid billing address to receive a questionnaire. The sample was stratified by zip codes in order to ensure reaching—as far as possible—a demographically representative segment of customers.[8] Furthermore, we ascertained that the targeted sample shared equal proportions of the different electricity contracts as in the total population. After eliminating duplicate addresses, we sent out survey packages to 11,989 households. A survey package included a cover letter, a printed and folded questionnaire, and a pre-stamped reply envelope.

Households who chose to participate could either do so by filling out the paper questionnaire or by using an on-line link, which was mentioned in the cover letter.[9] In total, we received 1,906 questionnaires (a response rate of 16%). 380 respondents chose the on-line route and 1,526 sent their questionnaire via ordinary mail. Importantly, as the analysis is executed in a stepwise format for different categories of predictors (see Section 3.5 for specifics), we make use only of fully completed questionnaires, which determines a final sample size of 1,362 respondents.[10]

Customers of the electric utility could choose from five different contracts, which varied in their prices per unit of electricity. The mean monetary 'distance' between the cheapest and the most expensive contract is roughly 15 Swiss cents per kilowatt hour. Figure 3.1 shows an overview of the five contracts available for selection.[11]

Table 3.1 provides an overview of the distribution of the different electricity contracts for the randomly selected sample, the sample data received, and the final sample of fully

[8] Note that having a valid billing address implies that the respective household is entitled to choose an electricity contract. Additionally, we hypothesize that key demographic variables, such as income, may be different between zip codes. Therefore, we stratify our targeted sample in this respect.

[9] Translated copies of the materials used can be found in the appendix.

[10] We thus keep the sample size constant across all analyses to ensure comparability. This list-wise deletion approach is more conservative than other methods (such as mean substitution or regression imputation). As we retain a large enough number of responses, statistical power is preserved (Allison, 2001).

[11] In order not to reveal the identity of the partnering utility, we use self-invented labels for each contract.

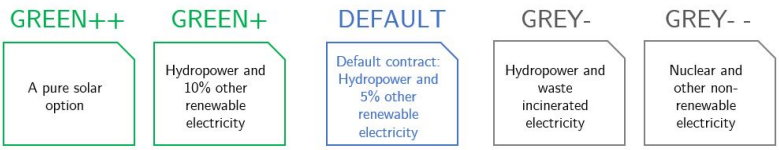

Figure 3.1: Selection of contracts

Note.—Prices per unit of electricity increase from GREY−− to GREEN++.

completed questionnaires retained for analysis.[12] The distribution of contracts in the randomly selected sample is representative of the population distribution by design. In the sample of received questionnaires, households who have actively opted out of the default are over-represented. This self-selection of households into the survey seems intuitive, as such respondents may have a higher interest for the topic in general. Households who remain in the default are thus slightly underrepresented. These observations also hold for the final sample used for the analysis. Importantly, this likely means that our results underestimate the choice distorting effect of the green electricity default.

Table 3.1: Distribution of contracts across random, received, and final sample

Electricity Contracts	Random Sample	Received Sample	Final Sample
GREEN++	0.29%	1.11%	1.40%
GREEN+	3.03%	8.71%	9.25%
DEFAULT	76.89%	64.85%	65.55%
GREY−	7.78%	12.33%	11.75%
GREY−−	12.01%	13.01%	12.11%
Total	100.00%	100.00%	100.00%

The questionnaire set off with an active-choice type of question that was embedded into a cheap-talk design (Cummings and Taylor, 1999). The active choice asked respondents to indicate which electricity contract they would choose, if they had to decide right now. The menu of contracts presented the actual electricity contract options available by the electric utility: prices per electricity unit were mean retail tariffs between 2013 and 2017

[12] Note that in accordance with Internal Review Board regulations and our legal agreement with the partnering electric utility all contractual data was anonymized such that it was impossible for the researchers to identify individual households. We used a generic identifier number to match the data on currently held contracts that we received directly from the utility with the data elicited from the consumers in the survey. Further, due to confidentiality reasons, we do not show the exact distribution of contracts in the total population.

(denoted in cents per kilowatt hour).[13] The description of electricity sources provided the same information as in the real contract choice.

The questionnaire then proceeded with three questions on energy literacy, with questions on reasons for making electricity contract decisions and on environmental preferences, evaluated the self-perception of the ability to decide between differing opportunities, looked into preferences for time discounting, trust, and altruism (using the items proposed by Falk, Becker, Dohmen, Huffman, and Sunde, 2016), and assessed political orientation of respondents. At last, we elicited standard socio-demographics such as age, gender, nationality, family situation, property ownership, occupation, education, and income.

3.4.2 Limitations of the survey data

Much like Dhami (2016), we believe that structured and carefully collected survey data benefits the analysis of behavioral phenomena and may complement results from tighter controlled laboratory experiments. Especially, more information and knowledge on key characteristics and demographics of respondents may increase the opportunity to provide insightful advice for economic policy making. Nevertheless, there are, at least, three important limitations of our survey approach, which we would like to address before subsequently presenting the descriptives of our sample and the corresponding results with regards to our research questions.

A first limitation concerns the hypothetical nature of our elicitation strategy of the actually preferred electricity contracts. The methodological gold standard in experimental or behavioral economics is to elicit choices in an incentive compatible fashion, i.e., providing extrinsic motivation for choice via monetary incentives. Ideally, we would thus have preferred to incentivize choices by directly implementing the actively preferred contracts on behalf of the households. However, this would have implied that our questionnaire was designed in a legally binding way—a design choice that was neither managerially feasible, nor organizationally possible within the time-frame of the study. Therefore, we opted to elicit hypothetical active choices of the preferred electricity contracts.

Earlier studies concerned with the estimation of the willingness-to-pay for environmen-

[13] We used the mean values of these tariffs in order to reflect the price fluctuations for the different contracts.

tal goods, such as green electricity, have found that hypothetical and actual valuation might differ if choices are inconsequential for respondents (e.g., Horowitz and McConnell, 2002). For our study this means that even though households might indicate to have preferences for green electricity, but if they had to incur the actual cost, they would refrain from switching to a greener, but also more expensive contract. In order to limit such potential hypothetical bias, we made use of a cheap-talk script, a well-founded design method (see Cummings and Taylor, 1999), which has been numerously and successfully applied in contingent valuation studies in environmental economics (e.g., Brown, Ajzen, and Hrubes, 2003; Murphy, Stevens, and Weatherhead, 2005). Cummings and Taylor (1999) demonstrated in several experiments that informing respondents on the existence of hypothetical bias, rather than just reminding them of budget constraints (such as in Loomis, Gonzalez-Caban, and Robin, 1994), effectively mitigates hypothetical bias. We follow this approach and implement a brief cheap-talk script before our active choice. Specifically, we explain in a simple example the difference between hypothetical and actual decision-making, thus explaining hypothetical bias to respondents. Consequently, respondents are asked to cast their active choices as if they were real. Thus, even though we cannot rule out completely that respondents' active choices in the questionnaire may still show some hypothetical answer biases, we tried to minimize this potential problem with previously well-tested methods. In support of this approach, results from other recent studies in the environmental domain point towards the effectiveness of cheap-talk designs to mitigate hypothetical bias in stated willingness-to-pay estimates (Howard, Roe, Nisbet, and Martin, 2017; Tonsor and Shupp, 2011).

In the same vein, a second limitation of our active choice concerns the situation where respondents may just randomly select one of the five offered contracts, as the choice is inconsequential to them. We can evaluate the extent of this potential problem by checking whether customers who have opted-out of and hold a current contract other than the default make active choices in our study that are consistent with their currently held contract. If random choice poses a problem to our data collection, we should observe only 20% of consistent matches for these households. Yet, on average 60% of respondents who hold a current contract other than the default make a consistent choice in the hypothetical

active choice in our questionnaire. Thus, we oppose the view that respondents did not take the hypothetical choice seriously.[14]

A third limitation concerns the self-selection of certain households in our study. While we made sure that our randomly selected sample does not differ from the population of the utility's customers in terms of electricity contract choice and zip code (see Table 3.1 for details), we cannot prevent that households who are potentially more interested in the topic of energy in general or who are more keen to enter prize draws, are also disproportionately represented in our final sample. It may well be that the 16% of households in the contacted sample who responded to our survey are fundamentally different in preferences, characteristics, or demographics than the 84% who did not respond. However, as we will discuss in more detail in Section 3.4.3, our final sample does not seem to be particularly different with respect to a number of observable characteristics of the total sampled population. Thus, we conclude that self-selection-bias is not a major concern in our study, at least with respect to those variables which we are able to observe. Moreover, even if, for instance, households who are better informed about electricity contract choices or who are more interested in the topic do answer our questionnaire more frequently than those who are not, our results should be of particular informational value as they may present—for most of our results—a lower bound of the choice distorting effect of green electricity defaults for different groups in society.

3.4.3 Characteristics of the final sample

In our analysis, we consider four sets of explanatory variables. We describe these variables below and discuss to what extent our final sample is representative of the total customer population by comparing the sample means to the population values of these variables wherever possible.

[14] We can only speculate on the reasons why some of the households who have already opted out of the default contract show inconsistencies in the hypothetical choice. It could be that those respondents cast socially-desired answers, which we could not prevent with our cheap-talk design. Approximately 74% of respondents who are inconsistent between current contract choice and actual preferences elicited in the questionnaire have cheap, and less environmentally-friendly current contracts, but hypothetically prefer expensive, and more environmentally-friendly electricity contracts. A different line of argumentation of why these inconsistencies occur could be that some households do not possess stable preferences for an electricity contract, but that preferences are constructed as the choice itself emerges (Ariely, Loewenstein, and Prelec, 2003). See Table 3.A.1 in the appendix for the specific numbers of consistent and inconsistent choices for each contract.

Demographics We collected standard demographic variables, such as age, gender, nationality, native language, being a single household, being a single parent, having children, property ownership situation, occupation, education, and income—these key variables have been used both in studies concerning the effect of defaults (e.g., Brown and Weisbenner, 2014), as well as in studies concerning the elicitation of preferences for different electricity contracts (e.g., Kaenzig et al., 2013). Comparing the demographic estimates with either specific data from the Swiss city we have sampled in or with data from the general Swiss population, we attain a satisfactory level of representativeness in terms of the distribution of property ownership situation, occupation, education, and income distribution.

Energy literacy We asked several questions assessing respondents' energy literacy. We find that less than a third of our respondents is able to correctly identify the average annual electricity consumption of a four-person family household. More than half of the households does not know the share of new renewable electricity (such as solar, wind, or biomass) in the market. Likewise, households seem to underestimate the already relatively high share of hydro power in the electricity system. In sum, there is substantial dispersion in our sample concerning the level of knowledge necessary to make an informed decision about an electricity contract.

Attitudes We elicited respondents' political attitudes, their preferences for the environment, their general ease at decision-making, as well as their attitudes and tendencies with regard to their patience, trust and altruism. Mean political attitude roughly corresponds to numbers of a representative national sample of the Swiss population even though it is slightly off to the left (Swiss Household Panel, 2017). There are no population estimates available for comparison for the other attitudes which we have elicited in our questionnaire.

Reasons for choice Finally, we asked individuals to think of their current electricity contract and presented them with a number of statements capturing reasons why they did or did not make a choice. These statements followed the main categories used in Brown et al. (2011) and Brown et al. (2016), i.e., looking at information related problems,

the impact of effort and procrastination, and the perception of the default as a recommendation. Roughly 40% indicate that they did not inform themselves before making a contract choice, a third of our respondents deems the choice rather complex, and still one sixth indicates that they were not aware that they had a choice at all. Thus, the level of information concerning the choice of an electricity contract seems to be rather dispersed across the sample. Electricity is often classified as a low-involvement good, as customers rarely change their contracts or inform themselves on their consumption levels. Nevertheless, our respondents do not deem the decision for an electricity contract as unimportant, apparently more than 80% deem it an important or very important decision. In line with this, procrastination or forgetting to decide only concerns a minor fraction of respondents in the sample. Furthermore, while choosing a contract, the recommendation signal of the default seems to play a role—over half of our respondents indicated that they interpreted the default as a recommendation by the utility. Additionally, we asked respondents who makes the decision for an electricity in their households. 42% indicate that they make such choices by themselves and 53% say that they make the choice together with their partner or flat mates. Less than 5% indicate that they do not make such choices.

In total, from a purely descriptive point of view, it seems that we have sampled a heterogeneous, and in several respects, representative fraction of our target population, i.e., of all consumers of the utility with which we cooperated. Table 3.A.2 in the appendix reports the complete summary statistics for our final sample.

3.5 Results

3.5.1 Who stays in the default?

Four years after the implementation, still over 75% of the households held the DEFAULT contract.[15] This large default effect is comparable to previous studies. Ebeling and Lotz

[15] Unfortunately, the utility could not provide figures of the distribution of contracts before the new default was introduced, hence we are unable to estimate the exact size of the default effect at the introduction. However, we could assess the change in electricity mix before and after the introduction of the green default. Prior to the introduction of the new default, customers could mix different electricity sources to form their own product. If a household did not choose to mix, it was provided by default with a substantially greyer option than the current green default (i.e., approximately 1% were from new

(2015, p. 869), for instance, find in a randomized controlled trial that "69.1% of purchased contracts were 'green'" in an opt-out decision treatment. We take this as evidence that the default manipulation in our natural field setting was successful and allows for further investigation of our research questions.

In this section we are mainly interested in the determinants of staying with the default versus opting out, thus investigating more into who is particularly prone to default and why. For this, we construct a dependent variable that takes the value 1 if a respondent has opted out of the default in the current contract choice, and that is 0 otherwise. We use a linear probability model (LPM)[16] to analyze—in a stepwise fashion—the influence of the four sets of explanatory variables elicited in the questionnaire.[17] Table 3.2 displays the results of this analysis, which we discuss in detail below.

Demographics Beginning with model (1) in which we consider only demographic variables as predictors, we can immediately see that the explanatory power of demographical variables for explaining whether respondents stay in or opt out of the default is rather limited. The higher the age of respondents, the more likely they are to opt out. Male respondents opt out more often than women.

Energy literacy Turning to model (2) only assessing the effects of variables capturing energy literacy on opting out of the default, we find that respondents who know the share of new renewables in the electricity mix have a higher probability of opting out of the default. Additionally, people who assess themselves as being capable of explaining the concept of green electricity easily are more likely to opt out of the default.

Attitudes In model (3) we see that political attitude is also a significant predictor of opting out of the default. We elicited political attitude on a scale from 0 (politically left-minded) to 10 (politically right-minded). The more respondents move up that scale,

renewable sources, 17% thermal waste, 33% from hydro-power, 49% nuclear). After the introduction of the green default, the shares in renewable electricity supplied to households almost doubled to 62%, thus indicating a strong effect of the default on renewable electricity demand.

[16] Note that LPM estimates can be directly interpreted as the change in the probability of opting out of the default associated with a one unit increase of the predictor variable.

[17] This stepwise approach allows to study the influence of a set of predictors in isolation, thus we can avoid that certain effects of predictors may be masked due to multicollinearity issues. Each of the analyses also estimates a full model with the complete set of predictors.

Table 3.2: Linear probability model | Opting out of the default

	Demo-graphics (1)	Energy literacy (2)	Attitudes (3)	Reasons for choice (4)	Full model (5)
Age	0.003***				0.001
	(0.001)				(0.001)
Gender: Male	0.078***				−0.003
	(0.029)				(0.030)
Nationality: Swiss	−0.033				−0.050
	(0.040)				(0.041)
Language: Native	0.052				0.026
	(0.046)				(0.046)
Single	0.040				0.009
	(0.036)				(0.040)
Single parent	−0.053				−0.057
	(0.089)				(0.085)
Has children	0.050				0.054
	(0.035)				(0.034)
Owns property	0.025				−0.011
	(0.034)				(0.033)
Occupation, *Base: Full-time*					
Part-time	0.028				0.023
	(0.036)				(0.035)
Self-employed	0.019				0.057
	(0.055)				(0.053)
In training/ in school	−0.057				−0.035
	(0.073)				(0.071)
Seeking work	0.006				−0.016
	(0.127)				(0.129)
House wife / house husband	−0.068				−0.040
	(0.078)				(0.077)
Retired	0.005				0.026
	(0.055)				(0.053)
Education, *Base: Compulsory schooling*					
Vocational training	−0.072				−0.066
	(0.096)				(0.099)
A-levels	0.038				0.026
	(0.104)				(0.106)
Higher education not university	−0.028				−0.036
	(0.099)				(0.102)
University	−0.015				−0.035
	(0.097)				(0.101)
Income, *Base: below CHF4,000*					
CHF4,001-6,000	−0.062				−0.060
	(0.049)				(0.048)
CHF6,001-8,000	0.039				0.009
	(0.051)				(0.050)
CHF8,001-10,000	−0.004				−0.027
	(0.056)				(0.053)
CHF10,001-12,000	−0.011				−0.049
	(0.063)				(0.060)
CHF12,001-14,000	−0.029				−0.064
	(0.069)				(0.067)
CHF14,001-16,000	0.064				0.009
	(0.079)				(0.076)
above CHF16,000	−0.005				−0.051
	(0.080)				(0.079)
Zip code fixed effect	Yes				Yes

Table 3.2 continued

	Demo-graphics (1)	Energy literacy (2)	Attitudes (3)	Reasons for choice (4)	Full model (5)
Energy literacy					
Knows consumption question		0.014			0.027
		(0.027)			(0.027)
Knows new renewable mix		0.082***			0.058**
		(0.026)			(0.026)
Knows hydro mix		−0.021			−0.018
		(0.029)			(0.029)
Explains concept of green electricity easily		0.091***			0.028
		(0.017)			(0.020)
Political attitude			0.014**		0.011*
			(0.006)		(0.006)
Personal attitudes					
Nature importance			0.013		−0.002
			(0.009)		(0.009)
Ease of decision			0.023***		0.009
			(0.006)		(0.006)
Patience			−0.011**		−0.009*
			(0.005)		(0.005)
Trust			−0.009*		−0.002
			(0.005)		(0.005)
Altruism			−0.006		−0.014**
			(0.005)		(0.006)
Reasons for choice					
I informed myself before choosing				0.105***	0.089***
				(0.016)	(0.018)
It was a complex decision				−0.046***	−0.038**
				(0.014)	(0.015)
I did not know that I could choose				−0.003	−0.006
				(0.013)	(0.014)
It was an unimportant decision				0.008	−0.001
				(0.015)	(0.016)
I forgot to decide				−0.021	−0.032**
				(0.016)	(0.016)
The default appeared to be a recommendation				−0.023*	−0.026**
				(0.012)	(0.013)
I kept the effort low				0.006	0.005
				(0.014)	(0.014)
I actually never made a decision				−0.052***	−0.036**
				(0.017)	(0.017)
I did not have enough info about the products				0.032**	0.031**
				(0.015)	(0.015)
Decision taking, *Base: Alone*					
Together				−0.054**	−0.039
				(0.026)	(0.033)
Not aware of decision				−0.083*	−0.069
				(0.046)	(0.052)

Table 3.2 continued

	Demo-graphics (1)	Energy literacy (2)	Attitudes (3)	Reasons for choice (4)	Full model (5)
Constant	0.104	0.045	0.151	0.287***	0.317*
	(0.123)	(0.050)	(0.111)	(0.085)	(0.189)
R^2	0.043	0.033	0.026	0.107	0.139
Observations	1,362	1,362	1,362	1,362	1,362

Note.— *p<0.1; **p<0.05; ***p<0.01
Linear Probability Model (OLS); the dependent variable is 1 if the respondent opted out of the default and 0 otherwise. Five models are shown in order to allow for the stepwise interpretability of different sets of coefficients. First, we analyze the influence of demographics only on default choice. Next, we look into whether energy literacy can predict the choice of an electricity default contract. In model (3), we assess the impact of attitudes on opting out of the default. Last, we assess the impact of reasons for choice on default rate. The final column estimates the complete model. Only fully completed questionnaires are considered. Heteroskedasticity-robust standard errors are in parentheses.

the higher the probability that they opt out of the default. Of intuitive appeal are some of the results concerning the personality and attitude measures. For instance, the more respondents are at ease to decide between two options, the more likely they are to have opted out of the default. Measures on time discounting (patience) and trust reveal that people who are more patient and more trusting are more likely to stay with the default.

Reasons for choice Model (4) concludes the analysis by providing more in-depth insights into why households are more or less likely to opt out of the default electricity contract. Specifically, we can see that the more respondents inform themselves before choosing, the more they opt out of the default. Likewise, the more complex customers perceive the decision, the less likely they are to opt out. Perceiving the default as a recommendation provided by the utility, also increases the propensity to stay with the default. Finally, the more people indicate to have procrastinated on the decision, the less likely they are to have opted out.[18] If we assess whether the decision is made by a single person only, or whether people jointly decide on a contract, we can see that making the decision together actually leads to a higher probability of staying with the default. Furthermore, those who are not aware of the possibility to choose also have a lower probability of opting out of the default. In sum, the data on choice processes and reasons for choice better explains the variation in default choice and opting out than the three preceding models (R^2 reasons for choice 0.107).

[18]The results on insufficient product information do not seem intuitive at first sight. In fact, the more people indicate that they lack information on the products, the higher the rate of opt outs. We may only speculate that not receiving enough information may generate resistance towards the default option.

Full model The full model (5) confirms that demographic data are only weak predictors for determining whether people opt out of a green electricity default, since none of the demographic variables remain significant. Energy literacy and attitudes are, to some extent, more informative. Respondents who know the correct share of new renewables in the market are more likely to opt out of the default. Additionally, more right wing political attitude seems to increase the probability to opt out of the default. Nevertheless, the reasons for choice seem to be most important for determining whether respondents opt out or stay with the default. Specifically, the more respondents agree that they have informed themselves the higher the probability that they opt out. Likewise, perceived complexity of the decision, forgetting to decide, seeing the default as a recommendation, and procrastination lead to higher default rates.

In conclusion, this first analysis provides some interesting insights about the reasons why the green electricity default in this setting works. In particular, it illustrates that demographic data only is not enough to make meaningful predictions about whether a household will stay with a default. Choice processes and reasons for choice are the more relevant predictors.

3.5.2 Active choices: who chooses which electricity contract?

The next step in our analysis is to examine the determinants of the active choice preference for an electricity contract. As described in Section 3.4.1, respondents could choose from a menu of five electricity contracts, which mirrored exactly the options offered in the real decision. Therefore, the dependent variable in question has five different outcome categories (i.e., GREY−−, GREY−, DEFAULT, GREEN+, GREEN++). These outcomes are ordered in the sense that the levels of environmental friendliness and per unit prices increase with each category from GREY−− to GREEN++, which provides a suitable setting for an analysis of the data with an ordered Logistic model. The ordered Logistic model estimates the probability of choosing one contract against all others. In Table 3.3 we use as a base category the contract GREEN++, thus the reported odds-ratios provide an indication whether changes in certain predictors are associated with an active prefer-

ence for greyer contracts.[19] Specifically, odds ratios above 1.00 indicate that a change in the predictor increases the probability of choosing a greyer contract than GREEN++.

Demographics Model (1) in Table 3.3 examines the influence of the demographic variables on the active choice of electricity contracts by our respondents. We observe that being male and having children adds to the probability of actively choosing less environmentally-friendly, and less expensive electricity contracts. Furthermore, being part-time employed or self-employed increases the probability of choosing greener contracts. Even more insightful predictors are education and household income, as the preferences for greener contracts increase with the level of formal education and income. Note, however, that the odds-ratios in the ordered Logistic estimation for higher income levels are only directionally below 1.00. In sum, the results highlight the importance of higher socio-economic status (in terms of education and income) as a relevant predictor for a greener electricity contract choice.

Energy literacy Turning towards the influence of energy literacy on electricity contract choice in model (2), we find that knowing the mix of new renewable electricity in the markets, as well as being at ease to explain the differences between green and grey electricity adds to the probability of choosing a greener electricity contract.

Attitudes Assessing results on the impact of attitudes on electricity contract choice in model (3), we find that greener active choices of electricity contracts are more preferred by people with politically left attitudes and people who attach a higher importance to nature and environment conservation. Additionally, the more people are at ease to decide between two options, the more they choose greyer contracts actively. Eventually, the more altruistic people are, the less likely they choose greyer contracts.

[19] The ordered Logistic model requires that we assume that the order of outcomes is proportional, i.e., implying that the perceived 'distance' between each of the contracts is equal. This assumption may not fully hold in our case, as differences in price and environmental friendliness are not uniform across the contracts. Therefore, we also estimated a generalized ordered Logistic model (GOM), which allows to relax the assumption that the effects of independent variables on outcome levels are uniform across each level. We highlight the few instances when this additional specification complements the insights of the ordered Logistic model in the text. In general, the results of both models are practically the same. Results of the GOM are reported in the appendix (Table 3.A.3). In addition to the generalized Ordered Logistic model one could also estimate a multinomial logit model (MNL), which assumes that there is no intrinsic order in the menu of the electricity contracts at all (outcome categories are nominal). However, the estimation is less parsimonious and does not add any additional insights to our results. Therefore, we refrain from reporting this model.

Table 3.3: Ordered Logistic model | Active choice of a contract

	Demo-graphics (1)	Energy literacy (2)	Attitudes (3)	Full model (4)
Age	0.999			1.001
	(0.005)			(0.005)
Gender: Male	1.277**			1.029
	(0.114)			(0.123)
Nationality: Swiss	1.222			1.179
	(0.157)			(0.162)
Language: Native	0.711*			0.693*
	(0.192)			(0.197)
Single	1.110			0.805
	(0.137)			(0.164)
Single parent	1.053			1.029
	(0.375)			(0.386)
Has children	1.382**			1.460***
	(0.136)			(0.139)
Owns property	0.865			0.849
	(0.127)			(0.130)
Occupation, *Base: Full-time*				
Part-time	0.536***			0.743**
	(0.138)			(0.142)
Self-employed	0.554***			0.667*
	(0.216)			(0.222)
In training/ in school	0.943			1.045
	(0.337)			(0.345)
Seeking work	0.735			1.178
	(0.490)			(0.493)
House wife / house husband	1.054			1.340
	(0.342)			(0.347)
Retired	0.999			1.047
	(0.210)			(0.215)
Education, *Base: Compulsory schooling*				
Vocational training	0.645			0.508*
	(0.374)			(0.402)
A-levels	0.556			0.555
	(0.403)			(0.433)
Higher education	0.416**			0.375**
not university	(0.387)			(0.417)
University	0.376***			0.425**
	(0.380)			(0.411)
Income, Base: *below CHF4,000*				
CHF4,001-6,000	1.020			0.928
	(0.196)			(0.203)
CHF6,001-8,000	0.916			0.803
	(0.196)			(0.205)
CHF8,001-10,000	0.943			0.896
	(0.219)			(0.227)
CHF10,001-12,000	0.820			0.712
	(0.239)			(0.247)
CHF12,001-14,000	0.657			0.572**
	(0.264)			(0.272)
CHF14,001-16,000	0.824			0.570*
	(0.308)			(0.314)
above CHF16,000	0.841			0.593
	(0.308)			(0.321)
Zip code fixed effect	Yes			Yes

Table 3.3 continued

	Demo-graphics (1)	Energy literacy (2)	Attitudes (3)	Full model (4)
Energy literacy				
Knows consumption question		0.973		1.018
		(0.106)		(0.110)
Knows new renewable mix		0.815**		0.888
		(0.100)		(0.106)
Knows hydro mix		0.844		1.040
		(0.112)		(0.117)
Explains concept of green electricity easily		0.859**		1.058
		(0.068)		(0.080)
Political attitude			1.309***	1.312***
			(0.024)	(0.026)
Personal attitudes				
Nature importance			0.744***	0.739***
			(0.034)	(0.036)
Ease of decision			1.055**	1.063**
			(0.023)	(0.024)
Patience			0.988	0.982
			(0.019)	(0.020)
Trust			0.989	1.001
			(0.020)	(0.020)
Altruism			0.957**	0.994
			(0.021)	(0.023)
Decision taking, *Base: Alone*				
Together				0.563***
				(0.135)
Not aware of decision				1.042
				(0.256)
Constant GREEN++ | GREEN+	0.023***	0.034***	0.012***	0.004***
	(0.503)	(0.235)	(0.464)	(0.712)
Constant GREEN+ | DEFAULT	0.149***	0.213***	0.085***	0.028***
	(0.492)	(0.211)	(0.450)	(0.701)
Constant DEFAULT | GREY−	0.477	0.641**	0.303***	0.108***
	(0.490)	(0.206)	(0.448)	(0.698)
Constant GREY− | GREY−−	4.205***	5.152***	3.314***	1.300
	(0.491)	(0.215)	(0.448)	(0.695)
AIC	3889.74	3937.88	3645.12	3627.13
Observations	1,362	1,362	1,362	1,362

Note.— *p<0.1; **p<0.05; ***p<0.01

Ordered Logistic Model; the dependent variable is an ordered outcome category taking the most expensive contract GREEN++ as basis. The model estimates the cumulative probability of preferring GREEN++ versus all other greyer contracts, i.e., GREEN+, DEFAULT, GREY−, and GREY−−. Four models are shown to allow for the stepwise interpretation as in the preceding analyses. First, we analyze the influence of demographics only on preferences for an electricity contract. Next, we look into whether energy literacy can predict greener or greyer active choices. In model (3), we assess the impact of attitudes on electricity contract choice. Last, we assess the complete model. Note that predictors on reasons for choice are not suitable for an analysis of the active choice, because respondents were specifically asked for their reasons of choice for the current contract selection. Coefficients are reported as odds-ratios. Only fully completed questionnaires are considered. Standard errors are shown in parentheses.

Full model At last, we can assess the full model (4) of active preferences for electricity contracts.[20] Evidently, most of the aforementioned results still hold. Better education leads to a lower probability of preferring greyer contracts, while the higher household income, the higher the probability to show preferences for greener contracts, thus underlining the main result that these socio-demographic characteristics are valid predictors of active preferences for the choice of an electricity contract.

3.5.3 Who has a mismatch between default and actively preferred contract?

A further cornerstone of our analysis concerns the examination of mismatches between current and actively preferred contract. In this section we compare our participants' active choices to the currently held contracts. Combined with our data on demographics, energy literacy, political and personal attitudes, and reasons for choice, we can evaluate which groups in society have strong mismatches and what the reasons for these mismatches are.

Table 3.4 shows the cross-tabulation of current contract choice and actively preferred contract indicating whether people's preferences match with their currently selected contract. There are pronounced deviations between current and actually preferred choice, both towards greener and more expensive contracts, as well as towards greyer and cheaper contracts. Most importantly, households currently in DEFAULT have substantive preferences for other contracts (highlighted cells in Table 3.4). For instance, more than 40% of the respondents who currently hold the DEFAULT contract would actually prefer a cheaper contract, while on the other hand, more than a quarter would actually prefer a greener contract than DEFAULT.

In order to assess for which groups in our sample these different deviations between current and actively preferred contracts occur, we construct dependent variables taking the value zero if a household that currently holds the DEFAULT contract also actively prefers the DEFAULT contract and that take the value one if the active choice is a contract other than the DEFAULT.[21]

[20] Note that predictors on reasons for choice are not suitable for an analysis of the active choice, as the questionnaire specifically asked respondents to think back to their *current* electricity contract choice in order to state their reasons for choice.

[21] We construct the dependent variables in that way as we are mainly interested in the preference mis-

Table 3.4: Matches and mismatches between current and actively preferred contract

Current \ Active	GREEN++	GREEN+	DEFAULT	GREY−	GREY−−	Current total
GREEN++	16	1	2	0	0	19
GREEN+	18	78	20	9	1	126
DEFAULT	43	203	275	329	42	892
GREY−	1	9	34	107	9	160
GREY−−	2	7	18	55	83	165
Active Total	80	298	349	500	135	1,362

Note.— Table 3.4 shows the distribution of currently and actively preferred electricity contracts in relation. Grey colored cells highlight mismatches from currently holding the default contract but actually preferring GREEN++, GREEN+, GREY−, or GREY−−. Total observations are 1,362, which include all fully-completed questionnaires.

As in Section 3.5.1, we first estimate linear probability models (LPM) to analyze the influence of the variables collected in the questionnaire on the presence of matches and mismatches between current contract and actively preferred choice. We separate this analysis into an examination of mismatches for consumers with greener preferences (i.e., consumers who actively prefer a contract that is greener than the current one) and mismatches for consumers with greyer preferences (i.e., consumers who actively prefer a contract that is greyer than the current one). The results of these analyses are reported in Table 3.5 (mismatch for consumers with greener preferences) and Table 3.6 (mismatch for consumers with greyer preferences) and will subsequently be discussed.[22]

3.5.3.1 Mismatch for consumers with greener preferences

In this section we discuss the mismatches for consumers in the default with preferences for greener contracts than the default based on the regression results reported in Ta-

matches for households with a DEFAULT contract (45%, $n = 617$, of respondents). In addition, 14% ($n = 186$) of our respondents do *not* hold the DEFAULT contract and make an inconsistent active choice that does not match their currently held contract. As discussed in Section 3.4.2, the latter may be seen as random noise in our data as they are likely due to instable preferences or respondents making random active choices. We use these observations to evaluate the robustness of our results by running the analyses also on dependent variables that capture a mismatch in general and not only for DEFAULT contracts.

[22] By dichotomizing the dependent variable in 'Match' or 'Mismatch', important information on patterns in our findings could be lost. Therefore, we also use (generalized) ordered Logistic regressions to further analyze the potential distortions between currently choosing DEFAULT and actively preferring greener or greyer contracts. The dependent variable in these models is one if a household currently holding the DEFAULT contract actively prefers GREEN+ / or GREY−, it takes the value two if a household currently holding the DEFAULT contract actively prefers GREEN++ / or GREY−−, and it is zero otherwise. We report each model variant without and with accounting for the mismatches of inconsistent ('noisy') choosers who do not have DEFAULT as the current contract, thus evaluating the robustness of our results. The models are reported in the appendix.

ble 3.5. The dependent variable in these analyses is one if a household currently holding the DEFAULT contract actively prefers a contract that is *greener* than the DEFAULT (i.e., GREEN+ or GREEN++), and it is zero otherwise.

Demographics Model (1) in Table 3.5 concerns the role of demographics only for explaining the extent of mismatches from currently holding DEFAULT to actively preferring the greener contracts GREEN+ or GREEN++. There are only few findings that may explain a mismatch for consumers with greener preferences than DEFAULT. Having children reduces the probability of a mismatch. The analysis of the generalized ordered Logistic model in Table 3.A.7 in the appendix additionally reveals that speaking the local language may also reduce the probability of a mismatch.

Energy literacy Moving to model (2) in Table 3.5 concerning energy literacy, we find no significant impact of energy literacy on mismatches for consumers with greener preferences.

Attitudes In model (3) concerning the influence of attitudes on mismatches for consumers with greener preferences, we find evidence that the more politically left, the higher the probability for a mismatch. The same holds for the personal importance of nature conservation: the higher the importance attached to the environment, the more likely it is that a household actively prefers a contract that is greener than the one currently held. Respondents who indicate to be more patient also seem to have a higher probability of preferring greener contracts than they actually have.

Reasons for choice Model (4) examines the reasons why a potential mismatch could occur. The better informed households are, the lower the probability that they would prefer a greener contract to their current one. Additionally, the more respondents deem the electricity contract choice an unimportant decision, the lower the probability of such a mismatch. In contrast, perceiving the decision as complex adds to the probability of having a mismatch. Taking the decision together with a partner or flat-mates also adds to the the probability of a mismatch. The generalized ordered Logistic model reported in Table 3.A.7 in the appendix additionally shows that the more people perceive the default as an

Table 3.5: Linear probability model | Mismatch for consumers with greener preferences

	Demo-graphics (1)	Energy literacy (2)	Attitudes (3)	Reasons for choice (4)	Full model (5)
Age	−0.001				−0.000
	(0.001)				(0.002)
Gender: Male	−0.046*				−0.009
	(0.025)				(0.036)
Nationality: Swiss	−0.021				−0.003
	(0.034)				(0.049)
Language: Native	0.044				0.044
	(0.036)				(0.057)
Single	0.000				0.052*
	(0.029)				(0.032)
Single parent	0.028				0.033
	(0.074)				(0.101)
Has children	−0.090***				−0.097***
	(0.027)				(0.027)
Owns property	0.005				0.010
	(0.025)				(0.039)
Occupation, *Base: Full-time*					
Part-time	0.040				0.005
	(0.030)				(0.044)
Self-employed	0.068				0.029
	(0.048)				(0.068)
In training/ in school	0.042				0.021
	(0.075)				(0.098)
Seeking work	−0.069				−0.087
	(0.069)				(0.120)
House wife / house husband	0.082				0.067
	(0.076)				(0.096)
Retired	0.010				−0.003
	(0.046)				(0.061)
Education, *Base: Compulsory schooling*					
Vocational training	0.006				−0.001
	(0.066)				(0.078)
A-levels	0.031				0.023
	(0.074)				(0.091)
Higher education not university	0.061				0.048
	(0.069)				(0.085)
University	0.038				0.014
	(0.067)				(0.082)
Income, *Base: below CHF4,000*					
CHF4,001-6,000	0.022				0.030
	(0.041)				(0.056)
CHF6,001-8,000	−0.003				0.023
	(0.040)				(0.059)
CHF8,001-10,000	0.032				0.055
	(0.046)				(0.066)
CHF10,001-12,000	0.024				0.056
	(0.051)				(0.075)
CHF12,001-14,000	0.096				0.132**
	(0.059)				(0.057)
CHF14,001-16,000	0.074				0.122*
	(0.068)				(0.067)
above CHF16,000	0.032				0.078
	(0.063)				(0.097)
Zip code fixed effect	Yes				Yes

Table 3.5 continued

	Demo-graphics (1)	Energy literacy (2)	Attitudes (3)	Reasons for choice (4)	Full model (5)
Energy literacy					
Knows consumption question		−0.003			−0.008
		(0.023)			(0.022)
Knows new renewable mix		−0.022			−0.018
		(0.022)			(0.021)
Knows hydro mix		0.019			−0.004
		(0.024)			(0.024)
Explains concept of green electricity easily		0.005			0.006
		(0.014)			(0.016)
Political attitude			−0.027***		−0.025***
			(0.005)		(0.005)
Personal attitudes					
Nature importance			0.023***		0.023***
			(0.006)		(0.006)
Ease of decision			−0.004		−0.002
			(0.005)		(0.005)
Patience			0.007*		0.007**
			(0.004)		(0.004)
Trust			−0.004		−0.006
			(0.004)		(0.004)
Altruism			−0.001		−0.004
			(0.004)		(0.005)
Reasons for choice					
I informed myself before choosing				−0.029**	−0.031**
				(0.012)	(0.013)
It was a complex decision				0.021*	0.030**
				(0.012)	(0.013)
I did not know that I could choose				0.017	0.021
				(0.013)	(0.013)
It was an unimportant decision				−0.056***	−0.037***
				(0.013)	(0.013)
I forgot to decide				0.017	0.019
				(0.017)	(0.017)
The default appeared to be a recommendation				−0.002	−0.006
				(0.011)	(0.011)
I kept the effort low				−0.012	−0.006
				(0.011)	(0.011)
I actually never made a decision				0.005	−0.004
				(0.016)	(0.017)
I did not have enough info about the products				−0.011	−0.007
				(0.013)	(0.013)
Decision taking, *Base: Alone*					
Together				0.075***	0.082***
				(0.021)	(0.025)
Not aware of decision				−0.023	−0.012
				(0.048)	(0.050)

Table 3.5 continued

	Demo-graphics (1)	Energy literacy (2)	Attitudes (3)	Reasons for choice (4)	Full model (5)
Constant	0.176*	0.171***	0.133	0.266***	0.081
	(0.091)	(0.042)	(0.082)	(0.072)	(0.145)
R^2	0.035	0.001	0.047	0.032	0.100
Observations	1,362	1,362	1362	1,362	1,362

Note.— *p<0.1; **p<0.05; ***p<0.01
Linear Probability Model (OLS); the dependent variable takes the value 1 if a respondent who is currently in DEFAULT has a mismatch (=an active preference) to a greener electricity contract. Five models are shown in order to allow for the stepwise interpretability of different sets of coefficients. First, we analyze the influence of demographics only on mismatch. Next, we look into whether energy literacy can predict the mismatch to greener contracts. In model (3) we assess the impact of attitudes on mismatch to greener contracts. Last, we assess the impact of reasons for choice mismatches. The final column estimates the complete model. Only fully completed questionnaires are considered. Heteroskedasticity-robust standard errors are in parentheses.

recommendation, the higher the probability of a mismatch of greener preferences. A key message seems to be that better information and making people aware of the importance of the decision could significantly reduce mismatches of greener preferences, and lead people with such preferences to buy the more environmentally friendly electricity contracts they actually prefer.

Full model The full linear probability model (5) confirms most of the main findings of the stepwise analysis of predictors. The ordered Logistic model shown in Table 3.A.5 in the appendix additionally confirms the basic results, but adds further relevant insights. For instance, single households have a higher probability of a mismatch, and so do households in higher income categories. Thus, there seems to be a significant share of the population with distinct environmental preferences and sufficient disposable income who may need more information in order to attain a higher demand of greener electricity contracts. An intermediate green electricity default as implemented by the utility we analyzed may actually prohibit greener choices on behalf of these households.

3.5.3.2 Mismatch for consumers with greyer preferences

In this section we discuss the mismatches for consumers in the default with preferences for greyer contracts than the default, based on the regression results reported in Table 3.6. The dependent variable in these analyses is one if a household currently holding the DEFAULT contract actively prefers a contract that is *greyer* than the DEFAULT (i.e., GREY− or GREY−−), and it is zero otherwise.

Demographics Model (1) in Table 3.6 concerns the role of demographics only on the extent of mismatches from currently holding DEFAULT to actively preferring the greyer contracts GREY−, and GREY−−. There are several interesting findings. First, owning property reduces the probability of having a mismatch. Similarly, the higher the formal education, the less likely it is that someone prefers a greyer contract than DEFAULT. Given that formal education, property ownership and income are all highly negatively correlated with the dependent variable (Mismatch/Education $r = -0.127$, $p < .001$, Mismatch/Income $r = -0.079$, $p = .002$, Mismatch/Property $r = -0.126$, $p < .001$) and given that these variables are also highly positively correlated with each other (Education/Income $r = 0.282$, $p < .001$, Education/Property $r = 0.094$, $p < .001$, Income/Property $r = 0.255$, $p < .001$), it seems that especially poorer respondents with a lower socio-economic status (lower education and no property ownership) are prone to suffer preference mismatches in this direction. These respondents tend to actively prefer greyer and cheaper contracts than they actually have. The specifications of the ordered Logistic models reported in the appendix confirm these results.

Energy literacy Moving to the second model (2) in Table 3.6 concerning energy literacy, it seems that the better the energy literacy, the less likely are respondents who currently have the default contract to actually prefer a greyer contract. Knowing the share of new renewable electricity and being able to explain the difference between green and grey electricity leads to a 7% reduced probability each of having a mismatch in this direction, keeping all other variables constant.

Attitudes The effects for political attitude and the importance of nature reported in model (3) are exactly the opposite of what we have observed in the analysis on mismatches for consumers with greener preferences: a more right-leaning political attitude and a lower importance of nature lead to a higher likelihood to prefer a greyer contract than the currently held DEFAULT contract.

Reasons for choice Model (4) examines the reasons why a mismatch could occur. The better informed respondents are, the lower the probability of having a mismatch for

Table 3.6: Linear probability model | Mismatch for consumers with greyer preferences

	Demo-graphics (1)	Energy literacy (2)	Attitudes (3)	Reasons for choice (4)	Full model (5)
Age	−0.001				0.000
	(0.001)				(0.001)
Gender: Male	−0.020				−0.010
	(0.028)				(0.029)
Nationality: Swiss	0.046				0.050
	(0.037)				(0.037)
Language: Native	−0.067				−0.051
	(0.048)				(0.047)
Single	−0.033				−0.063
	(0.033)				(0.038)
Single parent	0.093				0.086
	(0.089)				(0.082)
Has children	0.037				0.038
	(0.032)				(0.032)
Owns property	−0.093***				−0.073**
	(0.030)				(0.030)
Occupation, *Base: Full-time*					
Part-time	−0.054				−0.002
	(0.033)				(0.032)
Self-employed	−0.028				−0.017
	(0.050)				(0.049)
In training/ in school	0.107				0.125
	(0.088)				(0.084)
Seeking work	−0.017				0.018
	(0.133)				(0.128)
House wife / house husband	−0.015				0.0002
	(0.088)				(0.079)
Retired	−0.021				−0.029
	(0.048)				(0.047)
Education, *Base: Compulsory schooling*					
Vocational training	−0.055				−0.028
	(0.093)				(0.098)
A-levels	−0.210**				−0.157
	(0.099)				(0.104)
Higher education	−0.184*				−0.133
not university	(0.095)				(0.101)
University	−0.174*				−0.096
	(0.094)				(0.100)
Income, *Base: below CHF4,000*					
CHF4,001-6,000	0.099*				0.085*
	(0.051)				(0.050)
CHF6,001-8,000	−0.016				−0.017
	(0.049)				(0.048)
CHF8,001-10,000	0.021				0.020
	(0.054)				(0.054)
CHF10,001-12,000	−0.007				0.003
	(0.058)				(0.057)
CHF12,001-14,000	−0.038				−0.029
	(0.064)				(0.064)
CHF14,001-16,000	−0.029				−0.032
	(0.069)				(0.069)
above CHF16,000	0.052				0.043
	(0.075)				(0.076)
Zip code fixed effect	Yes				Yes

Table 3.6 continued

	Demo-graphics (1)	Energy literacy (2)	Attitudes (3)	Reasons for choice (4)	Full model (5)
Energy literacy					
Knows consumption question		−0.011			−0.006
		(0.026)			(0.026)
Knows new renewable mix		−0.069***			−0.045*
		(0.024)			(0.025)
Knows hydro mix		−0.019			0.004
		(0.027)			(0.027)
Explains concept of green electricity easily		−0.072***			−0.020
		(0.017)			(0.020)
Political attitude			0.019***		0.020***
			(0.006)		(0.006)
Personal attitudes					
Nature importance			−0.023***		−0.010
			(0.008)		(0.009)
Ease of decision			−0.008		0.003
			(0.006)		(0.006)
Patience			0.006		0.003
			(0.004)		(0.004)
Trust			0.003		0.001
			(0.005)		(0.005)
Altruism			−0.006		0.005
			(0.005)		(0.006)
Reasons for choice					
I informed myself before choosing				−0.064***	−0.054***
				(0.016)	(0.017)
It was a complex decision				0.022	0.003
				(0.014)	(0.015)
I did not know that I could choose				−0.012	−0.017
				(0.014)	(0.015)
It was an unimportant decision				0.056***	0.043***
				(0.015)	(0.016)
I forgot to decide				0.003	0.009
				(0.020)	(0.019)
The default appeared to be a recommendation				0.024**	0.028**
				(0.011)	(0.012)
I kept the effort low				−0.002	−0.006
				(0.013)	(0.013)
I actually never made a decision				0.009	0.003
				(0.019)	(0.019)
I did not have enough info about the products				0.004	−0.001
				(0.016)	(0.016)
Decision taking, *Base: Alone*					
Together				−0.052**	−0.060*
				(0.024)	(0.032)
Not aware of decision				0.164**	0.128*
				(0.067)	(0.068)

Table 3.6 continued

	Demo-graphics (1)	Energy literacy (2)	Attitudes (3)	Reasons for choice (4)	Full model (5)
Constant	0.524***	0.521***	0.427***	0.263***	0.367*
	(0.119)	(0.051)	(0.110)	(0.087)	(0.201)
R^2	0.065	0.025	0.028	0.082	0.118
Observations	1,362	1,362	1,362	1,362	1,362

Note.— *p<0.1; **p<0.05; ***p<0.01
Linear Probability Model (OLS); the dependent variable takes the value 1 if a respondent who is currently in DEFAULT has a mismatch (=an active preference) to a greyer electricity contract. Five models are shown in order to allow for the stepwise interpretability of different sets of coefficients. First, we analyze the influence of demographics only on mismatch. Next, we look into whether energy literacy can predict the mismatch to greyer contracts. In model (3) we assess the impact of attitudes on mismatch to greyer contracts. Last, we assess the impact of reasons for choice mismatches. The final column estimates the complete model. Only fully completed questionnaires are considered. Heteroskedasticity-robust standard errors are in parentheses.

consumer with greyer preferences. In contrast to what has been observed with regard to mismatches for consumers with greener preferences, the more unimportant respondents deem the decision, the higher is the probability of having a mismatch. Moreover, the more respondents indicate that the default appeared to be a recommendation, the higher is the probability of a mismatch. The same finding holds for people who were unaware that they could make a decision at all. Taking the decision together reduces the probability of having a mismatch for consumers with greyer preferences. Not being aware of the decision increases the probability of having a mismatch.

Full model The full linear probability model (5) partly confirms the findings of the stepwise analysis of predictors. Apparently, the significant effects of education seem to be masked by other variables in the linear probability models.[23] However, the ordered Logistic models shown in the appendix confirm that higher education is a significant predictor of fewer mismatches for consumers with greyer preferences. These results indicate that 'low-status' households, i.e., groups within the society that have lower income and lower formal education, are more prone to experience preference mismatches because of the DEFAULT, as they actually tend to prefer the cheaper contracts of the choice set. Thus, setting a green electricity default may severely restrict the utility of these social groups.

[23] As explained above, education, property ownership, and income are positively correlated, which could lead to multicollinearity issues in estimating the full model.

In total, our findings show that the green electricity default in this setting seems to have utility-decreasing effects in two directions. First, pro-environmentally minded, but ill-informed households do not choose the greener contracts which they prefer and second, poorer households do not choose the less expensive contracts they prefer.

3.5.4 Cost-benefit assessment of green electricity defaults

As described in Section 3.2, recent research in behavioral economics has started to look into the welfare effects of nudges. We contribute to these efforts and provide a discussion of the potential costs and benefits induced by the green electricity default for our specific field setting.

Our cost-benefit framework rests on the assumption that in a situation without a choice default, consumers fully optimize and make choices consistent with their preferences. Thus, comparing decisions of the active choice with the currently held contracts provides an appropriate basis for an approximation of the change in consumer welfare due to the choice default. This approach has been used before to derive 'sufficient statistics' for welfare evaluations of nudges (see, for instance, Allcott and Kessler, 2015; Chetty, 2008).[24] The method relies on the two central premises of a choice-oriented welfare framework described by Bernheim (2016, p. 33, italics in original): "*Premise A:* [...] *each of us is the best arbiter of our own well-being*; *Premise B:* [...] *we seek to benefit ourselves by selecting the alternative that* [...] *is most conducive to our well-being.*"

As asserted in Section 3.5.3, we observe two directions of mismatches induced by the green electricity default in our setting, which are relevant for consumers' welfare. There are mismatches for consumers with greener preferences than the default and there are mismatches for consumers with greyer preferences than the default. Both of these directions of mismatches come with different costs and benefits for individuals and for society.

[24] Chetty (2008) describes 'sufficient statistics' as a methodological approach in-between the estimation of structural models for the analysis of the welfare effects of policies and a simple program evaluation based on treatment level effects. Specifically, the concept of sufficient statistics aims at identifying a credible baseline for welfare evaluations, i.e., in our case the active choice, which then allows to make predictions on the welfare effects of policies.

3.5.4.1 Evaluation of costs and benefits at the individual level

We use the per unit price differences between the five electricity contracts available in our setting to quantify whether individuals incur a monetary loss or benefit from staying with the choice default. Contracts that are greyer than the default are cheaper and contracts that are greener than the default are more expensive. Based on these price differences and the annual electricity consumption data for each household—data we received from our partnering utility—we can calculate the annual monetary gains or losses of individual households. We extrapolate our estimation of costs and benefits for the total population of the utility's consumers.

Column (1) in Table 3.7 refers to different directions of mismatches that can occur at the individual level. The two top rows indicate mismatches for individuals with greener preferences than the default, the two bottom rows indicate mismatches for individuals with greyer preferences than the default.

Column (2) in Table 3.7 shows the monetary benefits and costs for households who currently hold the DEFAULT contract, but whose active choice indicates that they would prefer a greener or a greyer contract. Individuals who actively prefer a greener contract than the default (i.e., GREEN++ or GREEN+) but stay with DEFAULT save money. The per unit prices of electricity for the default are cheaper than for greener contracts, thus by not opting actively for a greener contract, households pay less for their electricity consumption (see rows 1 and 2 in Table 3.7). The economic interpretation is that an intermediate green electricity default prohibits skimming positive willingness-to-pay for green electricity, thus yielding negative utility for these individuals (as they would prefer paying more for a greener contract and receiving greener electricity in return). In fact, in the total population of households we sampled from, the default leaves approximately USD 1.4m of additional willingness-to-pay for green electricity untapped.

Individuals who actively prefer a greyer contract than the default (i.e., GREY−− or GREY−) but stay with the DEFAULT spend more money on an electricity contract. The per unit prices of electricity for the default are more expensive than for greyer contracts. Thus by not opting actively for a greyer contract, households pay more for their electricity consumption (see rows 3 and 4 in Table 3.7). In total, individuals annually loose roughly

Table 3.7: Simple cost-benefit evaluation of the green electricity default

Mismatch to default contract (1)	Individual level effects [USD] monetary benefits (+) | costs (−) (2)	GHG-emissions [t/CO_{2eq}] caused (+) | avoided (−) (3)
GREEN++	502, 442.00	3.26
GREEN+	942, 876.00	6.40
GREY−	−336, 665.00	−155.08
GREY−−	−60, 882.00	−3, 891.88

Note.— The table shows a simple cost-benefit evaluation of the green electricity default used in this study. Estimates are linearly extrapolated for the total population of the study, i.e., approximately 50,000 households. We use the 2016 annual electricity consumption data for each household from our partnering utility to calculate individual level benefits and costs. Direct GHG-emissions are calculated with official statistics on the greenhouse gas potential of different production sources in the Swiss market (Messmer and Frischknecht, 2016, p. 16). Each contract thus has a different emission potential based on its composition. For GREY−− we could not obtain the exact composition of electricity sources as there is only a legal obligation to label renewable sources of an electricity mix. Therefore, we used conservative estimates of direct GHG-emissions attaching equal weights to electricity produced from lignite and hard coal, although the share of nuclear power in the Swiss electricity mix is much higher. Thus, the emissions avoided by not choosing GREY−− should be seen as an upper bound. At the time of the study CHF 1 ≈ USD 1, therefore we directly report USD.

USD 400,000 from not optimizing away from the default to cheaper contracts. This implies a negative effect with respect to consumers' utility.

Note that this quantification of costs does ot take into account potential countervailing individual benefits of not needing to decide for a contract, i.e., avoiding the decision costs of choosing. If individuals act fully rationally, the costs incurred by not optimizing their choices should completely reflect the benefits of not having to choose. Given that the measurement of the individual cognitive costs of switching is not straightforward, we must remain conservative on the definite effects of the default at the individual level.

3.5.4.2 Evaluation of costs and benefits at the society's level

At the societal level, we can assess the implications of mismatches through the mismatch-related differences in greenhouse gas emissions. We use official statistics on the greenhouse gas potential of different electricity production sources in the Swiss market in order to quantify the per unit emissions of the electricity contracts in our setting.[25] Using the

[25] Direct GHG-emissions are calculated with official statistics on the greenhouse gas potential of different production sources in the Swiss market (Messmer and Frischknecht, 2016). Each contract has a different emission potential based on its composition of electricity sources used for production. For GREY−−

annual electricity consumption data for each household from our partnering utility, we can quantify the level of greenhouse gas emissions caused or avoided when consumers stay with the choice default instead of switching to their preferred contract. We extrapolate our estimation for the total population of the utility's consumers.

Column (3) in Table 3.7 shows the greenhouse gas emissions caused or avoided for consumers with a mismatch when currently holding DEFAULT but preferring a greener or greyer contract. Consumers who actively prefer a greener contract than the default (i.e., GREEN++ or GREEN+) but stay with DEFAULT cause slightly more greenhouse gas emissions. The carbon intensity of the default is, however, only marginally higher than those of the two greener contracts, i.e., the default electricity contract in this setting is already environmentally-friendly in terms of greenhouse gas emissions. Thus, at the societal level only roughly 10 additional tons of CO_{2eq} are caused by people not choosing a contract according to their preferences (see rows 1 and 2 in Table 3.7).[26]

In contrast, consumers who actively prefer a greyer contract than the default (i.e., GREY−− or GREY−) but stay with DEFAULT avoid a substantial amount of greenhouse emissions. The default contract is much less carbon intensive than the two greyer contracts. Thus, in total, approximately 4,000 tons of CO_{2eq} are not emitted by people not choosing a contract according to their preferences (see rows 3 and 4 in Table 3.7).

Having quantified the costs and benefits of the green electricity default in this setting, we can indicate its overall effectiveness in curbing harmful emissions by comparing columns (2) and (3) in Table 3.7. With our specific default in place, roughly 4,000 tons of emissions (the sum of estimates in column (3)) are avoided annually. If we only use the pure monetary costs of individuals (i.e., the costs for those individuals who prefer a cheaper contract but stay with the default), individuals pay roughly USD 400,000 for the avoidance of these

we could not obtain the exact composition of electricity sources as there is only a legal obligation to label renewable sources of an electricity mix. Therefore, we used conservative estimates of direct GHG-emissions attaching equal weights to electricity produced from lignite and hard coal, although the share of nuclear power in the Swiss electricity mix is much higher. Thus, the quantified emissions of GREY−− should be seen as an upper bound.

[26] It is, however, important to note that the greener contracts provide different incentives for renewable technology adoption in the market; hence although GREEN++, for instance, is only marginally more efficient in curbing GHG-emissions, a strengthened demand for this contract provides clear investment signals for new renewable technology, such as solar PV, in the market. Such a broader adoption of new renewable technology seems to be important, as more and more countries agree to phase out older conventional power plants.

4,000 tons of emissions (approx. USD 100 per ton of CO_{2eq}). If we also include the negative effects of the default on the utility of those individuals who prefer a greener contract than the default, emission avoidance is even more costly at the societal level (approx. USD 460 per ton of CO_{2eq}). Comparing these estimates with recent estimates of the social cost of carbon in the range of USD 30 to USD 200 (e.g., Howard and Sterner, 2017; Nordhaus, 2017), emission avoidance due to the default in this setting seems to be rather costly.[27]

In sum, our back-of-the-envelope calculations concerning the impact of the green electricity default in our setting on individuals' and societal welfare reveal two important results: (1) the choice default negatively affects optimization on behalf of approximately 70% of consumers who currently hold the default contract, but do not opt out to their actively preferred choice. Individuals experiencing a mismatch between active choice and current contract either lose money because they do not switch to greyer and cheaper contracts or they experience a negative effect on their utility, because their actual willingness-to-pay for green electricity is not sufficiently exploited, as they do not switch to greener contracts; (2) the choice default overall successfully curbs direct GHG-emissions from electricity consumption, however, this seems to be rather costly when compared to some recent estimates of the social cost of carbon.

3.6 Policy implications and conclusions

The results from our study provide a number of insights on the costs and benefits of choice defaults. These findings are relevant for policy makers who plan to apply choice defaults to curb greenhouse gas emissions. In general, green choice defaults in residential electricity markets follow two intentions. First, these interventions shall nudge people into contracts that they prefer. Second, choice defaults shall help to curb greenhouse gas emissions. These two policy goals may complement each other, but may also generate trade-offs in terms of policy outcomes.

We see that, first, the choice default in our setting often does not nudge people into contracts they prefer. There are mismatches between currently held and actively preferred

[27] Social cost of carbon (SCC) refers to the societal costs of emitting an additional ton of CO_2. Note that the literature on different estimates of SCC is diverse. There are different methodologies on how to assess the societal damage from climate change, which are controversially discussed. Nordhaus (2017) recently provided a SCC of as low as USD 31.20 per ton of CO_2. Howard and Sterner (2017) use different damage functions, which produce estimates three to four times above this value. Given the range of estimates, policy makers need to allow for sufficient sensitivity of the presented results.

contracts that lead to substantial losses in individual utility. Reducing the costs of opting out of the default could help aligning consumer preferences with their actual choices and lower the extent of mismatches (see also Chesterley, 2017).[28] When implementing a default, choice architects should therefore consider how they can optimize individual decision making. An option is to lower switching costs in order to encourage consumers for whom the default is not the preferred choice to opt-out.

Another option is to provide targeted informational campaigns for households who do not choose according to their preferences. Specifically, we see that there seems to be a significant share of respondents in our sampled population with distinct environmental preferences and sufficient disposable income who forgo to switch to an even more environmentally friendly contract. Much of this mismatch can be explained by informational problems and the complexity of choice. Informational campaigns may help such households to choose their preferred contracts.

A third option is to reduce the monetary losses for households who stay with the default and do not choose according to their preferences. Poorer households with lower formal education are more prone to stay in the default, although they indicate having preferences for cheaper contracts in the choice set. To avoid this type of mismatch, choice architects could consider subsidies to alleviate their losses from not switching away from a default. According to our results all of these three options could help to close the wedge between active preferences and the choice under a default regime.

Moving to the second intention of the default, i.e., curbing greenhouse gas emissions, our cost-benefit analysis reveals that indeed the default successfully avoids harmful greenhouse gas emissions. However, the avoidance seems to come at individual costs that are somewhat higher than some recent estimates of the social cost of carbon. Therefore, the default applied in our setting seems to be only a second-best instrument for curbing emissions. A tax on the social marginal damage of electricity consumption could be a more efficient instrument for curbing emissions. Taxes, however, are often politically more difficult to implement than less salient behavioral interventions such as choice defaults.

[28] Chesterley's (2017) model illustrates that it is optimal for a consumer to stick with the default as long as the cost of making an active decision and switching away from the default is greater than the loss from the preference mismatch that occurs when the consumer stays in the default.

In total, we need to acknowledge that if green choice defaults are used as a means of environmental policy (e.g., instead of taxes), policy makers need to carefully consider the welfare impacts of this nudge. There are apparent trade-offs between the costs imposed on individuals and the benefits for the environment that accrue at the societal level.

Several open questions remain. First, a number of authors have advocated 'big data' approaches for designing smart (or personalized) defaults that intelligently adapt to individuals' preferences (e.g., Allcott and Kessler, 2015; Sunstein, 2012). Our data, however, suggest that choice defaults that try to correspond to individual preferences may only be of limited use as long as the data available for tailoring the personalized default to individuals' preferences are limited to demographics. According to our results, socio-demographics alone provide only limited predictive power for the assessment of preferences for different electricity contracts. Therefore, more research, possibly at the interdisciplinary level of data science, law and public policy, is needed to find out how to design and legally embed such personalized choice defaults in light of heterogeneous consumer preferences.

Secondly, future research should examine when and under which market conditions choice defaults fare better or worse than traditional instruments from a regulatory point of view. It may be also interesting to study whether and how lobby or industry efforts could trigger the decision to use nudges as a policy tool and how these efforts may shape the design of choice defaults (Schnellenbach and Schubert, 2015; Sugden, 2013).

A final point to address are the potential implications of the design of the choice default. In theory, a green electricity default should only 'affect' those households who prefer this default anyway, while incentivizing choice among households who have preferences for other contracts. Ideally, the choice architect should aim at balancing the individual losses from not switching away from a green choice default with the potential benefits of the nudge for the society.

However, given that we can only observe the effects of one particular default in our setting, it is possible that the impact on individuals' and society's costs and benefits change with the position of the default. Chesterley (2017) calls this the 'composition effect' of a choice default, i.e., the losses for consumers not switching away and emissions caused by staying with the default may considerably change if the choice architect switches

defaults from, for instance, DEFAULT to GREEN+. Therefore, future work should look into the importance of these composition effects. In this respect, we encourage more tightly controlled lab-in-the-field research that complements the insights of our field study. Careful cost-benefit analysis of such changes in the decision architecture may generate important insights for choice architects in residential electricity markets and beyond.

Appendix

3.A Appendix

3.A.1 Inconsistent choices

In order to check whether the individually stated preferences for an electricity contract are subject to random-answer bias, we can assess whether customers who have opted out of and hold a current contract other than the default also make active choices in our questionnaire that are consistent with their current contract. If random choice poses a problem, we would observe consistent choices only for 20% of the cases (there a five contracts on offer in the active choice.) Table 3.A.1 shows that random choice does not pose a problem for study. On average, 60% of respondents make consistent choices. The level of consistent choices differs between the varying contracts, it seems that customers who currently hold a greyer contract more often deviate in their hypothetical active choice towards greener contracts, which may be a sign of a social-desirability bias.

Table 3.A.1: Consistent and inconsistent choices between current and actively preferred contract

Current \ Active	GREEN++	GREEN+	DEFAULT	GREY−	GREY−−	Share of consistent choices
GREEN++	16	1	2	0	0	84%
GREEN+	18	78	20	9	1	62%
GREY−	1	9	34	107	9	67%
GREY−−	2	7	18	55	83	50%

Note.— Table 3.A.1 shows the consistent and inconsistent choices between current and actively preferred contract for consumers who have actively opted out of the default. Grey highlighted cells indicate consistent choices. The last column to the right provides the percentage share of consistent choices for each contract.

3.A.2 Characteristics of the final sample

Table 3.A.2: Characteristics of the final sample

	Final Sample	Population
Demographics		
Age, mean	48.01 (16.90)	39.60
Gender: Male	62.63%	49.05%
Nationality: Swiss	86.42%	75.88%
Native Speaker	91.12%	–
Single household	28.27%	–
Single parent	2.35%	–
Has children	22.93%	–
Owns property	35.54%	38.40%(*)
Occupation		
Full-time	41.48%	49.90%(*)
Part-time	23.42%	37.80%(*)
Self-employed	6.98%	12.30%(*)
In training/ in school	2.86%	–
Seeking work	1.10%	4.70%(*)
House wife / house husband	2.42%	–
Retired	21.73%	–
Education		
Compulsory schooling	2.13%	–
Vocational training	28.27%	35.70% (*)
A-levels	9.47%	20.70%(*)
Higher education not university	17.91%	14.40%(*)
University	42.22%	29.30%(*)
Income		
below CHF4,000	11.16%	13.10%(*)
CHF4,001-6,000	18.36%	16.30%(*)
CHF6,001-8,000	24.08%	17.20%(*)
CHF8,001-10,000	16.59%	15.70%(*)
CHF10,001-12,000	12.33%	12.10%(*)
CHF12,001-14,000	7.56%	8.80%(*)
CHF14,001-16,000	4.99%	5.80%(*)
above CHF16,000	4.92%	11.10%(*)
Energy literacy		
Knows consumption question	30.69%	–
Knows new renewable mix	43.76%	–
Knows hydro power mix	25.26%	–
Attitudes		
Political attitude	4.38 (2.34)	5.00(*)
on scale from 0 (left) to 10 (right)		
Personal attitudes		
on a scale from 0 to 10 (see table notes for specifics)		
Nature importance	8.15 (1.63)	–
Ease of decision	7.19 (2.22)	–
Patience	6.47 (2.70)	–
Trust	5.10 (2.62)	–
Altruism	7.44 (2.63)	–

Table 3.A.2 continued

	Final Sample	Population
Reasons for choice		
on a scale from 1 (does not apply) to 4 (does apply)		
I informed myself before choosing	2.67 (0.99)	–
It was a complex decision	2.01 (0.88)	–
I did not know that I could choose	1.52 (1.00)	–
It was an unimportant decision	1.65 (0.88)	–
I forgot to decide	1.36 (0.82)	–
The default appeared to be a recommendation	2.52 (1.04)	–
I kept the effort low	2.74 (0.98)	–
I actually never made a decision	1.61 (0.99)	–
I did not have enough info about the products	1.65 (0.91)	–
Decision was taken		
Alone	41.78%	–
Together with partner	53.23%	–
Not aware of decision	4.99%	–
Sample Size	1,362	

Note.— Table 3.A.2 shows summary statistics for the four sets of explanatory variables. Standard deviations are in parentheses where applicable. Population estimates are indicated to display the degree of representativeness of our sample. Population estimates are taken from official population statistics of the Swiss city we have sampled in, or if unavailable for the specific city, we have used data at the national level (indicated by an asterisk *). Please note that the sources of the official population statistics can be obtained on request from the authors, as publication would lead to the identification of the partnering utility and would thus violate confidentiality agreements. The specific question text for personal attitudes is available in the printed copy of the questionnaire in the appendix. The higher the scores, the more importance is attached towards the given notion. Note that the score on altruism is reverse-coded in order to reflect that higher values mean that the respondent attaches a higher importance towards altruism.

3.A.3 Additional econometric specifications

In addition to the main analysis in the text, further models of the analysis are presented in this appendix.

For the analysis of active choices presented in Section 3.5.2, we add a generalized ordered Logistic model (Table 3.A.3).

For the analysis of mismatches for consumers with greener preferences presented in Section 3.5.3.1, we add a linear probability model including the inconsistent choices of households not in DEFAULT (Table 3.A.4). Further, we estimate the mismatches for consumers with greener preferences with ordered Logistic and generalized ordered Logistic models, both for the sample with and without inconsistent choices (Tables 3.A.5, 3.A.6, 3.A.7, 3.A.8.)

For the analysis of mismatches of greyer preferences presented in Section 3.5.3.2, we add a linear probability model including the inconsistent choices of households not in DEFAULT (Table 3.A.9). Further, we estimate the mismatches for consumers with greyer preferences with ordered Logistic and generalized ordered Logistic models, both for the sample with and without inconsistent choices (Tables 3.A.10, 3.A.11, 3.A.12, 3.A.13).

Table 3.A.3: Generalized ordered Logistic model | Active choice of a contract

	Demo-graphics (1)	Energy literacy (2)	Attitudes (3)	Full model (4)
Age	0.999			1.000
	(0.005)			(0.008)
Gender: Male	1.064			0.991
	(0.056)			(0.059)
Nationality: Swiss	1.209			1.255
	(0.155)			(0.233)
Language: Native	0.937			0.879
	(0.097)			(0.098)
Single	1.103			0.739
	(0.134)			(0.236)
Single parent	1.067			1.082
	(0.363)			(0.531)
Has children	1.375**			1.669**
	(0.138)			(0.221)
Owns property	0.863			0.800
	(0.126)			(0.189)
Occupation, *Base: Full-time*				
Part-time	0.547***			0.688*
	(0.146)			(0.207)
Self-employed	0.563***			0.586
	(0.223)			(0.336)
In training/ in school	0.946			1.093
	(0.327)			(0.476)
Seeking work	0.770			1.488
	(0.493)			(0.707)
House wife / house husband	1.038			1.576
	(0.329)			(0.496)
Retired	1.013			1.128
	(0.206)			(0.304)
Education, *Base: Compulsory schooling*				
Vocational training	0.654			0.381
	(0.374)			(0.606)
A-levels	0.567			0.425
	(0.403)			(0.642)
Higher education	0.422**			0.250**
not university	(0.394)			(0.661)
University	0.383**			0.309*
	(0.388)			(0.632)
Income, *Base: below CHF4,000*				
CHF4,001-6,000	1.014			0.926
	(0.191)			(0.281)
CHF6,001-8,000	0.924			0.734
	(0.192)			(0.297)
CHF8,001-10,000	0.948			0.858
	(0.215)			(0.318)
CHF10,001-12,000	0.839			0.631
	(0.236)			(0.357)
CHF12,001-14,000	0.658			0.463*
	(0.263)			(0.410)
CHF14,001-16,000	0.837			0.465*
	(0.303)			(0.462)
above CHF16,000	0.844			0.455
	(0.304)			(0.489)
Zip code fixed effect	Yes			Yes

Table 3.A.3 continued

	Demo-graphics (1)	Energy literacy (2)	Attitudes (3)	Full model (4)
Energy literacy				
Knows consumption question		0.946		1.014
		(0.139)		(0.154)
Knows new renewable mix		0.754**		0.835
		(0.135)		(0.152)
Knows hydro mix		0.823		1.055
		(0.147)		(0.164)
Explains concept of green electricity easily		1.097***		1.013
		(0.033)		(0.038)
Political attitude			1.481***	1.455***
			(0.072)	(0.087)
Personal attitudes				
Nature importance			1.043**	1.027
			(0.018)	(0.019)
Ease of decision			1.084**	1.022*
			(0.036)	(0.013)
Patience			0.985	0.977
			(0.028)	(0.028)
Trust			0.984	1.001
			(0.029)	(0.029)
Altruism			0.940*	0.989
			(0.032)	(0.032)
Decision taking, Base: Alone				
Together				0.466***
				(0.245)
Not aware of decision				1.109
				(0.358)
Constant GREEN++ | GREEN+	0.024***	0.011***	0.001***	0.0003***
	(0.606)	(0.543)	(1.350)	(1.973)
Constant GREEN+ | DEFAULT	0.153***	0.126***	0.025***	0.006***
	(0.518)	(0.338)	(0.900)	(1.436)
Constant DEFAULT | GREY−	0.477	0.539**	0.167**	0.040***
	(0.490)	(0.267)	(0.711)	(1.171)
Constant GREY− | GREY−−	4.067***	8.213***	5.248**	1.271
	(0.504)	(0.330)	(0.680)	(0.975)
AIC	3892.11	3932.03	3641.71	3629.46
Observations	1,362	1,362	1,362	1,362

Note.— *p<0.1; **p<0.05; ***p<0.01

Generalized Ordered Logistic Model; the dependent variable is an ordered outcome category taking the most expensive contract GREEN++ as basis. The model estimates the cumulative probability of preferring GREEN++ versus all other greyer contracts, i.e., GREEN+, DEFAULT, GREY−, and GREY−−. The model relaxes the proportional odds assumption. Four models are shown to allow for the stepwise interpretation as in the preceding analyses. First, we analyze the influence of demographics only on preferences for an electricity contract. Next, we look into whether energy literacy can predict greener or greyer active choices. In model (3), we assess the impact of attitudes on electricity contract choice. Last, we assess the complete model. Note that predictors on reasons for choice are not suitable for an analysis of the active choice, because respondents were specifically asked for their reasons of choice for the current contract selection. Coefficients are reported as odds-ratios. Only fully completed questionnaires are considered. Standard errors are shown in parentheses.

Table 3.A.4: Linear probability model | Mismatch for consumers with greener preferences, including inconsistent choices

	Demo-graphics (1)	Energy literacy (2)	Attitudes (3)	Reasons for choice (4)	Full model (5)
Age	0.000				0.001
	(0.001)				(0.001)
Gender: Male	−0.017				0.016
	(0.028)				(0.030)
Nationality: Swiss	−0.051				−0.027
	(0.040)				(0.039)
Language: Native	0.055				0.052
	(0.044)				(0.044)
Single	−0.029				0.012
	(0.033)				(0.039)
Single parent	0.081				0.083
	(0.089)				(0.086)
Has children	−0.086***				−0.094***
	(0.033)				(0.032)
Owns property	0.019				0.017
	(0.031)				(0.031)
Occupation, *Base: Full-time*					
Part-time	0.043				0.005
	(0.034)				(0.035)
Self-employed	0.106*				0.078
	(0.055)				(0.056)
In training/ in school	0.050				0.022
	(0.084)				(0.078)
Seeking work	−0.036				−0.064
	(0.106)				(0.109)
House wife / house husband	0.083				0.083
	(0.088)				(0.080)
Retired	0.008				−0.001
	(0.052)				(0.052)
Education, Base: *Compulsory schooling*					
Vocational training	−0.033				−0.043
	(0.089)				(0.089)
A-levels	0.021				0.013
	(0.097)				(0.097)
Higher education not university	0.035				0.021
	(0.092)				(0.093)
University	0.004				−0.018
	(0.090)				(0.091)
Income, Base: *below CHF4,000*					
CHF4,001-6,000	0.006				0.014
	(0.046)				(0.046)
CHF6,001-8,000	0.044				0.067
	(0.047)				(0.047)
CHF8,001-10,000	0.031				0.050
	(0.053)				(0.053)
CHF10,001-12,000	0.006				0.036
	(0.058)				(0.059)
CHF12,001-14,000	0.107				0.142**
	(0.067)				(0.066)
CHF14,001-16,000	0.143*				0.187**
	(0.079)				(0.078)
above CHF16,000	0.031				0.066
	(0.075)				(0.078)
Zip code fixed effect	Yes				Yes

Table 3.A.4 continued

	Demo-graphics (1)	Energy literacy (2)	Attitudes (3)	Reasons for choice (4)	Full model (5)
Energy literacy					
Knows consumption question		0.003			0.001
		(0.027)			(0.026)
Knows new renewable mix		0.026			0.024
		(0.025)			(0.025)
Knows hydro mix		−0.0002			−0.025
		(0.028)			(0.028)
Explains concept of green electricity easily		0.006			−0.011
		(0.017)			(0.019)
Political attitude			−0.027***		−0.027***
			(0.006)		(0.006)
Personal attitudes					
Nature importance			0.026***		0.024***
			(0.007)		(0.008)
Ease of decision			0.005		0.004
			(0.005)		(0.006)
Patience			0.005		0.007
			(0.004)		(0.005)
Trust			−0.005		−0.005
			(0.005)		(0.005)
Altruism			−0.005		−0.008
			(0.005)		(0.005)
Reasons for choice					
I informed myself before choosing				−0.010	−0.018
				(0.016)	(0.017)
It was a complex decision				0.009	0.021
				(0.014)	(0.016)
I did not know that I could choose				0.030**	0.037**
				(0.015)	(0.015)
It was an unimportant decision				−0.059***	−0.044***
				(0.015)	(0.015)
I forgot to decide				0.005	0.004
				(0.018)	(0.018)
The default appeared to be a recommendation				−0.012	−0.018
				(0.012)	(0.013)
I kept the effort low				−0.001	0.003
				(0.013)	(0.014)
I actually never made a decision				−0.014	−0.015
				(0.018)	(0.018)
I did not have enough info about the products				0.016	0.020
				(0.017)	(0.017)
Decision taking, *Base: Alone*					
Together				0.079***	0.069**
				(0.025)	(0.032)
Not aware of decision				−0.047	−0.061
				(0.055)	(0.057)

Table 3.A.4 continued

	Demo-graphics (1)	Energy literacy (2)	Attitudes (3)	Reasons for choice (4)	Full model (5)
Constant	0.239**	0.256***	0.182*	0.326***	0.164
	(0.116)	(0.050)	(0.099)	(0.086)	(0.185)
R^2	0.029	0.001	0.034	0.026	0.081
Observations	1,362	1,362	1362	1,362	1,362

Note.— *p<0.1; **p<0.05; ***p<0.01

Linear Probability Model (OLS); the dependent variable takes the value 1 if a respondent who is currently in DEFAULT has a mismatch (=an active preference) to a greener electricity contract. Additionally, the dependent variable also includes those households who have a mismatch to a greener contract, but who are not in DEFAULT. Five models are shown in order to allow for the stepwise interpretability of different sets of coefficients. First, we analyze the influence of demographics only on mismatch. Next, we look into whether energy literacy can predict the mismatch to greener contracts. In model (3) we assess the impact of attitudes on mismatch to greener contracts. Last, we assess the impact of reasons for choice mismatches. The final column estimates the complete model. Only fully completed questionnaires are considered. Heteroskedasticity-robust standard errors are in parentheses.

Table 3.A.5: Ordered Logistic model | Mismatch for consumers with greener preferences

	Demo-graphics (1)	Energy literacy (2)	Attitudes (3)	Reasons for choice (4)	Full model (5)
Age	0.994				1.000
	(0.007)				(0.008)
Gender: Male	0.737*				0.983
	(0.161)				(0.181)
Nationality: Swiss	0.874				0.997
	(0.223)				(0.232)
Language: Native	1.262				1.301
	(0.305)				(0.314)
Single	1.030				1.796**
	(0.193)				(0.251)
Single parent	1.264				1.320
	(0.551)				(0.577)
Has children	0.501***				0.436***
	(0.214)				(0.224)
Owns property	1.053				1.088
	(0.191)				(0.200)
Occupation, *Base: Full-time*					
Part-time	1.351				1.084
	(0.195)				(0.208)
Self-employed	1.602				1.143
	(0.294)				(0.308)
In training/ in school	1.311				1.214
	(0.442)				(0.464)
Seeking work	0.496				0.364
	(1.063)				(1.126)
House wife / house husband	1.894				1.630
	(0.475)				(0.501)
Retired	1.079				0.862
	(0.306)				(0.321)
Education, *Base: Compulsory schooling*					
Vocational training	1.254				1.268
	(0.647)				(0.673)
A-levels	1.510				1.514
	(0.674)				(0.705)
Higher education not university	1.902				1.926
	(0.657)				(0.687)
University	1.565				1.470
	(0.650)				(0.681)
Income, *Base: below CHF4,000*					
CHF4,001-6,000	1.170				1.201
	(0.283)				(0.296)
CHF6,001-8,000	0.956				1.138
	(0.289)				(0.303)
CHF8,001-10,000	1.220				1.465
	(0.315)				(0.334)
CHF10,001-12,000	1.153				1.590
	(0.348)				(0.369)
CHF12,001-14,000	1.781				2.460**
	(0.368)				(0.391)
CHF14,001-16,000	1.703				2.701**
	(0.413)				(0.442)
above CHF16,000	1.245				1.930
	(0.444)				(0.470)
Zip code fixed effect	Yes				Yes

Table 3.A.5 continued

	Demo-graphics (1)	Energy literacy (2)	Attitudes (3)	Reasons for choice (4)	Full model (5)
Energy literacy					
Knows consumption question		0.962			0.879
		(0.153)			(0.166)
Knows new renewable mix		0.850			0.862
		(0.145)			(0.158)
Knows hydro mix		1.117			0.922
		(0.160)			(0.173)
Explains concept of green electricity easily		1.026			1.025
		(0.096)			(0.125)
Political attitude			0.816***		0.815***
			(0.036)		(0.040)
Personal attitudes					
Nature importance			1.215***		1.251***
			(0.054)		(0.061)
Ease of decision			0.970		0.994
			(0.033)		(0.036)
Patience			1.061**		1.074**
			(0.028)		(0.030)
Trust			0.969		0.939**
			(0.028)		(0.030)
Altruism			0.985		0.955
			(0.030)		(0.034)
Reasons for choice					
I informed myself before choosing				0.804**	0.749***
				(0.095)	(0.107)
It was a complex decision				1.170*	1.272**
				(0.085)	(0.097)
I did not know that I could choose				1.128	1.175*
				(0.081)	(0.087)
It was an unimportant decision				0.643***	0.770**
				(0.106)	(0.113)
I forgot to decide				1.159	1.216*
				(0.105)	(0.112)
The default appeared to be a recommendation				0.991	0.974
				(0.072)	(0.077)
I kept the effort low				0.915	0.918
				(0.079)	(0.085)
I actually never made a decision				1.034	0.945
				(0.105)	(0.111)
I did not have enough info about the products				0.922	0.954
				(0.097)	(0.101)
Decision taking, *Base: Alone*					
Together				1.704***	2.139***
				(0.153)	(0.225)
Not aware of decision				0.878	1.061
				(0.374)	(0.402)

Table 3.A.5 continued

	Demo-graphics (1)	Energy literacy (2)	Attitudes (3)	Reasons for choice (4)	Full model (5)
Constant DEFAULT | GREEN+	5.564**	4.641***	8.954***	2.531*	22.129**
	(0.774)	(0.290)	(0.684)	(0.498)	(1.272)
Constant GREEN+ | GREEN++	39.107***	31.421***	63.929***	17.749***	174.202***
	(0.787)	(0.322)	(0.699)	(0.516)	(1.282)
AIC	1528.85	1524.86	1458.92	1494.05	1460.20
Observations	1,362	1,362	1,362	1,362	1,362

Note.— *p<0.1; **p<0.05; ***p<0.01

Ordered Logistic Model; the dependent variable is an ordered outcome, the base category is a match in current contract and active preference. The dependent variable further splits in two categories, a mismatch between DEFAULT and GREEN+, and a mismatch between DEFAULT and GREEN++. Five models are shown in order to allow for the stepwise interpretability of different sets of coefficients. First, we analyze the influence of demographics only on mismatch. Next, we look into whether energy literacy can predict the mismatch to greener contracts. In model (3) we assess the impact of attitudes on mismatch to greener contracts. Last, we assess the impact of reasons for choice mismatches. Only fully completed questionnaires are considered. Coefficients are reported as odds-ratios. Standard errors are shown in parentheses.

Table 3.A.6: Ordered Logistic model | Mismatch for consumers with greener preferences, including inconsistent choices

	Demo-graphics (1)	Energy literacy (2)	Attitudes (3)	Reasons for choice (4)	Full model (5)
Age	0.994				1.000
	(0.007)				(0.008)
Gender: Male	0.737*				0.983
	(0.161)				(0.181)
Nationality: Swiss	0.874				0.996
	(0.223)				(0.232)
Language: Native	1.262				1.300
	(0.305)				(0.314)
Single	1.030				1.795**
	(0.193)				(0.251)
Single parent	1.265				1.326
	(0.551)				(0.577)
Has children	0.501***				0.436***
	(0.214)				(0.224)
Owns property	1.053				1.088
	(0.191)				(0.200)
Occupation, *Base: Full-time*					
Part-time	1.351				1.084
	(0.195)				(0.208)
Self-employed	1.603				1.143
	(0.294)				(0.308)
In training/ in school	1.311				1.215
	(0.442)				(0.464)
Seeking work	0.496				0.364
	(1.063)				(1.126)
House wife / house husband	1.894				1.633
	(0.475)				(0.501)
Retired	1.079				0.862
	(0.306)				(0.321)
Education, *Base: Compulsory schooling*					
Vocational training	1.254				1.270
	(0.647)				(0.673)
A-levels	1.510				1.516
	(0.674)				(0.705)
Higher education not university	1.902				1.928
	(0.657)				(0.687)
University	1.566				1.472
	(0.650)				(0.681)
Income, *Base: below CHF4,000*					
CHF4,001-6,000	1.170				1.202
	(0.283)				(0.296)
CHF6,001-8,000	0.956				1.138
	(0.289)				(0.303)
CHF8,001-10,000	1.220				1.466
	(0.315)				(0.334)
CHF10,001-12,000	1.152				1.590
	(0.348)				(0.369)
CHF12,001-14,000	1.781				2.459**
	(0.368)				(0.391)
CHF14,001-16,000	1.703				2.701**
	(0.413)				(0.442)
above CHF16,000	1.245				1.932
	(0.444)				(0.470)
Zip code fixed effect	Yes				Yes

Table 3.A.6 continued

	Demo-graphics (1)	Energy literacy (2)	Attitudes (3)	Reasons for choice (4)	Full model (5)
Energy literacy					
Knows consumption question		0.962			0.879
		(0.153)			(0.166)
Knows new renewable mix		0.850			0.862
		(0.145)			(0.158)
Knows hydro mix		1.117			0.922
		(0.160)			(0.173)
Explains concept of green electricity easily		1.026			1.025
		(0.096)			(0.125)
Political attitude			0.816***		0.815***
			(0.036)		(0.040)
Personal attitudes					
Nature importance			1.215***		1.251***
			(0.054)		(0.061)
Ease of decision			0.970		0.994
			(0.033)		(0.036)
Patience			1.061**		1.074**
			(0.028)		(0.030)
Trust			0.969		0.939**
			(0.028)		(0.030)
Altruism			0.985		0.955
			(0.030)		(0.034)
Reasons for choice					
I informed myself before choosing				0.804**	0.749***
				(0.095)	(0.107)
It was a complex decision				1.170*	1.272**
				(0.085)	(0.097)
I did not know that I could choose				1.128	1.175*
				(0.081)	(0.087)
It was an unimportant decision				0.643***	0.770**
				(0.106)	(0.113)
I forgot to decide				1.159	1.216*
				(0.105)	(0.112)
The default appeared to be a recommendation				0.991	0.974
				(0.072)	(0.077)
I kept the effort low				0.915	0.918
				(0.079)	(0.085)
I actually never made a decision				1.034	0.945
				(0.105)	(0.111)
I did not have enough info about the products				0.922	0.954
				(0.097)	(0.101)
Decision taking, *Base: Alone*					
Together				1.704***	2.140***
				(0.153)	(0.225)
Not aware of decision				0.878	1.061
				(0.374)	(0.402)

Table 3.A.6 continued

	Demo-graphics (1)	Energy literacy (2)	Attitudes (3)	Reasons for choice (4)	Full model (5)
Constant DEFAULT | GREEN+	5.564**	4.641***	8.954***	2.531*	22.129**
	(0.774)	(0.290)	(0.684)	(0.498)	(1.272)
Constant GREEN+ | GREEN++	39.107***	31.421***	63.929***	17.749***	174.202***
	(0.787)	(0.322)	(0.699)	(0.516)	(1.282)
AIC	1529.91	1524.86	1458.92	1494.07	1463.52
Observations	1,362	1,362	1,362	1,362	1,362

Note.— *p<0.1; **p<0.05; ***p<0.01

Ordered Logistic Model; the dependent variable is an ordered outcome, the base category is a match in current contract and active preference. The dependent variable further splits in two categories, a mismatch between DEFAULT and GREEN+, and a mismatch between DEFAULT and GREEN++. Additionally, the categories of the dependent variable also include those households who have a mismatch to a greener contract, but who are not in DEFAULT. Five models are shown in order to allow for the stepwise interpretability of different sets of coefficients. First, we analyze the influence of demographics only on mismatch. Next, we look into whether energy literacy can predict the mismatch to greener contracts. In model (3) we assess the impact of attitudes on mismatch to greener contracts. Last, we assess the impact of reasons for choice mismatches. Only fully completed questionnaires are considered. Coefficients are reported as odds-ratios. Standard errors are shown in parentheses.

Table 3.A.7: Generalized ordered Logistic model | Mismatch for consumers with greener preferences

	Demo-graphics (1)	Energy literacy (2)	Attitudes (3)	Reasons for choice (4)	Full model (5)
Age	0.998				1.000
	(0.003)				(0.005)
Gender: Male	0.891				1.002
	(0.079)				(0.118)
Nationality: Swiss	0.969				1.034
	(0.096)				(0.157)
Language: Native	0.393**				0.638
	(0.363)				(0.347)
Single	1.012				1.436*
	(0.078)				(0.212)
Single parent	1.107				1.202
	(0.222)				(0.378)
Has children	0.769**				0.589**
	(0.133)				(0.237)
Owns property	1.009				1.049
	(0.077)				(0.132)
Occupation, *Base: Full-time*					
Part-time	1.112				1.039
	(0.090)				(0.137)
Self-employed	1.144				1.042
	(0.135)				(0.207)
In training/ in school	1.051				1.054
	(0.188)				(0.316)
Seeking work	0.493				0.382
	(0.710)				(0.919)
House wife / house husband	1.349				1.411
	(0.218)				(0.343)
Retired	1.020				0.904
	(0.125)				(0.212)
Education, *Base: Compulsory schooling*					
Vocational training	1.131				1.285
	(0.305)				(0.476)
A-levels	1.193				1.418
	(0.318)				(0.499)
Higher education not university	1.337				1.688
	(0.322)				(0.499)
University	1.233				1.403
	(0.312)				(0.484)
Income, *Base: below CHF4,000*					
CHF4,001-6,000	1.073				1.134
	(0.120)				(0.199)
CHF6,001-8,000	0.978				1.075
	(0.119)				(0.201)
CHF8,001-10,000	1.095				1.291
	(0.133)				(0.234)
CHF10,001-12,000	1.057				1.352
	(0.144)				(0.263)
CHF12,001-14,000	1.275				1.821*
	(0.172)				(0.320)
CHF14,001-16,000	1.270				1.942*
	(0.186)				(0.358)
above CHF16,000	1.136				1.583
	(0.184)				(0.335)
Zip code fixed effect	Yes				Yes

Table 3.A.7 continued

	Demo-graphics (1)	Energy literacy (2)	Attitudes (3)	Reasons for choice (4)	Full model (5)
Energy literacy					
Knows consumption question		0.962			0.938
		(0.153)			(0.113)
Knows new renewable mix		0.850			0.917
		(0.145)			(0.108)
Knows hydro mix		1.117			0.928
		(0.160)			(0.114)
Explains concept of green electricity easily		1.026			1.014
		(0.096)			(0.082)
Political attitude			0.816***		0.873***
			(0.036)		(0.052)
Personal attitudes					
Nature importance			1.215***		1.155**
			(0.054)		(0.064)
Ease of decision			0.970		0.998
			(0.033)		(0.024)
Patience			1.061**		1.042
			(0.028)		(0.026)
Trust			0.969		0.959*
			(0.028)		(0.024)
Altruism			0.985		0.971
			(0.030)		(0.025)
Reasons for choice					
I informed myself before choosing				0.873*	0.839*
				(0.076)	(0.097)
It was a complex decision				1.027	1.163*
				(0.076)	(0.084)
I did not know that I could choose				1.081	1.100
				(0.072)	(0.069)
It was an unimportant decision				1.277***	0.850*
				(0.091)	(0.095)
I forgot to decide				0.950	1.124
				(0.090)	(0.086)
The default appeared to be a recommendation				1.145**	0.989
				(0.068)	(0.051)
I kept the effort low				0.842	0.947
				(0.142)	(0.059)
I actually never made a decision				1.002	0.984
				(0.085)	(0.075)
I did not have enough info about the products				1.097	0.978
				(0.083)	(0.067)
Decision taking, *Base: Alone*					
Together				2.365**	1.593**
				(0.384)	(0.230)
Not aware of decision				0.366	1.062
				(1.050)	(0.263)

Table 3.A.7 continued

	Demo-graphics (1)	Energy literacy (2)	Attitudes (3)	Reasons for choice (4)	Full model (5)
Constant DEFAULT | GREEN+	6.463***	4.641***	8.954***	0.657	15.224***
	(0.419)	(0.290)	(0.684)	(1.359)	(0.903)
Constant GREEN+ | GREEN++	14.702***	31.421***	63.929***	30.791***	59.741***
	(0.497)	(0.322)	(0.699)	(1.067)	(1.117)
AIC	1523.21	1524.86	1458.92	1476.26	1463.64
Observations	1,362	1,362	1,362	1,362	

Note.— *p<0.1; **p<0.05; ***p<0.01

Generalized Ordered Logistic Model; the dependent variable is an ordered outcome, the base category is a match in current contract and active preference. The dependent variable further splits in two categories, a mismatch between DEFAULT and GREEN+, and a mismatch between DEFAULT and GREEN++. The model relaxes the proportional odds assumption. Five models are shown in order to allow for the stepwise interpretability of different sets of coefficients. First, we analyze the influence of demographics only on mismatch. Next, we look into whether energy literacy can predict the mismatch to greener contracts. In model (3) we assess the impact of attitudes on mismatch to greener contracts. Last, we assess the impact of reasons for choice mismatches. Only fully completed questionnaires are considered. Coefficients are reported as odds-ratios. Standard errors are shown in parentheses.

Table 3.A.8: Generalized ordered Logistic model | Mismatch for consumers with greener preferences, including inconsistent choices

	Demo-graphics (1)	Energy literacy (2)	Attitudes (3)	Reasons for choice (4)	Full model (5)
Age	0.991**				1.000
	(0.005)				(0.005)
Gender: Male	0.921				1.002
	(0.056)				(0.118)
Nationality: Swiss	0.975				1.034
	(0.066)				(0.157)
Language: Native	0.409**				0.638
	(0.363)				(0.347)
Single	1.005				1.436*
	(0.052)				(0.212)
Single parent	1.069				1.202
	(0.154)				(0.378)
Has children	0.833*				0.589**
	(0.097)				(0.237)
Owns property	1.011				1.049
	(0.048)				(0.132)
Occupation, *Base: Full-time*					
Part-time	1.084				1.039
	(0.064)				(0.137)
Self-employed	1.087				1.042
	(0.090)				(0.207)
In training/ in school	1.048				1.054
	(0.149)				(0.316)
Seeking work	0.599				0.382
	(0.518)				(0.919)
House wife / house husband	1.224				1.411
	(0.157)				(0.343)
Retired	1.009				0.904
	(0.078)				(0.212)
Education, *Base: Compulsory schooling*					
Vocational training	0.982				1.285
	(0.172)				(0.476)
A-levels	1.011				1.418
	(0.183)				(0.499)
Higher education not university	1.101				1.688
	(0.184)				(0.499)
University	1.055				1.403
	(0.176)				(0.484)
Income, *Base: below CHF4,000*					
CHF4,001-6,000	1.058				1.134
	(0.079)				(0.199)
CHF6,001-8,000	1.005				1.075
	(0.079)				(0.201)
CHF8,001-10,000	1.071				1.291
	(0.090)				(0.234)
CHF10,001-12,000	1.057				1.352
	(0.097)				(0.263)
CHF12,001-14,000	1.181				1.821*
	(0.120)				(0.320)
CHF14,001-16,000	1.187				1.942*
	(0.129)				(0.358)
above CHF16,000	1.096				1.583
	(0.123)				(0.335)
Zip code fixed effect	Yes				Yes

Table 3.A.8 continued

	Demo-graphics (1)	Energy literacy (2)	Attitudes (3)	Reasons for choice (4)	Full model (5)
Energy literacy					
Knows consumption question		0.962			0.938
		(0.153)			(0.113)
Knows new renewable mix		0.850			0.917
		(0.145)			(0.108)
Knows hydro mix		1.117			0.928
		(0.160)			(0.114)
Explains concept of green electricity easily		1.026			1.014
		(0.096)			(0.082)
Political attitude			0.816***		0.873***
			(0.036)		(0.052)
Personal attitudes					
Nature importance			1.215***		1.155**
			(0.054)		(0.064)
Ease of decision			0.970		0.998
			(0.033)		(0.024)
Patience			1.061**		1.042
			(0.028)		(0.026)
Trust			0.969		0.959*
			(0.028)		(0.024)
Altruism			0.985		0.971
			(0.030)		(0.025)
Reasons for choice					
I informed myself before choosing				0.873*	0.839*
				(0.076)	(0.097)
It was a complex decision				1.027	1.163*
				(0.076)	(0.084)
I did not know that I could choose				1.081	1.100
				(0.072)	(0.069)
It was an unimportant decision				1.277***	0.850*
				(0.091)	(0.095)
I forgot to decide				0.950	1.124
				(0.090)	(0.086)
The default appeared to be a recommendation				1.145**	0.989
				(0.068)	(0.051)
I kept the effort low				0.842	0.947
				(0.142)	(0.059)
I actually never made a decision				1.002	0.984
				(0.085)	(0.075)
I did not have enough info about the products				1.097	0.978
				(0.083)	(0.067)
Decision taking, *Base: Alone*					
Together				2.365**	1.593**
				(0.384)	(0.230)
Not aware of decision				0.366	1.062
				(1.050)	(0.263)

Table 3.A.8 continued

	Demo-graphics (1)	Energy literacy (2)	Attitudes (3)	Reasons for choice (4)	Full model (5)
Constant DEFAULT | GREEN+	3.714***	4.641***	8.954***	0.657	15.224***
	(0.346)	(0.290)	(0.684)	(1.359)	(0.903)
Constant GREEN+ | GREEN++	6.522***	31.421***	63.929***	30.791***	59.741***
	(0.472)	(0.322)	(0.699)	(1.067)	(1.117)
AIC	1521.11	1524.86	1458.92	1476.26	1463.64
Observations	1,362	1,362	1,362	1,362	

Note.— *p<0.1; **p<0.05; ***p<0.01

Generalized Ordered Logistic Model; the dependent variable is an ordered outcome, the base category is a match in current contract and active preference. The dependent variable further splits in two categories, a mismatch between DEFAULT and GREEN+, and a mismatch between DEFAULT and GREEN++. Additionally, the categories of the dependent variable also include those households who have a mismatch to a greener contract, but who are not in DEFAULT. The model relaxes the proportional odds assumption. Five models are shown in order to allow for the stepwise interpretability of different sets of coefficients. First, we analyze the influence of demographics only on mismatch. Next, we look into whether energy literacy can predict the mismatch to greener contracts. In model (3) we assess the impact of attitudes on mismatch to greener contracts. Last, we assess the impact of reasons for choice mismatches. Only fully completed questionnaires are considered. Coefficients are reported as odds-ratios. Standard errors are shown in parentheses.

Table 3.A.9: Linear probability model | Mismatch for consumers with greyer preferences, including inconsistent choices

	Demo-graphics (1)	Energy literacy (2)	Attitudes (3)	Reasons for choice (4)	Full model (5)
Age	−0.001				0.001
	(0.001)				(0.001)
Gender: Male	−0.011				0.003
	(0.028)				(0.030)
Nationality: Swiss	0.029				0.036
	(0.040)				(0.039)
Language: Native	−0.032				−0.018
	(0.044)				(0.048)
Single	−0.014				−0.045
	(0.033)				(0.040)
Single parent	0.065				0.060
	(0.089)				(0.083)
Has children	0.047				0.045
	(0.033)				(0.033)
Owns property	−0.101***				−0.081***
	(0.031)				(0.031)
Occupation, *Base: Full-time*					
Part-time	−0.061*				−0.014
	(0.034)				(0.034)
Self-employed	−0.047				−0.034
	(0.055)				(0.050)
In training/ in school	0.097				0.110
	(0.084)				(0.083)
Seeking work	−0.050				−0.017
	(0.106)				(0.127)
House wife / house husband	0.047				0.056
	(0.088)				(0.088)
Retired	−0.044				−0.047
	(0.052)				(0.049)
Education, *Base: Compulsory schooling*					
Vocational training	−0.078				−0.051
	(0.089)				(0.098)
A-levels	−0.220**				−0.162
	(0.097)				(0.105)
Higher education not university	−0.199**				−0.151
	(0.092)				(0.101)
University	−0.174*				−0.094
	(0.090)				(0.101)
Income, *Base: below CHF4,000*					
CHF4,001-6,000	0.096**				0.085*
	(0.046)				(0.050)
CHF6,001-8,000	−0.013				−0.012
	(0.047)				(0.049)
CHF8,001-10,000	0.011				0.010
	(0.053)				(0.054)
CHF10,001-12,000	−0.014				−0.003
	(0.058)				(0.058)
CHF12,001-14,000	−0.063				−0.049
	(0.067)				(0.065)
CHF14,001-16,000	−0.026				−0.024
	(0.079)				(0.072)
above CHF16,000	0.104				0.100
	(0.075)				(0.079)
Zip code fixed effect	Yes				Yes

Table 3.A.9 continued

	Demo-graphics (1)	Energy literacy (2)	Attitudes (3)	Reasons for choice (4)	Full model (5)
Energy literacy					
Knows consumption question		0.010			0.007
		(0.027)			(0.027)
Knows new renewable mix		−0.069***			−0.045*
		(0.025)			(0.025)
Knows hydro mix		−0.006			0.015
		(0.028)			(0.028)
Explains concept of green electricity easily		−0.074***			−0.011
		(0.017)			(0.020)
Political attitude			0.016***		0.017***
			(0.006)		(0.006)
Personal attitudes					
Nature importance			−0.024***		−0.011
			(0.008)		(0.009)
Ease of decision			−0.008		0.002
			(0.006)		(0.006)
Patience			0.006		0.002
			(0.005)		(0.005)
Trust			0.004		0.002
			(0.005)		(0.005)
Altruism			−0.007		0.001
			(0.005)		(0.005)
Reasons for choice					
I informed myself before choosing				−0.070***	−0.056***
				(0.016)	(0.017)
It was a complex decision				0.024	0.008
				(0.015)	(0.016)
I did not know that I could choose				−0.026*	−0.030**
				(0.015)	(0.015)
It was an unimportant decision				0.050***	0.038**
				(0.016)	(0.016)
I forgot to decide				0.001	0.007
				(0.020)	(0.020)
The default appeared to be a recommendation				0.024**	0.028**
				(0.012)	(0.012)
I kept the effort low				−0.005	−0.010
				(0.014)	(0.014)
I actually never made a decision				0.004	−0.004
				(0.019)	(0.020)
I did not have enough info about the products				0.014	0.010
				(0.017)	(0.017)
Decision taking, *Base: Alone*					
Together				−0.060**	−0.064*
				(0.025)	(0.033)
Not aware of decision				0.135**	0.102
				(0.067)	(0.068)

Table 3.A.9 continued

	Demo-graphics (1)	Energy literacy (2)	Attitudes (3)	Reasons for choice (4)	Full model (5)
Constant	0.532***	0.547***	0.498***	0.344***	0.478**
	(0.121)	(0.052)	(0.111)	(0.089)	(0.195)
R^2	0.062	0.024	0.025	0.072	0.120
Observations	1,362	1,362	1362	1,362	1,362

Note.— *p<0.1; **p<0.05; ***p<0.01

Linear Probability Model (OLS); the dependent variable takes the value 1 if a respondent who is currently in DEFAULT has a mismatch (=an active preference) to a greyer electricity contract. Additionally, the dependent variable also includes those households who have a mismatch to a greyer contract, but who are not in DEFAULT. Five models are shown in order to allow for the stepwise interpretability of different sets of coefficients. First, we analyze the influence of demographics only on mismatch. Next, we look into whether energy literacy can predict the mismatch to greyer contracts. In model (3) we assess the impact of attitudes on mismatch to greyer contracts. Last, we assess the impact of reasons for choice mismatches. The final column estimates the complete model. Only fully completed questionnaires are considered. Heteroskedasticity-robust standard errors are in parentheses.

Table 3.A.10: Ordered Logistic model | Mismatch for consumers with greyer preferences

	Demo-graphics (1)	Energy literacy (2)	Attitudes (3)	Reasons for choice (4)	Full model (5)
Age	0.994				1.002
	(0.006)				(0.007)
Gender: Male	0.906				0.938
	(0.144)				(0.161)
Nationality: Swiss	1.329				1.351
	(0.202)				(0.212)
Language: Native	0.677*				0.699
	(0.228)				(0.239)
Single	0.855				0.712*
	(0.171)				(0.204)
Single parent	1.702				1.757
	(0.426)				(0.443)
Has children	1.191				1.238
	(0.170)				(0.177)
Owns property	0.600***				0.644**
	(0.167)				(0.175)
Occupation, *Base: Full-time*					
Part-time	0.729*				0.953
	(0.176)				(0.189)
Self-employed	0.817				0.847
	(0.277)				(0.291)
In training/ in school	1.661				1.985*
	(0.376)				(0.393)
Seeking work	0.821				1.025
	(0.583)				(0.613)
House wife / house husband	0.958				1.011
	(0.405)				(0.422)
Retired	0.865				0.822
	(0.266)				(0.280)
Education, *Base: Compulsory schooling*					
Vocational training	0.650				0.679
	(0.407)				(0.425)
A-levels	0.304***				0.343**
	(0.457)				(0.479)
Higher education not university	0.362**				0.412*
	(0.432)				(0.453)
University	0.367**				0.494
	(0.419)				(0.443)
Income, *Base: below CHF4,000*					
CHF4,001-6,000	1.571*				1.500
	(0.238)				(0.251)
CHF6,001-8,000	0.919				0.913
	(0.249)				(0.261)
CHF8,001-10,000	1.099				1.152
	(0.277)				(0.290)
CHF10,001-12,000	0.953				1.006
	(0.311)				(0.328)
CHF12,001-14,000	0.797				0.824
	(0.355)				(0.375)
CHF14,001-16,000	0.836				0.808
	(0.404)				(0.423)
above CHF16,000	1.316				1.291
	(0.388)				(0.410)
Zip code fixed effect	Yes				Yes

Table 3.A.10 continued

	Demo-graphics (1)	Energy literacy (2)	Attitudes (3)	Reasons for choice (4)	Full model (5)
Energy literacy					
Knows consumption question		0.948			0.948
		(0.134)			(0.143)
Knows new renewable mix		0.687***			0.759**
		(0.128)			(0.140)
Knows hydro mix		0.895			0.984
		(0.145)			(0.156)
Explains concept of green electricity easily		0.687***			0.945
		(0.083)			(0.103)
Political attitude			1.109***		1.137***
			(0.028)		(0.032)
Personal attitudes					
Nature importance			0.875***		0.929*
			(0.039)		(0.044)
Ease of decision			0.961		1.009
			(0.028)		(0.031)
Patience			1.033		1.015
			(0.024)		(0.026)
Trust			1.012		1.008
			(0.024)		(0.026)
Altruism			0.971		1.028
			(0.024)		(0.028)
Reasons for choice					
I informed myself before choosing				0.709***	0.743***
				(0.082)	(0.090)
It was a complex decision				1.104	0.998
				(0.074)	(0.081)
I did not know that I could choose				0.964	0.944
				(0.072)	(0.076)
It was an unimportant decision				1.325***	1.242***
				(0.075)	(0.080)
I forgot to decide				1.008	1.041
				(0.086)	(0.091)
The default appeared to be a recommendation				1.145**	1.178**
				(0.065)	(0.069)
I kept the effort low				0.986	0.971
				(0.072)	(0.075)
I actually never made a decision				1.026	0.999
				(0.087)	(0.093)
I did not have enough info about the products				1.023	0.986
				(0.081)	(0.085)
Decision taking, *Base: Alone*					
Together				0.742**	0.712**
				(0.131)	(0.169)
Not aware of decision				1.721**	1.484
				(0.268)	(0.291)

Table 3.A.10 continued

	Demo-graphics (1)	Energy literacy (2)	Attitudes (3)	Reasons for choice (4)	Full model (5)
Constant DEFAULT | GREY−	0.671	0.749	1.107	2.806**	1.188
	(0.565)	(0.243)	(0.524)	(0.427)	(0.968)
Constant GREY− | GREY−−	8.772***	9.127***	13.588***	37.143***	17.741***
	(0.578)	(0.277)	(0.541)	(0.452)	(0.978)
AIC	1831.58	1832.54	1828.92	1775.29	1773.26
Observations	1,362	1,362	1,362	1,362	1,362

Note.— *p<0.1; **p<0.05; ***p<0.01

Ordered Logistic Model; the dependent variable is an ordered outcome, the base category is a match in current contract and active preference. The dependent variable further splits in two categories, a mismatch between DEFAULT and GREY−, and a mismatch between DEFAULT and GREY−−. Five models are shown in order to allow for the stepwise interpretability of different sets of coefficients. First, we analyze the influence of demographics only on mismatch. Next, we look into whether energy literacy can predict the mismatch to greyer contracts. In model (3) we assess the impact of attitudes on mismatch to greyer contracts. Last, we assess the impact of reasons for choice mismatches. The final column estimates the complete model. Only fully completed questionnaires are considered. Coefficients are reported as odds-ratios. Standard errors are shown in parentheses.

Table 3.A.11: Ordered Logistic model | Mismatch for consumers with greyer preferences, including inconsistent choices

	Demo-graphics (1)	Energy literacy (2)	Attitudes (3)	Reasons for choice (4)	Full model (5)
Age	0.994				1.001
	(0.006)				(0.007)
Gender: Male	0.949				0.975
	(0.141)				(0.158)
Nationality: Swiss	1.365				1.406
	(0.199)				(0.209)
Language: Native	0.698				0.717
	(0.226)				(0.237)
Single	0.922				0.801
	(0.168)				(0.200)
Single parent	1.467				1.470
	(0.422)				(0.439)
Has children	1.194				1.234
	(0.168)				(0.174)
Owns property	0.633***				0.675**
	(0.164)				(0.173)
Occupation, *Base: Full-time*					
Part-time	0.706**				0.914
	(0.173)				(0.185)
Self-employed	0.777				0.829
	(0.271)				(0.285)
In training/ in school	1.764				2.059*
	(0.372)				(0.387)
Seeking work	0.766				0.974
	(0.583)				(0.612)
House wife / house husband	1.411				1.514
	(0.386)				(0.403)
Retired	0.835				0.799
	(0.261)				(0.275)
Education, *Base: Compulsory schooling*					
Vocational training	0.566				0.580
	(0.403)				(0.421)
A-levels	0.278***				0.320**
	(0.451)				(0.473)
Higher education not university	0.313***				0.350**
	(0.428)				(0.449)
University	0.347**				0.471*
	(0.414)				(0.438)
Income, *Base: below CHF4,000*					
CHF4,001-6,000	1.591*				1.531*
	(0.237)				(0.249)
CHF6,001-8,000	1.012				1.014
	(0.245)				(0.257)
CHF8,001-10,000	1.044				1.087
	(0.275)				(0.288)
CHF10,001-12,000	0.964				1.016
	(0.307)				(0.324)
CHF12,001-14,000	0.757				0.775
	(0.354)				(0.372)
CHF14,001-16,000	0.947				0.938
	(0.391)				(0.409)
above CHF16,000	1.684				1.659
	(0.374)				(0.395)
Zip code fixed effect	Yes				Yes

Table 3.A.11 continued

	Demo-graphics (1)	Energy literacy (2)	Attitudes (3)	Reasons for choice (4)	Full model (5)
Energy literacy					
Knows consumption question		1.027			1.034
		(0.130)			(0.139)
Knows new renewable mix		0.694***			0.768*
		(0.126)			(0.137)
Knows hydro mix		0.915			1.023
		(0.142)			(0.152)
Explains concept of green electricity easily		0.690***			0.935
		(0.081)			(0.101)
Political attitude			1.108***		1.133***
			(0.028)		(0.032)
Personal attitudes					
Nature importance			0.853***		0.902**
			(0.039)		(0.043)
Ease of decision			0.966		1.011
			(0.027)		(0.030)
Patience			1.023		1.000
			(0.023)		(0.025)
Trust			1.017		1.016
			(0.024)		(0.026)
Altruism			0.965		1.009
			(0.024)		(0.028)
Reasons for choice					
I informed myself before choosing				0.709***	0.744***
				(0.080)	(0.088)
It was a complex decision				1.090	1.000
				(0.072)	(0.080)
I did not know that I could choose				0.936	0.917
				(0.071)	(0.075)
It was an unimportant decision				1.328***	1.225**
				(0.074)	(0.079)
I forgot to decide				0.993	1.017
				(0.085)	(0.090)
The default appeared to be a recommendation				1.132*	1.165**
				(0.063)	(0.067)
I kept the effort low				0.988	0.961
				(0.070)	(0.073)
I actually never made a decision				0.995	0.963
				(0.086)	(0.092)
I did not have enough info about the products				1.071	1.043
				(0.079)	(0.084)
Decision taking, *Base: Alone*					
Together				0.759**	0.747*
				(0.128)	(0.166)
Not aware of decision				1.619*	1.435
				(0.265)	(0.288)

Table 3.A.11 continued

	Demo-graphics (1)	Energy literacy (2)	Attitudes (3)	Reasons for choice (4)	Full model (5)
Constant DEFAULT | GREY−	0.646	0.730	0.751	2.385**	0.611
	(0.561)	(0.239)	(0.520)	(0.420)	(0.958)
Constant GREY− | GREY−−	7.235***	7.642***	7.994***	26.817***	7.764**
	(0.571)	(0.266)	(0.531)	(0.439)	(0.963)
AIC	1912.27	1912.62	1901.02	1862.16	1853.66
Observations	1,362	1,362	1,362	1,362	1,362

Note.— *p<0.1; **p<0.05; ***p<0.01

Ordered Logistic Model; the dependent variable is an ordered outcome, the base category is a match in current contract and active preference. The dependent variable further splits in two categories, a mismatch between DEFAULT and GREY−, and a mismatch between DEFAULT and GREY−−. Additionally, the categories of the dependent variable also include those households who have a mismatch to a greyer contract, but who are not in DEFAULT. Five models are shown in order to allow for the stepwise interpretability of different sets of coefficients. First, we analyze the influence of demographics only on mismatch. Next, we look into whether energy literacy can predict the mismatch to greyer contracts. In model (3) we assess the impact of attitudes on mismatch to greyer contracts. Last, we assess the impact of reasons for choice mismatches. The final column estimates the complete model. Only fully completed questionnaires are considered. Coefficients are reported as odds-ratios. Standard errors are shown in parentheses.

Table 3.A.12: Generalized ordered Logistic model | Mismatch for consumers with greyer preferences

	Demo-graphics (1)	Energy literacy (2)	Attitudes (3)	Reasons for choice (4)	Full model (5)
Age	0.994				1.000
	(0.006)				(0.004)
Gender: Male	0.907				0.971
	(0.125)				(0.082)
Nationality: Swiss	1.302				1.191
	(0.185)				(0.118)
Language: Native	0.709*				0.825
	(0.206)				(0.128)
Single	0.876				0.826*
	(0.148)				(0.111)
Single parent	1.665				1.445
	(0.366)				(0.230)
Has children	1.160				1.093
	(0.152)				(0.093)
Owns property	0.650**				0.815*
	(0.170)				(0.105)
Occupation, *Base: Full-time*					
Part-time	0.759*				0.999
	(0.161)				(0.095)
Self-employed	0.854				0.938
	(0.237)				(0.146)
In training/ in school	1.531				1.442*
	(0.339)				(0.221)
Seeking work	0.899				1.111
	(0.531)				(0.314)
House wife / house husband	0.963				1.003
	(0.348)				(0.207)
Retired	0.890				0.903
	(0.235)				(0.142)
Education, *Base: Compulsory schooling*					
Vocational training	0.616				0.752
	(0.350)				(0.228)
A-levels	0.331***				0.541**
	(0.411)				(0.279)
Higher education not university	0.363***				0.587**
	(0.378)				(0.256)
University	0.378***				0.642*
	(0.369)				(0.248)
Income, *Base: below CHF4,000*					
CHF4,001-6,000	0.776				0.805
	(0.202)				(0.201)
CHF6,001-8,000	0.856				0.794
	(0.210)				(0.202)
CHF8,001-10,000	0.865				0.701*
	(0.221)				(0.216)
CHF10,001-12,000	0.929				0.808
	(0.257)				(0.238)
CHF12,001-14,000	1.080				1.064
	(0.301)				(0.282)
CHF14,001-16,000	0.798				0.726
	(0.396)				(0.366)
above CHF16,000	0.674				0.733
	(0.340)				(0.313)
Zip code fixed effect	Yes				Yes

Table 3.A.12 continued

	Demo-graphics (1)	Energy literacy (2)	Attitudes (3)	Reasons for choice (4)	Full model (5)
Energy literacy					
Knows consumption question		0.957			0.975
		(0.113)			(0.071)
Knows new renewable mix		0.725***			0.917*
		(0.112)			(0.077)
Knows hydro mix		0.918			0.984
		(0.122)			(0.078)
Explains concept of green electricity easily		0.723***			0.983
		(0.075)			(0.051)
Political attitude			1.097***		1.077***
			(0.030)		(0.022)
Personal attitudes					
Nature importance			0.920**		0.913
			(0.038)		(0.024)
Ease of decision			0.983		1.006
			(0.018)		(0.016)
Patience			1.013		1.001
			(0.015)		(0.013)
Trust			0.960		0.968*
			(0.025)		(0.021)
Altruism			0.981		1.009
			(0.016)		(0.014)
Reasons for choice					
I informed myself before choosing				0.745***	0.860**
				(0.085)	(0.060)
It was a complex decision				0.940	0.889*
				(0.060)	(0.065)
I did not know that I could choose				0.964	0.966
				(0.063)	(0.039)
It was an unimportant decision				1.277***	1.134**
				(0.074)	(0.049)
I forgot to decide				0.999	1.021
				(0.076)	(0.047)
The default appeared to be a recommendation				1.128**	1.097**
				(0.058)	(0.040)
I kept the effort low				0.981	0.975
				(0.063)	(0.039)
I actually never made a decision				1.033	0.997
				(0.076)	(0.047)
I did not have enough info about the products				1.016	0.981
				(0.071)	(0.043)
Decision taking, *Base: Alone*					
Together				0.772**	0.838*
				(0.120)	(0.093)
Not aware of decision				1.590*	1.178
				(0.246)	(0.153)

Table 3.A.12 continued

	Demo-graphics (1)	Energy literacy (2)	Attitudes (3)	Reasons for choice (4)	Full model (5)
Constant DEFAULT | GREY−	0.735	0.767	1.355	2.385***	1.324
	(0.527)	(0.204)	(0.370)	(0.377)	(0.504)
Constant GREY− | GREY−−	6.711***	6.442***	7.667***	26.817***	3.528***
	(0.567)	(0.293)	(0.666)	(0.506)	(0.606)
AIC	1842.11	1831.28	1809.38	1776.23	1779.09
Observations	1,362	1,362	1,362	1,362	1,362

Note.— *p<0.1; **p<0.05; ***p<0.01

Generalized Ordered Logistic Model; the dependent variable is an ordered outcome, the base category is a match in current contract and active preference. The dependent variable further splits in two categories, a mismatch between DEFAULT and GREY−, and a mismatch between DEFAULT and GREY−−. The model relaxes the proportional odds assumption. Five models are shown in order to allow for the stepwise interpretability of different sets of coefficients. First, we analyze the influence of demographics only on mismatch. Next, we look into whether energy literacy can predict the mismatch to greyer contracts. In model (3) we assess the impact of attitudes on mismatch to greyer contracts. Last, we assess the impact of reasons for choice mismatches. The final column estimates the complete model. Only fully completed questionnaires are considered. Coefficients are reported as odds-ratios. Standard errors are shown in parentheses.

Table 3.A.13: Generalized ordered Logistic model | Mismatch for consumers with greyer preferences, including inconsistent choices

	Demo-graphics (1)	Energy literacy (2)	Attitudes (3)	Reasons for choice (4)	Full model (5)
Age	0.994				1.001
	(0.006)				(0.003)
Gender: Male	0.952				0.967
	(0.128)				(0.067)
Nationality: Swiss	1.357*				1.174*
	(0.185)				(0.097)
Language: Native	0.713				0.863
	(0.208)				(0.105)
Single	0.934				0.893
	(0.150)				(0.086)
Single parent	1.475				1.244
	(0.374)				(0.182)
Has children	1.185				1.127
	(0.154)				(0.078)
Owns property	0.671**				0.857*
	(0.165)				(0.084)
Occupation, *Base: Full-time*					
Part-time	0.736*				0.994
	(0.165)				(0.075)
Self-employed	0.791				0.957
	(0.245)				(0.119)
In training/ in school	1.659				1.375*
	(0.346)				(0.175)
Seeking work	0.835				0.983
	(0.545)				(0.266)
House wife / house husband	1.340				1.092
	(0.354)				(0.164)
Retired	0.864				0.886
	(0.239)				(0.120)
Education, *Base: Compulsory schooling*					
Vocational training	0.563				0.738
	(0.362)				(0.196)
A-levels	0.309***				0.571**
	(0.427)				(0.240)
Higher education not university	0.331***				0.585**
	(0.397)				(0.226)
University	0.368***				0.656**
	(0.384)				(0.210)
Income, *Base: below CHF4,000*					
CHF4,001-6,000	0.795				0.846
	(0.199)				(0.199)
CHF6,001-8,000	0.934				1.014
	(0.203)				(0.202)
CHF8,001-10,000	0.860				0.735
	(0.220)				(0.221)
CHF10,001-12,000	0.864				0.937
	(0.248)				(0.237)
CHF12,001-14,000	1.052				1.175
	(0.300)				(0.287)
CHF14,001-16,000	0.954				0.867
	(0.341)				(0.350)
above CHF16,000	0.859				1.134
	(0.298)				(0.287)
Zip code fixed effect	Yes				Yes

Table 3.A.13 continued

	Demo-graphics (1)	Energy literacy (2)	Attitudes (3)	Reasons for choice (4)	Full model (5)
Energy literacy					
Knows consumption question		1.027			1.029
		(0.130)			(0.058)
Knows new renewable mix		0.694***			0.904
		(0.126)			(0.063)
Knows hydro mix		0.915			0.973
		(0.142)			(0.063)
Explains concept of green electricity easily		0.690***			0.970
		(0.081)			(0.043)
Political attitude			1.106***		1.052***
			(0.031)		(0.019)
Personal attitudes					
Nature importance			0.908***		0.960*
			(0.037)		(0.022)
Ease of decision			0.985		1.004
			(0.016)		(0.012)
Patience			1.006		1.000
			(0.013)		(0.010)
Trust			1.004		1.006
			(0.013)		(0.011)
Altruism			0.973		0.939***
			(0.024)		(0.020)
Reasons for choice					
I informed myself before choosing				0.749***	0.886**
				(0.081)	(0.051)
It was a complex decision				0.929	0.883*
				(0.059)	(0.063)
I did not know that I could choose				0.940	0.974
				(0.061)	(0.033)
It was an unimportant decision				1.270***	1.104**
				(0.071)	(0.041)
I forgot to decide				0.986	1.005
				(0.073)	(0.038)
The default appeared to be a recommendation				1.114*	1.073**
				(0.056)	(0.032)
I kept the effort low				0.982	0.977
				(0.060)	(0.032)
I actually never made a decision				1.005	0.977
				(0.074)	(0.038)
I did not have enough info about the products				1.055	1.014
				(0.068)	(0.035)
Decision taking, *Base: Alone*					
Together				0.792**	0.889
				(0.114)	(0.074)
Not aware of decision				1.489*	1.113
				(0.234)	(0.125)

Table 3.A.13 continued

	Demo-graphics (1)	Energy literacy (2)	Attitudes (3)	Reasons for choice (4)	Full model (5)
Constant DEFAULT | GREY−	0.713	0.730	1.178	2.370**	1.048
	(0.543)	(0.239)	(0.383)	(0.362)	(0.421)
Constant GREY− | GREY−−	6.092***	7.642***	5.699***	18.731***	3.149**
	(0.555)	(0.266)	(0.572)	(0.467)	(0.469)
AIC	1923.88	1912.62	1877.94	1862.62	1852.33
Observations	1,362	1,362	1,362	1,362	1,362

Note.— *p<0.1; **p<0.05; ***p<0.01

Generalized Ordered Logistic Model; the dependent variable is an ordered outcome, the base category is a match in current contract and active preference. The dependent variable further splits in two categories, a mismatch between DEFAULT and GREY−, and a mismatch between DEFAULT and GREY−−. Additionally, the categories of the dependent variable also include those households who have a mismatch to a greyer contract, but who are not in DEFAULT. The model relaxes the proportional odds assumption. Five models are shown in order to allow for the stepwise interpretability of different sets of coefficients. First, we analyze the influence of demographics only on mismatch. Next, we look into whether energy literacy can predict the mismatch to greyer contracts. In model (3) we assess the impact of attitudes on mismatch to greyer contracts. Last, we assess the impact of reasons for choice mismatches. The final column estimates the complete model. Only fully completed questionnaires are considered. Coefficients are reported as odds-ratios. Standard errors are shown in parentheses

3.A.4 Cover letter and questionnaire

The following pages include a copy of the cover letter and questionnaire that were sent out to the households. Note that the original versions were in German language and have been translated by the authors. The originals can be obtained from the authors on request.

Utility XY
1111 City XY

Telephone +41 (0) 123456
customer.service@utilityxy.ch
www.utilityxy.ch

Address

Date

Be part of it: A study on electricity contract choice

Dear Madam, dear Sir

The Federal Institute of Technology Zurich studies the electricity contract choice of households. Utility XY supports this project and asks for assistance by providing answers to the attached questionnaire. It will take less than 10 minutes to complete it. You have two possibilities to participate:

- Fill in the printed questionnaire enclosed with this letter and send it back via ordinary mail (free reply coupon is attached)
- Fill in the questionnaire on-line:
  http://www.econ.ethz.ch/study.html
  Your participation number: 1000123

Among all fully completed questionnaires, we will **draw three winners**, which will receive three star prizes of a total worth of **1000 Swiss Francs**. Winners will be drawn randomly and contacted in written form.

Within the context of the study, Utility XY will forward data of your current electricity product and consumption to the Federal Institute of Technology Zurich. These data is handled and treated confidentially and anonymously.

Do you have questions concerning the study? Please write or call us.
study@econ.gess.ethz.ch or 000 123 456, Mo–Fr, 10–12 a.m.

Thank you for your participation in this study.

Best regards

Utility XY and Federal Institute of Technology Zurich

ETH
Eidgenössische Technische Hochschule Zürich
Swiss Federal Institute of Technology Zurich

ABC123456

Data privacy is important to us!

Members of the ethical committee of the ETH Zurich may review your originally provided data under strict confidentiality; however, use for commercial reasons is prohibited. The ethical committee of the ETH Zurich has approved this study (EK 2017-N-01).

☑ **Yes, I agree with the terms and conditions of this study and I allow sharing my electricity contract and consumption data with the researchers.**

QUESTIONNAIRE

In the following you see five different electricity products, which differentiate themselves between their relative prices and their sources of production.

If you need to decide right now for one of these five electricity products, which one would you choose?

Please note:

Earlier studies have shown that many humans choose differently in choice situations, which have no direct implications for themselves as opposed to choice situations that pose immediate consequences.

For instance, if people are requested in a survey to imagine that they receive 1000 Swiss Francs and are then asked how much of this money they would be willing to give to another person, people often respond that they are willing to give 500 of the 1,000 Swiss Francs. However, if the same persons actually receive the 1,000 Francs and are asked how much they would possible give to another person, the amount is generally much less. This difference in behavior between hypothetical and actual decision behavior may be explained by people who do not sufficiently think of the consequences of their own decision-making.

Therefore, we ask you to select one of the five electricity products, as if you actually need to select an option.

☒ ***Please select only one option***

☐ **Solar electricity**
Peak: 23,20 Rappen / kWh | Off-peak: 23,20 Rappen / kWh

☐ **Hydropower and a minimum of 10% other renewable energy (e.g., wind, solar, biomass)**
Peak: 12,60 Rappen / kWh | Off-peak: 10,30 Rappen / kWh

☐ **Hydropower and a minimum of 5% other renewable energy (e.g., wind, solar, biomass)**
Peak: 9,10 Rappen / kWh | Off-peak: 6,80 Rappen / kWh

☐ **80% Hydropower, 20% electricity from thermal waste utilization**
Peak: 8,30 Rappen / kWh | Off-peak: 6,00 Rappen / kWh

☐ **Nuclear power and other non-renewable energy**
Peak: 8,10 Rappen / kWh | Off-peak: 5,80 Rappen / kWh

*Peak: Monday-Friday 7am-8pm, Saturday 7am-1pm; Off-peak: all other times

Turn pages please!

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Please estimate: How much electricity (in kilowatt-hours) does a family with two adults and two children consume annually on average? *Hint: A washing machine uses on average one kilowatt-hour per stage of the washing program (60 degrees Celsius).*

☐ up to 1'000	☐ 1'001-2'000	☐ 2'001-3'000	☐ 3'001-4'000	☐ 4'001-5'000	☐ 5'001-6'000	☐ 6'001-7'000

Switzerland produces electricity from different energy sources. What do you suspect to be the share of electricity production...

...of new renewable electricity, like solar, wind, or biomass?	☐ 1-5%	☐ 6-10%	☐ 11-15%	☐ 16-20%	☐ 21-25%
...of hydropower?	☐ 26-35%	☐ 36-45%	☐ 46-55%	☐ 56-65%	☐ 66-75%

Imagine you are asked to explain to another person the difference between green and conventional electricity. How well could you possibly describe this difference?

☐ Poor	☐ Rather poor	☐ Rather good	☐ Very good

How do you decide which electricity contract to select in your household?

☐ I take these decisions on my own.
☐ I take these decisions with somebody else in my household, e.g., with my partner, shared flat...
☐ I do not take these decisions in my household.

Please think of your <u>current product from your utility</u>. How applicable are the following statements to you?

	Does not apply	*Does rather not apply*	*Does rather apply*	*Does apply*
I have well informed myself before I have taken a decision on an electricity contract.	☐	☐	☐	☐
I deemed the selection of products as very complex.	☐	☐	☐	☐
I did not know that I could choose between different contracts.	☐	☐	☐	☐
I deemed the choice as unimportant.	☐	☐	☐	☐
I forgot to take a decision.	☐	☐	☐	☐
I have chosen an ecological electricity product.	☐	☐	☐	☐
I have chosen an inexpensive product.	☐	☐	☐	☐
The automatically offered electricity product of the utility seemed to me to be a recommendation.	☐	☐	☐	☐
I have invested little time and effort for this decision.	☐	☐	☐	☐
I never got around taking an active decision.	☐	☐	☐	☐
I did not have enough information about the products in order to take an active decision.	☐	☐	☐	☐

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How applicable are the following statements to you?

	0 = does not describe me at all										*describes me perfectly = 10*
Taking care of nature and the environment is important to me.	☐ 0	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5	☐ 6	☐ 7	☐ 8	☐ 9	☐ 10
Generally, I find it easy to decide between two opportunities.	☐ 0	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5	☐ 6	☐ 7	☐ 8	☐ 9	☐ 10
I abstain from things today so that I will be able to afford more tomorrow.	☐ 0	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5	☐ 6	☐ 7	☐ 8	☐ 9	☐ 10
As long as I am not convinced otherwise I always assume that people have only the best intentions.	☐ 0	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5	☐ 6	☐ 7	☐ 8	☐ 9	☐ 10
I do not understand why people spend their lifetime fighting for a cause that is not directly beneficial for them.	☐ 0	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5	☐ 6	☐ 7	☐ 8	☐ 9	☐ 10

Natural gas products are often sold in combination with electricity products. How related are these two products for you?

☐ Completely different	☐ Rather different	☐ Rather similar	☐ Very similar

How applicable are the following statements to you, if you need to decide for a natural gas product?

	Does not apply	*Does rather not apply*	*Does rather apply*	*Does apply*
I would buy an environmentally friendly natural gas product.	☐	☐	☐	☐
I would buy an inexpensive natural gas product.	☐	☐	☐	☐

Please answer some last questions concerning your person.
These questions are important to contextualize the results of this study. Please note that we treat this data confidentially and report possible results only in aggregate form. The authors of this study may not relate any data to your personal identity.

How old are you?

years

What is your gender?

☐ Female	☐ Male

What is your... *Multiple answers and abbreviations allowed*

Nationality	First language	Birth country

How many persons live in your household? *Please fill in the corresponding numbers.*

_____ Adults	_____ Children (below the age of 18)

What is your current housing situation? *Please also fill in the number of rooms available.*

☐ Rent _____ number of rooms	☐ Own property _____ number of rooms

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What is your current employment status?

☐ Employed (full-time)	☐ Seeking work
☐ Employed (part-time)	☐ House wife / House husband
☐ Self-employed	☐ Retired
☐ In training / In school	☐ Other

What is your highest level of education?

☐ Compulsory school	☐ Higher education not university
☐ Vocational training	☐ University
☐ A-Levels	☐ Other

What is your monthly income (before taxes) for your complete household (in Swiss Francs)?

☐ up to 4'000	☐ 4'001-6'000	☐ 6'001-8'000	☐ 8'001-10'000	☐ 10'001-12'000	☐ 12'001-14'000	☐ 14'001-16'000	☐ above 16'000	☐ No answer

In politics, we often talk in terms of 'left' or 'right'. Where would you rank yourself in that spectrum?

0 = left *right = 10*

☐ 0	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5	☐ 6	☐ 7	☐ 8	☐ 9	☐ 10

In which zip code area do you live?

☐ Zip code A	☐ Zip code B	☐ Zip code C	☐ Zip code D	☐ Zip code E

You have the opportunity to leave comments or feedback.
Please use the corresponding field.

Please put the filled-out questionnaire into the enclosed envelope and deliver it at the next postbox. You do not need to put a stamp on it.

Thank you for your help and good luck in the prize draw!

4 | 4

Chapter 4

Nudge for Good? Choice Defaults and Negative Spillover Effects*

Abstract

Policy makers increasingly use choice defaults to promote 'good' causes by influencing socially relevant decisions in desirable ways, e.g., to foster retirement savings, charitable giving, or pro-environmental choices. Such default nudges are remarkably successful when judged by their effects on the targeted behaviors in isolation. However, there is scant knowledge about possible spillover effects of defaults on subsequent related choices. Theoretically, such behavioral spillover effects could amplify, eliminate or reverse the initially positive effects of choice defaults. We report the results from a laboratory experiment that explores the subsequent behavioral consequences of nudging people into initial pro-social behavior via the use of choice defaults. We find that pro-social defaults do not impose adverse effects on subsequent pro-social behavior. Our results are promising for policy makers who might worry about unexpected negative consequences caused by choice defaults.

Keywords: defaults, nudge, licensing, consistency, pro-social behavior
JEL Classification: C91, D03, D04

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4.1 Introduction

Choice defaults appear to be very effective nudges for promoting 'good' causes. For instance, defaults strongly impact individual donation behavior (Altmann et al., 2014), promote the uptake of green energy contracts (Ebeling and Lotz, 2015), and help increase retirement savings (Choi et al., 2003; Cronqvist and Thaler, 2004). Thus, even though there is a lively debate on the ethicality of using defaults as nudges (Bovens, 2009; Desai, 2011; Hausman and Welch, 2010; Sunstein, 2015), their effects on different groups in society (Brown et al., 2011; Löfgren et al., 2012), and whether their use corresponds to the criteria of 'libertarian paternalism' (Carroll et al., 2009; Choi et al., 2003; Ghesla, 2017; Keller et al., 2011), the effectiveness of default nudges for promoting 'good' causes has generally been taken for granted.

However, for an accurate assessment of the overall effects of default nudges on desired 'good' behaviors, policy makers should take into account not only the direct impact of default nudges on targeted choices, but also their potential spillover effects on subsequent, related behaviors (see also d'Adda, Capraro, and Tavoni, 2017).[1] In principle, such spillovers could amplify, eliminate or reverse the initially positive effects of choice defaults. For instance, if nudging someone into an increased uptake of green electricity prevents energy-efficient behavior in the future, the net effect of the choice default for promoting pro-environmental behavior may be less positive than when no such spillover occurs. In contrast, for instance, if nudging someone into giving more to charities additionally positively impacts an individual's helping behavior, the effects of the nudge may be reinforcing.

Given that behavioral spillovers may occur across a multiplicity of domains and contexts, we may expect different directions of effects in different situations. The existing empirical literature on sequential decision making and how initial behavior affects subsequent behavior points to three distinct possibilities: (i) individuals may behave 'consistently' and

[1] Note that in this paper we define the term 'spillover' as the effect of an initial behavior on a subsequent related, but not completely identical behavior (Dolan and Galizzi, 2015). This definition separates spillover effects from 'adaptive learning', in which effects of an initial behavior on a subsequent same behavior are considered (Fudenberg and Levine, 1998). Additionally, it is different from rebound effects, i.e., when an intervention triggers a change in the initial behavior, which may then bring along a further change in the same behavior after a given amount of time (Alcott, 2005). Further, it distinguishes itself from the 'backfiring' of a policy on the same initial behavior (Schultz et al., 2007).

in accordance with their prior decisions (e.g., Cialdini et al., 1995; Gneezy, Imas, Brown, Leif, and Norton, 2012) leading to positive spillovers; (ii) they may 'license' themselves into adverse behavior with respect to their prior decisions (e.g., Meritt et al., 2010) leading to negative spillovers; or (iii) they may view related decisions as independent and make sequential decisions in isolation (e.g., similar to when people narrowly bracket choices, see, e.g., Read, Loewenstein, and Rabin, 1999), leading to the absence of spillovers.

In terms of policy making the question of prime concern is: Do choice defaults have adverse effects on subsequent behavior, thus potentially eliminating the initial positive effect (i.e., are there negative spillover effects to be expected)? Answering this question to the positive would shed important doubts on the usefulness of such instruments and would lead to a more critical discussion of their true potential. In contrast, answering it to the negative would provide proponents of using behavioral interventions for promoting societal goals a strong argument for the effectiveness of such instruments. Moreover, an alleged absence of any significant spillover effects would liberate choice architects to focus their attention exclusively on changing targeted behavior without having to worry about subsequent responses.

In this paper we experimentally examine whether and how choice defaults targeted at promoting an initial 'good' behavior affect behavior in an untreated *subsequent* decision. To do so, we use a pro-social choice default on giving to charities as a proxy variable for initial 'good' behavior.[2]

Our experimental design specifically addresses the methodological concern that some of the recent literature on licensing and consistency effects has omitted control conditions, hence being unable to cleanly test for spillover effects by contrasting subsequent behavior across experimental conditions (Mullen and Monin, 2016). In particular, our control strategy ensures that the directions of potential spillover effects can be clearly separated from income effects and utility stemming from altruistic motivations in the initial behavior.

In a first decision, subjects play a modified dictator game with a charitable organization as the recipient. Two treatments apply: one treatment without a pre-set default donation

[2] We acknowledge the view that donation decisions to other persons or organizations may only serve as an approximation for providing a public good in more general terms and that specific context—principally—could influence spillover effects. However, we follow up a rich strand in the experimental economics literature that has made use of decisions to give (to other persons or charities) as a proxy for funding public goods (see for instance Dhami, 2016, chp. 5).

(NO DEFAULT) and one treatment with a pre-set default donation (DEFAULT). In NO DEFAULT subjects actively choose their level of pro-social behavior, that is how much to give to the charity, by typing the desired amount into an entry field. In DEFAULT subjects are defaulted into being fully pro-social and donating the maximum possible amount to the charity. They need to opt out if they want to keep their complete (or parts of the) endowment for themselves. Mirroring relevant real-life applications of choice defaults, the implementation of the default in the experiment is subtle and participants can opt out without significant cost (simply by clicking on a button and entering the desired amount).

In a subsequent second decision, subjects play a second dictator game, in which they are randomly paired with another participant from the same session figuring as the recipient. This second decision represents our main dependent variable, as we want to detect whether behavior induced by a default nudge from the first decision spills over to subsequent decision-making. In addition, we employ two control conditions for assessing individual behavior in the subsequent second decision, which serve as counter-factuals, i.e., what subjects would have done if they had not donated initially. First, we control for potential income effects on subsequent giving; second, we also control for the impact of altruistic utility stemming from the first decision on subsequent giving. Thus, we make sure that we can clearly separate spillover effects in behavior from income effects or altruistic motivations.

We find that the choice default promotes significantly higher mean donation levels to charities in the initial decision, but does not lead to negative spillovers in subsequent pro-social behavior. These results hold when contrasting behavior in the default treatment with the behavior in two control conditions. The absence of negative spillover effects of an initial nudge carries positive messages for policy makers and choice architects who aim at organizing contexts in which people make decisions with choice defaults.

When exploring the drivers of the individual heterogeneity in our results, we find that for the women in our sample the pro-social default actually prohibits negative spillovers. Women in NO DEFAULT give directionally less in the second decision, whereas giving in DEFAULT matches with the control conditions. Men seem to be unaffected by the default, as it does not lead to subsequent adverse behavior even when controlling for income effects and altruistic utility.

The remainder of this paper is organized as follows. Section 4.2 provides the related background on the use of defaults and links this literature with relevant advances in the research of consistency, licensing, and bracketing effects. Section 4.3 details our experimental design, behavioral predictions, and procedures. Section 4.4 presents the results of the experiment. Section 4.5 discusses relevant findings and concludes.

4.2 Related literature

The distinctive presence of a default effect is a robust finding in individual decision making research. Individuals tend to choose an option that is presented as default more often than other alternatives. This effect has been demonstrated in the laboratory and in the field in several domains such as pro-social decision making (Altmann and Falk, 2009; Cappelletti et al., 2014; Carlsson et al., 2015; Johnson and Goldstein, 2003), pro-environmental decisions (Brown et al., 2013; Ebeling and Lotz, 2015; Egebark and Ekstroem, 2016), or retirement savings (Choi et al., 2003; Cronqvist and Thaler, 2004). Choice architects often use defaults in the sense of 'libertarian paternalism' to promote socially desirable behavior without infringing upon decision makers' freedom to choose other alternatives (Thaler and Sunstein, 2003). However, a missing element in the literature on defaults and their behavioral and societal effects are the potential negative spillover effects of initially successful default nudges on subsequent behavior, which could undermine the overall effectiveness of a libertarian paternalistic policy intervention.

In a general context, without specifically considering the effects of default nudges, potential spillover effects of initial on subsequent related behavior have been increasingly studied in the psychological and behavioral economics literature (for overviews see Dolan and Galizzi, 2015; Truelove et al., 2014). This strand of literature documents the existence of two—directionally opposed—possible spillover effects. First, initial behavior can carry over to subsequent behavior via 'consistency' effects (e.g., Cialdini et al., 1995; Conway and Peetz, 2012), as people continue to do more of the same (positive spillover). Second, in contrast, initial pro-social behavior can lead to adverse subsequent behavior because of so-called 'moral licensing' (e.g., Meritt et al., 2010). Thus, when people compensate earlier

decisions in subsequent choices, we can expect negative spillovers of behavioral manipulations of pro-social choices (e.g., Brown, Tamborski, Wang, Barnes, Mumford, Connelly, and Devenport, 2011; Clot, Grolleau, and Ibanzez, 2014; Schmitz, 2016).

There is rich empirical evidence from the laboratory and from the field for both of these two competing outcomes. First, individuals seem to behave 'consistently' through a so-called 'foot-in-the-door effect', i.e., by getting persons to initially agree to a moderate request, which then subsequently lets them agree to a larger request (Beaman, Cole, Preston, Klentz, and Steblay, 1983; Burger, 1999; Freedman and Fraser, 1966). Consistent subsequent behavior may also be induced through 'survey or answering effects', i.e., that specifically framed items in a questionnaire may consequently lead to more positive answers (Fitzsimons and Shiv, 2001), or when rational decisions in a market setting subsequently spill over to a more favorable valuation of non-market goods (Cherry, Crocker, and Shogren, 2003). Evidence from economic experiments shows that subjects' behavior in one game may positively influence behavior in a subsequent game (Grimm and Mengel, 2012; Knez and Camerer, 2000). Additionally, field evidence from towel reuse (Baca-Motes, Brown, Gneezy, Keenan, and Nelson, 2013) or energy conversation (Abrahamse, Steg, Viek, and Rothengatter, 2005; Brandon, Ferraro, List, Metcalfe, Price, and Rundhammer, 2011) also supports the notion of consistent subsequent behavior.

Second, in contrast, individuals have been shown to exhibit licensing behavior in pro-social decision making (Conway and Peetz, 2012; Monin and Miller, 2001), pro-environmental decisions (Harding and Rapson, 2013; Jacobsen, Kotchen, and Vandenbergh, 2010; Tiefenbeck, Staake, Roth, and Sachs, 2013), or health related decision making (Chang and Chiou, 2014; Hennecke and Freund, 2014).

Moreover, there is some literature analyzing when to expect consistency or licensing effects. For instance, the review by Mullen and Monin (2016) suggests that consistency is triggered when people consciously reflect on what the initial behavior means for their own values, whereas licensing may occur if conscious self-reflection is absent. Gneezy et al. (2012) find that the cost of the initial behavior is an important factor for whether consistency or licensing effects occur. They argue that if the initial pro-social behavior is 'costly' to the individual, i.e., if subjects pay a donation to a charity from their own

endowment, it serves as a signal to the self about one's 'moral type' (see, e.g., Bénabou and Tirole, 2011) and leads to consistent subsequent behavior. If the initial behavior is not costly, i.e., subjects just observe that there is money donated to a charity, but this donation does not impede their endowment, there seems to be no signaling effect and we may observe licensing.

The co-existence of licensing and consistency effects across different domains is perplexing. Additionally, often, the reported effect sizes are rather small. For instance, in a meta-analysis of moral licensing studies, Blanken, van den Ven, and Zeelenberg (2015) report small to medium effect sizes (Cohen's d of 0.31). These circumstances complicate the assessment of potential spillover effects due to initial nudging interventions, as researchers may not be able to come up with initial priors in which direction such spillovers might occur and whether these effects are of sizable interest for policy making.[3]

Thus, based on the existing literature on sequential decision making and behavioral spillovers, it remains unclear whether and which spillovers we can expect to see when people are nudged into an initial pro-social behavior via choice defaults. To the best of our knowledge, there are only few studies that cover the consequences of nudges on subsequent behavior in general. The recent paper by d'Adda et al. (2017) is close to our approach as they use a similar design as ours in order to test relevant spillovers of various policy interventions, including a number of typical 'nudges' such as choice defaults and information about social norms on subsequent behavior. They find that traditional policy interventions in the form of monetary incentives or contractual regulation lead to positive spillovers, whereas nudging interventions have no spillover effects. However, with regard to choice defaults their results remain inconclusive, as their manipulation does not produce a significant default effect in the initial behavior. Thus, their data is limited in providing insights on the spillover effects of successful default nudges. In our setting, the default effect is significant and hence allows for inference of the behavioral consequences of successful default nudges for subsequent decision-making.

With regard to the effects of defaults on subsequent choice behavior, de Haan and Linde (2017) present an interesting experimental study demonstrating that well-designed

[3] We will discuss at some length in Section 4.3.3 the choices of experimental design in this study and why these choices may provide a relevant starting point for this type of research.

defaults, i.e., defaults that correspond to people's preferences, can be reinforcing in behavior and may strengthen the default effect in the future. Specifically, people who were presented with such a default in a first choice were more likely to follow a similar choice default again in a second and related choice later on. Our research question differs from de Haan and Linde (2017) with respect to the subsequent behavior in which we are interested. While de Haan and Linde (2017) focus on how the design of choice defaults impacts the strength of default effects over time, we analyze whether a default spills over to an untreated, related behavior when no default is present.

4.3 Experimental design

To study whether a pro-socially set default option in an initial choice affects behavior in an untreated subsequent decision we make use of a 'sequential behavior paradigm' (Mullen and Monin, 2016). This straightforward design allows studying subjects' *subsequent* behavior (dependent variable) in relation to an *initial* behavior (independent variable). For both behaviors we implement dictator games (Forsythe, Horowitz, Savin, and Sefton, 1994; Kahneman et al., 1986).

Specifically, in the initial behavior ('Dictator Stage I') subjects play a dictator game paired with a charity as the recipient. We implement two treatment variations in Dictator Stage I. In the NO DEFAULT treatment, subjects choose actively if and how much to donate to a charity. In the DEFAULT treatment, subjects are nudged into being fully pro-social and donate the maximum possible amount to a charity by default.[4] Additionally, we construct two control conditions, which we will use to separate the influence of income effects and altruistic utility on spillover effects. In the subsequent behavior ('Dictator Stage II') subjects play another dictator game, in which they are now paired with a randomly allotted person from the same laboratory session. Sections 4.3.1 and 4.3.2 specify the details. In Section 4.3.3 we discuss several important aspects of the experimental design.

[4]The nudge is a pre-set donation of a subjects' maximum endowment. Subjects can, however, change the pre-set amount without incurring any cost.

4.3.1 Dictator Stage I

Subjects play a dictator game paired with a recipient in form of a charitable organization. Subjects can choose from nine different charities, which serve different purposes. Specifically, subjects can choose between national and international charities that deal with environmental and nature conservation, human rights, or health related matters. The purpose of this design is to preclude situations in which subjects would like to donate, but do not find a suitable charity to do so (Crumpler and Grossman, 2008). Thus, we aim at enabling a relatively straightforward environment for a donation decision.

Subjects receive information on each charity by reading a statement of purpose.[5] Subjects need to read about each charity, before they are able to make a choice.[6] Once they have read about all charities, subjects first decide whether to donate or not. Naturally, if subjects do not donate at all, they keep all experimental points. However, if they want to donate, they need to decide to which of the nine charities (only one can be selected) they want to donate to. Second, subjects then decide how much of their endowment of 100 experimental points (ECU) to donate to a charity. For this donation decision two treatment conditions apply:

T1 **NO DEFAULT:** Subjects need to actively type the donation into an input box. The box is initially blank.

T2 **DEFAULT:** Subjects' donation is pre-set in an input box. Concretely, by default subjects donate the complete amount of their 100 points to a self-chosen charity. If subjects want to change this amount, they need to actively click on the box and input any other desired amount.[7]

In addition to the two treatment conditions, we construct two control conditions in order to be able to cleanly identify potential spillover effects in the subsequent behavior.

[5] These statements were taken from the website of Zewo Foundation, a Swiss institution that certifies charitable organizations with respect to integrity, efficient use of funds, and transparency, see www.zewo.ch/en/.

[6] Figure 4.A.1 in the appendix displays the decision screen in Dictator Stage I.

[7] This implementation of a default ensures an almost costless nudge, as changing the value is just one click away. Our design is guided by the intentions of libertarian paternalism, which advocates coercion-free decision environments (Thaler and Sunstein, 2003) and it is similar to the default implementation in a recent field experiment on default effects in charitable giving (Altmann et al., 2014).

First, in the CONTROL-Income condition we control for possible income effects stemming from the potentially different donation decisions in Dictator Stage I in DEFAULT and NO DEFAULT.

C1 **CONTROL-Income:** Subjects do not participate in Dictator Stage I, but receive lump-sum payments (denoted as $\hat{X}$) in addition to their show-up fees. The amounts of these lump-sum payments are derived from the distribution of donation amounts of subjects in both treatment conditions in Dictator Stage I. Thus, each donation decision in the treatments is matched with a lump-sum payment a participant receives in the control condition.[8]

Second, in the CONTROL-Passive Giving condition we control for income effects and additional altruistic utility of giving in Dictator Stage I that could affect subsequent giving. It could be argued that the act of giving in Dictator Stage I may yield utility due to altruistic motives (Andreoni, 1989), and this altruistic utility (stemming from the individual knowledge that through the donation to a charity a public good has been promoted) may affect subsequent behavior.

C2 **CONTROL-Passive Giving:** Subjects receive the identical lump-sum payments according to the same procedures as subjects in CONTROL-Income. Yet, they do participate (to a limited extent) in Dictator Stage I. We restrict subjects' participation in this stage to reading about the nine charities and selecting one to which a fixed and predefined amount of points is donated to. The choice of whether a subject wants to donate or not is thus effectively turned off by imposing a pro-social act (see also Gneezy et al., 2012).[9]

[8] To provide an illustrative example, if, for instance, a subject in NO DEFAULT donates 40 experimental points to a charity, then a subject in this control receives 60 (100-40) experimental points in addition to the show-up fee. This ensures that the distributions of income levels between subjects in treatments and control are identical after Dictator Stage I. We further specify the details of the income matching procedure in the appendix.

[9] What this condition does not control for is the warm-glow (Andreoni, 1990) stemming from the donation decision in Dictator Stage I. This is intentional, as it is exactly this warm-glow (i.e., the feeling of having done something good) which may affect subjects' subsequent decisions (Schmitz, 2016). Additionally, as subjects read about the charities and need to pick one, this condition may also control for any possible priming effects of that task on subsequent decision-making. Again, to provide an illustrative example, if, for instance, a subject in DEFAULT donates 60 experimental points to a charity of her choice, then a subject in this control receives 40 experimental points in addition to the show-up fee. Furthermore, the experimenter donates, independently of the subject's endowment, 60 points to the charity chosen by the subject in this control condition.

4.3.2 Dictator Stage II

In Dictator Stage II subjects play a standard dictator game. Each subject is paired randomly with another subject in the same session. Both subjects remain completely anonymous with respect to each other within and after the study. Accordingly, they are not able to influence the other participant's decision. To maximize the number of observations, we use a variant of the strategy method (Selten, 1967) and elicit choices for both roles of the dictator and the recipient respectively. More precisely, each subject decides on the allocation of 200 experimental points between herself and the paired recipient. However, it is common knowledge that only one of the two decisions of each pair of subjects is implemented, and that the computer randomly determines which one. Dictator Stage II is completely identical for subjects in NO DEFAULT, DEFAULT, CONTROL-Income, and CONTROL-Passive Giving.[10]

Table 4.1: Experimental design and parameters

		Dictator Stage I	Dictator Stage II
	Show-up fee	ECU for decision	ECU for decision
T1 NO DEFAULT	100	100	200
T2 DEFAULT	100	100	200
C1 CONTROL-Income	100+$\hat{X}$	–	200
C2 CONTROL-Passive Giving	100+$\hat{X}$	fixed: (100-$\hat{X}$)	200

Note.— Subjects in CONTROL-Income and CONTROL-Passive Giving receive a lump-sum payment $\hat{X}$ in addition to their show-up fee depending on the donation amounts and the distribution thereof in the treatment conditions. In Dictator Stage I only subjects in treatment conditions can donate to charities. Participants in CONTROL-Income do not participate; participants in CONTROL-Passive Giving just observe that a fixed amount based on the donation distributions in the two treatment conditions and independent of their own endowment is given to a charity of their choice. Decisions in Dictator Stage II are the same for all subjects. They are matched in pairs of two subjects and need to decide on the allocation of 200 ECU, however, only one of the two decisions of each subject pair is implemented. 100 ECU $\equiv$ CHF 10. At the time of the experiment CHF 1 $\approx$ USD 1.

Table 4.1 summarizes the experimental design and provides an overview of relevant parameters. Note that the lump-sum payments $\hat{X}$ for the two control conditions are

[10] Engel (2011) reports that multiple tests (including the strategy method) generally decreases giving in Dictator games. While the strategy method could lead to individual diffusion of responsibility, we do not deem this a particular issue for our results as we apply the method in all experimental conditions.

directly added to subjects' show up fees. Subjects in CONTROL-Passive Giving do not decide on the amount of giving to a charity in Dictator Stage I, thus ECU for the decision are fixed. The endowment for Dictator Stage II is identical across treatments and controls.

4.3.3 Discussion of the experimental design and hypotheses

From the preceding literature discussed in Section 4.2, it seems evident that different spillovers likely occur within different contexts. Mullen and Monin (2016, p. 365) argue that the relevance of spillovers varies from "domain specific" behavior, i.e., when doing good yesterday, promotes doing good today, to "global" behavior, i.e., when subsequent choices are only weakly linked to each other. Given this multiplicity of behaviors in different contexts, it remains a challenge for researchers in this field to have definitive priors as to when and why spillovers may likely occur. This circumstance complicates the general design of experiments aimed at examining whether spillovers could be induced due to nudge interventions in an initial behavior.[11] Therefore, we need to acknowledge that there is likely not *the* obvious and straightforward choice of an experimental design in order to answer our research question.

Dictator Stages As a starting point for the motivation of our design, we use the theoretical framework of spillover behavior by Dolan and Galizzi (2015), in which the authors conceptually describe three distinct building blocks, which make it likely for behavioral spillover effects to occur. First, behaviors are executed sequentially and should be different from each other. Difference in behaviors ensures that possible spillover effects can be clearly separated from other effects, such as adaptive learning, which focuses on the influence of performing the same behavior over and over again (Fudenberg and Levine, 1998). Moreover, spillover effects shall also be separated from 'backfiring behavior' (Schultz et al., 2007), i.e., that initial behavior has an effect on a subsequent same behavior.

In this respect, we provide different types of recipients in Dictator Stage I (charitable organizations) and Dictator Stage II (recipient is another peer within the same session). Thus, behaviors are roughly related in the nature of the game, but conceptually differ in

[11] Interestingly, Mullen and Monin (2016) document that none of the surveyed studies in their review actually shares the same pair of domains for initial and subsequent behavior.

the perception of the potential act to give.[12] Additionally, the design choice of having different recipients in both stages is further motivated by the research question itself. Our prime aim is to identify whether a choice default in one behavior, may have spillover effects to another *untreated* behavior. Thus, we assume that once the default is set in place, it seems more interesting to observe behavior in a related, but untreated domain. For instance, if a policy maker introduces a green electricity default in order to increase the uptake of more environmentally-friendly electricity, it seems of less policy relevance to study spillover effects when looking at the same behavior shortly after. Certainly, we acknowledge that assessing the persistence effects of an intervention is per se an interesting research area, however it distinguishes itself from our narrower view of spillover effects on related behaviors.

Second, when studying whether interventions aimed at changing initial behavior have effects on subsequent behavior, Dolan and Galizzi (2015, p. 3, italics in original) argue that both behaviors should be connected to each other "by some underlying *motive*". By motives the authors think of motivations and preferences that could drive both initial and subsequent behavior. Therefore, we deem the choice of two subsequent dictator games suitable, as both experimental institutions aim at measuring individuals' altruistic preferences (Charness and Gneezy, 2008; Engel, 2011; Peysakhovich and Rand, 2016).[13] Of course, the stylized observance of altruistic preferences in dictator games may not generalize to each domain in similar ways. Therefore, it is important to clarify that with this design choice we restrict our assessment to potential spillover effects that may occur due to pro-social behavior. Still, the external validity of giving in dictator games has been documented in the previous literature (Benz and Meier, 2008; Franzen and Pointner, 2013). Thus, there seem to be reasons to believe that our design choice has relevant potential to assess spillover effects in altruistic behavior in general.

[12] Note that 76% of subjects indicate that they did not know any other person present in their experimental session. An additional 20% indicate that they did not know more than one person in the same session. Thus, potential confounds in terms of strategic behavior due to interpersonal relations between subjects may be ruled out.

[13] Researchers who may study the impact of other behavioral nudges on initial behavior, such as for instance social information, may favor designs that are capable of measuring people's cooperative preferences, e.g., with the help of public goods games or prisoners' dilemma setups, see e.g., d'Adda et al. (2017).

Third, and related to the second point, behavior in the initial decision may lead to a behavior in the second decision, which can either be reinforcing, i.e., subjects give more to recipients, because they have already given in the initial stage (acting consistent with their prior decisions), or which may be detrimental, i.e., subjects give less to recipients, because they have already given in the initial stage (show licensing behavior because of their prior decisions). Hence, the experimental design for examining spillover effects allows for both of these opposing directions of potential outcomes.

In sum, our attempt with this experimental design is to provide obvious design choices that fit with a general framework of behavioral spillover effects.

Control conditions In a comprehensive overview of studies assessing spillover effects across different domains, Mullen and Monin (2016) point out that studies often omit control conditions that could help identifying positive or negative spillovers in subsequent behavior. Our design of Dictator Stage I meets these concerns as we construct a two-tiered control strategy in order to stringently assess subsequent behavior in terms of spillover effects, income effects and altruistic utility.

Both of our two treatments conditions, i.e., NO DEFAULT and DEFAULT, could theoretically produce positive or negative spillover effects in the subsequent, untreated Dictator Stage II. Comparing subjects' behavior in NO DEFAULT and DEFAULT in Dictator Stage II can only tell us whether the nudge intervention may induce subsequent different behavior between the two conditions. While this is already informative, if we find an effect of the default on subsequent giving, we may be unable to detect why it occurred. It could be that different income levels after Dictator Stage I affect subsequent giving. It could also be that components of altruistic utility may affect decision-making in Dictator Stage II. In order to control for these effects, we need to design appropriate control conditions of behavior in Dictator Stage II that serve as counter-factuals, i.e., what subjects would have done if they had not donated in Dictator Stage I already. Only with such an experimental setup, we can avoid comparing subsequent behavior without relevant control conditions, a flaw in many designs of spillover studies (Mullen and Monin, 2016).

The design of CONTROL-Income is straightforward. The main aim is that subjects in this control condition arrive with, on average, the same income as subjects who partici-

pated in Dictator Stage I, but without actually making a donation decision. It is worth noting that due to different numbers of participants in treatments and control conditions, we cannot achieve a perfect matching of pairs of subjects for each income category. Therefore, the main aim is to equalize the distribution of income levels across treatments and control conditions.[14] It could be argued that even though we reach balance in the distribution of income levels, there might be other individual characteristics that differ between conditions and which could affect subsequent giving. However, we oppose that claim as subjects are randomly selected into different conditions, which should not foster differences in characteristics.[15]

The design of CONTROL-Passive Giving follows the same design principles in terms of income matching as CONTROL-Income. In addition, it keeps two other central elements of the first decision constant. First, by letting subjects in this control experience the choice of charities in Dictator Game I, we preclude any situations in which the decision environment primes subsequent behavior. Second, by implementing a costless donation, i.e., subjects just observe that a certain amount of ECUs is donated to a charity, we keep altruistic utility (the utility of knowing that money has been donated to a charity) constant (Andreoni, 1989, 1990).

In sum, our experimental design allows for the identification of spillover effects due to the default (by comparing NO DEFAULT and DEFAULT). Further, we may separate spillover from income effects (by comparing treatments with CONTROL-Income).[16] At

[14] We demonstrate in the appendix that the distribution of income levels is the same for treatment and control conditions before Dictator Stage II.

[15] We check for balance in gender, age, IQ-scores, and all personality characteristics we have elicited. We do not find significant differences for these variables in treatment and control conditions. Thus, we conclude that our income matching procedure did not lead to groups that are inherently different in individual characteristics. As an additional support for this claim we have also analyzed our matching procedure with the means of propensity score matching, i.e., assuming that subjects are not randomly assigned to treatment or control conditions. The propensity score matching algorithm matches a treatment subject with a control subject based on equal propensity scores for the covariates in question (i.e., gender, age, IQ-Score, BIG5-characteristics, need for cognition, psychological reactance and regret). This allows to compare behavior in Dictator Stage II for 'twins' of subgroups in treatment and control. We have used several matching methods, like for instance, 'nearest neighbor', i.e., a technique that matches treatment and control in its proximity of the values of the covariates, or 'subclassfication', i.e., a technique that matches treatment and control given that the distribution of levels in covariates is similar for different subclasses. Again, we do not find evidence that our results are driven by different underlying characteristics of subjects in treatment or control conditions.

[16] Given the design of our control conditions, we can also compare subjects' behavior in the two different matching groups in CONTROL-Income, i.e., the subjects who have received lump-sum payments according to the income distribution in NO DEFAULT and DEFAULT respectively, to assess the direct effect of income for subsequent giving.

last, we can identify if potential spillovers are governed by the altruistic utility of giving in Dictator Stage I keeping income constant (by comparing treatments with CONTROL-Passive Giving).

Hypotheses To guide our analysis in Section 4.4 we briefly specify testable hypotheses. In general, in order to assess whether the pro-socially set default induces changes in behavior in Dictator Stage II, we need to make sure that two important assumptions hold. First, given rich preceding evidence (e.g., Andreoni, 1990; Crumpler and Grossman, 2008; Eckel and Grossman, 1996), we assume that subjects act to a certain extent pro-socially. Second, default options have a demonstrably strong influence on individual decision making (see our discussion of default effects in Sections 4.1 and 4.2). Hence, we expect subjects in DEFAULT to donate significantly higher amounts to charities and to stick significantly more often to the exact amount of the default donation than subjects in NO DEFAULT.

As indicated in Section 4.2, experimental and empirical evidence is mixed on the effects of past behavior on subsequent behavior. There is reliable evidence for the existence of positive (e.g., Gneezy et al., 2012) and negative spillovers (e.g., Blanken et al., 2015), but there are also reasons to believe that any kind of spillover is unlikely to occur (e.g., because of narrow bracketing tendencies (Read et al., 1999)). Hence, there exist three competing hypotheses concerning the behavior in Dictator Stage II.

Hypothesis 1A Subjects in DEFAULT give less in Dictator Stage II than subjects in NO DEFAULT (negative spillover).

Hypothesis 1B Subjects in DEFAULT give more in Dictator Stage II than subjects in NO DEFAULT (positive spillover).

Hypothesis 1C Subjects in DEFAULT show no difference in giving Dictator Stage II compared to subjects in NO DEFAULT (no spillover).

We have argued that in terms of policy relevance, rejection of Hypothesis 1A seems to be most relevant for policy makers. If we cannot reject hypotheses 1B and 1C, implications for

policy-makers seem much more positive, as there are no detrimental effects on subsequent behavior to be expected due to the choice default. In addition to the analysis of subjects' behavior in NO DEFAULT and DEFAULT, we can also use our control conditions to further separate a potential spillover effect from income effects or altruistic utility.

In terms of the policy relevance of spillover effects, we may further question the net effect of an intervention after having assessed the direction of the spillover (Truelove et al., 2014). For instance, Jacobsen et al. (2010) find that an informational campaign on carbon emission offsets lead some consumers to increase their electricity consumption (i.e., a negative spillover). However, the total effect of the emission reduction due to offsetting was greater than the additional emissions caused by the increasing electricity consumption. Thus, it seems important to also assess the net effect of interventions to say more about their suitability for sound policy making. In this regard, specifically, if there are negative spillover effects, subjects' total giving in both Dictator Stages provides insights, whether a strong default effect in a first decision (i.e., higher giving to charities in DEFAULT as compared to in NO DEFAULT), could offset negative spillovers of subjects in DEFAULT in Dictator Stage II. Therefore, the second set of hypotheses conditions on whether there is a negative spillover effect, i.e., when we cannot reject Hypothesis 1A.[17]

Hypothesis 2A Subjects' total giving in Dictator Stages I and II is lower in DEFAULT as compared to the total giving of subjects in NO DEFAULT. A pro-social default does not offset subsequent negative spillover (negative net effect).

Hypothesis 2B Subjects' total giving in Dictator Stages I and II is equal or higher in DEFAULT as compared to the total giving of subjects in NO DEFAULT. A pro-social default offsets negative spillover (no or positive net effect).

[17] We do not explicitly state the case where Hypothesis 1B cannot be rejected. Positive spillover plus initial 'good' behavior due the default should always lead to an overall positive net effect of a policy. If we cannot reject Hypothesis 1C, the assessment of the net effect of the policy is—of course—not of relevance.

4.3.4 Procedures

We conducted fourteen sessions with a total of 448 participants at the Decision Science Laboratory (DeSciL) at ETH Zurich in July and September 2016. In order to obtain the amounts and the distribution of the lump-sum payments ($\hat{X}$), we ran four sessions of NO DEFAULT and DEFAULT first. Subsequently, we varied treatment and control conditions between sessions. We scheduled sessions such that treatments and controls were evenly distributed across different times and days.[18] We computerized the experiment using the software z-tree (Fischbacher, 2007) and recruited subjects using hroot (Bock, Baetge, and Nicklisch, 2014). The subject pool consisted of students at the University of Zurich and the Swiss Federal Institute of Technology (ETH) in Zurich. An experimental session lasted roughly 50 minutes. The average payment was approximately CHF 27. In total, we donated CHF 1,088 to nine different charities.[19]

Subjects first completed Dictator Stage I (except in CONTROL-Income). Subsequently, we included a filler task between Dictator Stage I and II. In this task subjects completed a shortened version of an IQ-test after Cattell (1940). The test was divided into two parts, each part lasting for exactly 90 seconds. The basic intention of the filler task is to temporally separate Dictator Stage I and II (such as in Gneezy et al., 2012; Sachdeva, Illiev, and Medin, 2009). The filler task adds to the external validity of the results, as in relevant real life settings, an initial behavior is most likely not followed immediately by a subsequent behavior. Additionally, it limits the potential for demand (Zizzo, 2010) and anchoring effects (see, e.g., d'Adda et al., 2017).[20]

[18] One treatment session was conducted in a within fashion due to unbalanced show up of participants. Results of this single session are not significantly different with respect to the remaining sessions (Kolmogorov-Smirnov tests, $D = 0.14$, $p = .435$ (distribution of giving in Dictator Stage I), $D = 0.27$, $p = .139$ (distribution of giving in Dictator Stage II)).

[19] When checking in for the experimental sessions, subjects were randomly assigned to computer-equipped cubicles. Common rules for the participation in the study were read aloud via a microphone and subjects were requested to fill in a standardized form of consent. Subjects were then informed that they would receive on-screen instructions for each part of the study. It was common knowledge that the study consisted of several parts, but the contents of each part were not revealed before the respective instructions were provided. In order to ensure comprehension, subjects needed to answer control questions before each part. Without completing the control questions correctly, subjects were not able to proceed with the experiment. Questions were answered individually and in private. A translation of instructions can be found in the appendix.

[20] It could be argued that the task may facilitate subjects to view the two decisions as independent and thus inducing the absence of spillovers. However, the filler task lasts a maximum of five minutes during the conduct of the experiment. Hence, if it is the case that distractions, like filler tasks, are sufficient to eliminate potential spillover effects, it is unlikely that such spillovers are actually relevant in real-life decision making where distractions are presumably frequent and the time that passes between potentially linked decisions is likely to be longer.

After the filler task subjects proceeded to Dictator Stage II. Upon completion of these tasks, they received a summary of their final payoffs and were asked to fill in a supplemental questionnaire. This questionnaire focused on standard measures elicited in previous studies on behavioral spillovers, i.e., measures of personality (Donnellan, Oswald, Baird, and Lucas, 2006), need for cognition (Beissert, Köhler, Rempel, and Beierlein, 2014), psychological reactance (Herzberg, 2002), and regret (Schwartz, Ward, Monterosso, Lyubomirsky, White, and Lehman, 2002). Moreover, the questionnaire consisted of questions concerning the implemented default and elicited participants' socio-demographics.

4.4 Results

Due to the between-session randomization of treatments, there are 129 subjects in NO DEFAULT and DEFAULT respectively, 98 subjects in CONTROL-Income, and 92 subjects in CONTROL-Passive Giving.[21] For additional characteristics of the sample see Table 4.A.1 in the appendix. Table 4.2 provides a descriptive analysis of subjects' behavior in Dictator Stage I and II across treatments and controls.

4.4.1 Pro-sociality and default effect

The results in Table 4.2 reveal that overall giving in Dictator Stage I is positive and significantly different from zero (27.44, $t(256) = 17.25$, $p < .001$). Subjects donate roughly one third of their endowment to a charitable organization.[22]

The default effect can be assessed from three different points of view. First, we can compare mean giving between subjects in DEFAULT and NO DEFAULT in Dictator Stage I. Subjects in DEFAULT donate on average 25% more than subjects in NO DEFAULT. Thus, our default manipulation in Dictator Stage I has a significant positive effect on donation levels (34.26 | 27.44, $t(256) = -1.92$, $p = .028$). We report one-sided tests for

[21] We targeted a sample size of 150 subjects for DEFAULT and NO DEFAULT, and respectively 100 subjects per control condition, but ended up with fewer participants because of no-shows.

[22] The treatment conditions do not affect the selection of different charities, Chi^2-tests all p-values $p > .050$. When prompted to state whether a personally important charity was missing, 81% of the subjects answer this question to the negative. Therefore, the provided set of charities does not prohibit donation decisions on behalf of subjects who would like to act pro-socially, but do not find a suitable charity to give to.

Table 4.2: Experimental data

	Giving in ECU			
	N	Dictator Stage I	Dictator Stage II	Σ DG I/II
NO DEFAULT	129	27.44 (25.38)	35.89 (36.80)	63.33 (49.74)
DEFAULT	129	34.26 (31.47)	39.69 (39.80)	73.95 (60.34)
CONTROL-Income (NO DEFAULT matching)	49	–	39.39 (44.32)	–
CONTROL-Income (DEFAULT matching)	49	–	40.20 (40.59)	–
CONTROL-Passive Giving (NO DEFAULT matching)	46	–	34.57 (39.87)	–
CONTROL-Passive Giving (DEFAULT matching)	46	–	43.70 (39.80)	–

Note.— Giving is denoted in experimental points (ECU). Numbers in brackets are standard deviations. The endowment in Dictator Stage I is 100 experimental points; in the Dictator Stage II the endowment is 200 experimental points. Each row shows the main experimental data for each condition and for both Dictator Stages. The data for the two control conditions is split into the respective income matching category, i.e., NO DEFAULT and DEFAULT. Giving in CONTROL-Passive Giving (DEFAULT matching) seems to be upward biased by two subjects who have given more than the fair split of 100 ECU in Dictator Stage II. However, still the difference between the two controls is not significantly different (34.57 | 43.70, $t(90) = -1.06$, $p = .293$).

the default effect as we had a clear and directed ex-ante hypothesis for this effect. For all other effects we had competing hypotheses and thus report two-sided tests.

Moreover, the default also works at the extensive margin. The default induces higher shares of subjects choosing exactly the default amount (=100 ECU) (15/129 | 6/129, proportion test, $p = .034$).

Last, the default does not affect the number of subjects who decide not to give (32/129 (NO DEFAULT) | 32/129 (DEFAULT), proportion test, $p = .500$). However, if we assess only those subjects who decide to give, the default has a significant effect on donation levels as subjects in the DEFAULT condition donate on average 25% more than subjects in NO DEFAULT (45.58 | 36.49, $t(192) = -2.45$, $p = .008$). The default works again at the extensive margin. Subjects who give a positive amount in DEFAULT more often choose exactly the pre-set default amount than subjects in NO DEFAULT (15/97 | 6/97; proportion test, $p = .032$).

4.4.2 Spillover effects

In order to assess the direction of spillover effects we compare giving in Dictator Stage II between the DEFAULT and NO DEFAULT treatments. Table 4.2 shows that subjects in both treatments give about one fifth of their endowment to the paired recipient. In the NO DEFAULT treatment subjects give 35.89 experimental points (18% of their endowment). In the DEFAULT treatment average giving amounts to 39.69 experimental points (20% of a subjects' endowment). The difference of less than 4 experimental points is statistically not significant (39.69 | 35.89, $t(256) = 0.80$, $p = .427$). We summarize this finding as our first result with regards to hypotheses 1A-C.

Result 1 We find no support for spillover effects (negative or positive) induced by a pro-socially set default. **Hypotheses 1A and 1B** are rejected.

Consequently, as there are no negative spillover effects in subsequent giving, the assessment of the net benefit of the intervention seems less relevant. The initial 'good' behavior is not offset by subsequent behavior. The sum of giving in Dictator Stage I and II is not statistically different between DEFAULT and NO DEFAULT (73.95 | 63.33, $t(256) = -1.54$, $p = .124$).

Result 2 We find no support that a default in an initial choice leads to an overall lower giving, thus potentially undermining initial positive effects. Hence, **Hypothesis 2A** is rejected.

Figure 4.1 illustrates these results. Panel A illustrates the statistically significant impact of a default on charitable giving in Dictator Stage I. Panel B shows behavior in the subsequent and untreated Dictator Stage II. There is no significant difference in behavior between DEFAULT and NO DEFAULT.

4.4.3 Control conditions

As we do not observe spillover effects in subjects' subsequent behavior comparing NO DEFAULT and DEFAULT, the potential benefit of our control conditions for the analysis is less pronounced. If there had been a negative or positive spillover in subsequent behavior

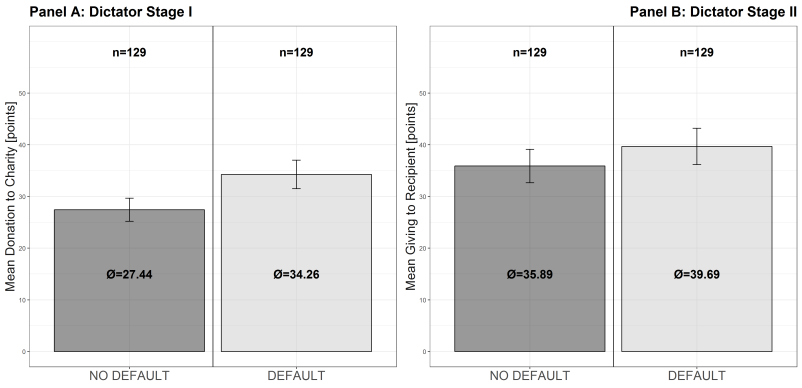

Figure 4.1: Choices in Dictator Stage I and II

Note.— Panel A in Figure 4.1 shows donation decisions (mean points donated to charities) in Dictator Stage I for NO DEFAULT and DEFAULT. Panel B shows benevolent giving (mean points given to recipient) in the Dictator Stage II for the two treatment conditions. Error-bars denote plus/minus one standard error of the mean.

due to the choice default, it would have been straightforward to isolate that effect from the effect of income and altruistic utility.

For now, we can still assess the levels of giving in Dictator Stage II in the treatment conditions with the levels of giving in the control conditions. These analyses lend further support to our main result that no negative or positive spillover seems to occur and that these results are not driven by the level of income remaining after Dictator Stage I, nor driven by potential altruistic utility experienced by the act of giving to charities.

Subjects' choices in NO DEFAULT are not significantly different to those in CONTROL-Income and CONTROL-Passive Giving (NO DEFAULT vs. CONTROL-Income: 35.89 | 39.39, $t(176) = -0.49$, $p = .625$; NO DEFAULT vs. CONTROL-Passive Giving: 35.89 | 34.57, $t(173) = 0.21$, $p = .838$). Likewise, subjects' choices in DEFAULT are not significantly different to those in CONTROL-Income and CONTROL-Passive Giving (DEFAULT vs. CONTROL-Income: 39.69 | 40.20, $t(176) = 0.08$, $p = .939$; DEFAULT vs. CONTROL-Passive Giving: 39.69 | 43.70 $t(173) = 0.57$, $p = .567$).[23]

[23]In each CONTROL condition we had half the subjects matched to the monetary income before Dictator Stage II of a participant in DEFAULT and the other half to the income of a participant in NO DEFAULT. For the t-tests reported above, we only consider these exact matches in order to ensure perfect comparability. In the regressions reported in Table 4.3 we use the full data from the CONTROL conditions when controlling for possible income effects and can thus increase statistical

Table 4.3: Regression models | Giving in Dictator Stage I and II

	Giving DG I	Giving DG II	Giving DG II Yes/No	Giving DG II
	(1) OLS	(2) OLS	(3) Logistic	(4) Gamma GLM
Intercept	27.442^{***}	35.891^{***}	0.658^{***}	3.998^{***}
	(2.235)	(3.242)	(0.186)	(0.065)
DEFAULT	6.822^{*}	3.798	−0.034	0.112
	(3.560)	(4.775)	(0.262)	(0.093)
CONTROL-Income		3.904	0.019	0.097
		(5.360)	(0.283)	(0.099)
CONTROL-Passive Giving		3.239	0.018	0.080
		(5.396)	(0.288)	(0.101)
Observations	258	448	448	295
R^2 / Pseudo R^2	0.014	0.002	0.001	0.004

Note.— $^{*}p < .10$; $^{**}p < .05$; $^{***}p < .01$. Regression models on giving in Dictator Stage I and II; the dependent variable in the first column is giving in DG I, the dependent variable in the second column is giving in DG II (both of these models are estimated with OLS). The latter two columns provide models to account for zero-inflated continuous data. First, we fit a Logistic regression to predict the probability of giving or not giving in DG II. Next, we estimate a Gamma generalized linear model (GLM) to predict the mean of giving for the non-zero data. Estimates in column (3) are on a Logistic scale, estimates in column (4) are on a log-scale. NO DEFAULT is the omitted treatment captured by the intercepts. Wald contrasts testing for differences in giving to the recipient between DEFAULT and CONTROL conditions are not statistically significant. Robust standard errors are in parentheses.

The regressions in Table 4.3 summarize the results of Sections 4.4.1, 4.4.2, and 4.4.3 for the complete sample. Model (1) shows the effect of the default with an OLS regression of giving in Dictator Stage I. Model (2) indicates that there is neither a spillover effect of giving in Dictator Stage II when assessing behavior between NO DEFAULT and DEFAULT, nor are there any spillovers when controlling for income or altruism (the main effects of DEFAULT, CONTROL-Income, and CONTROL-Passive Giving are insignificant). Subjects in DEFAULT do not give more based on their initial level of giving than subjects in NO DEFAULT, nor are there any significant differences compared to the two CONTROL conditions (using post-estimation Wald tests we find no significant differences when comparing the coefficient of DEFAULT with the coefficients of the two control conditions).

Models (3) and (4) account for the inflation of zeros in the continuous data of giving in Dictator Stage II in our data, i.e., 34% of subjects do not give to recipients. Therefore, we

power. Note additionally, that if we compare the two matching groups of CONTROL-Income with each other we can directly test whether different income levels may have an effect on subsequent giving. This does not seem to be the case, as giving in CONTROL-Income (DEFAULT matching) and giving in CONTROL-Income (NO DEFAULT matching) are not significantly different (40.20 | 39.39, $t(96) = -0.09$, $p = .924$). Similarly, comparing matching groups in CONTROL-Passive Giving with giving in Dictator Stage II is not significantly different (34.57 | 43.70, $t(90) = -1.06$, $p = .293$).

first predict the probability of giving or not giving in model (3), before fitting a Gamma generalized linear model (GLM) to predict the mean of giving for the non-zero data (Zuur and Ieno, 2016). This hurdle model adds additional robustness to our results, as the findings are practically the same compared to the simpler specification of model (2).

4.4.4 Exploring individual heterogeneity

The effect sizes of observed spillover effects in the literature are rather small (see meta-analysis by Blanken et al., 2015). Given the different possible directions of spillover effects, potential findings for subgroups in our sample may even out in the average results, thus masking patterns that are important for evaluating the spillover effects of choice defaults. Therefore, we use the available data from our post-experimental questionnaire to explore the extent of individual heterogeneity present in our outcomes.

Table 4.4 shows the results of regressions exploring the effects of individual characteristics for decisions taken in Dictator Stage I and Dictator Stage II. We discuss the results in detail below.

4.4.4.1 Individual characteristics and giving in Dictator Stage I

Model (1) in Table 4.4 reports results from an OLS regression examining the influence of different characteristics of subjects in both treatments on charitable giving in Dictator Stage I. Gender seems to play an important role in the analysis of these results. Therefore, the intercept captures giving decisions for female participants in NO DEFAULT at the sample mean of all other observed characteristics. Women in the DEFAULT condition give significantly more to charities than women in NO DEFAULT (40.97 | 28.75, $t(142) = -2.50$, $p = .007$). Male participants, however, are unaffected by the default. In fact, there is no significant default effect for the male subsample in Dictator Stage I (25.79 | 25.79, $t(112) = 0$, $p = 1.000$).

To further examine why the default is highly significant for women, but does not seem to influence male subjects in our sample, we explore which personality characteristics interact with the DEFAULT treatment.

Model (1) in Table 4.4 shows that subjects in DEFAULT with higher intellect scores and higher IQ-scores (as measured with the IQ-test in the filler task) give less to charities.

Table 4.4: Regression models | Giving in Dictator Stage I and II, exploratory results

	Giving DG I	Giving DG II	Giving DG II Yes/No	Giving DG II
	(1) OLS	(2) OLS	(3) Logistic	(4) Gamma GLM
Intercept	30.236***	34.725***	1.050***	3.788***
	(3.183)	(4.319)	(0.311)	(0.102)
DEFAULT	11.744**	12.542*	0.313	0.328**
	(5.003)	(6.431)	(0.444)	(0.136)
CONTROL-Income		12.274	−0.019	0.404***
		(8.513)	(0.479)	(0.153)
CONTROL-Passive Giving		11.478	0.437	0.254*
		(7.306)	(0.533)	(0.150)
Extraversion	−4.997**	−10.092***	−0.540**	−0.083
	(2.153)	(3.604)	(0.231)	(0.075)
DEFAULT x Extraversion	3.134	5.607	0.409	−0.024
	(3.398)	(5.061)	(0.316)	(0.105)
Agreeableness	1.506	7.352*	0.407*	0.141
	(2.758)	(3.890)	(0.246)	(0.091)
DEFAULT x Agreeableness	−1.098	−9.139*	−0.240	−0.300**
	(4.168)	(5.470)	(0.335)	(0.128)
Conscientiousness	−1.799	−0.919	−0.211	0.030
	(1.871)	(3.082)	(0.207)	(0.080)
DEFAULT x Conscientiousness	−1.705	4.569	0.296	0.060
	(3.551)	(4.830)	(0.301)	(0.114)
Neuroticism	0.236	1.676	−0.259	0.124*
	(2.211)	(3.719)	(0.213)	(0.073)
DEFAULT x Neuroticism	−1.477	1.843	−0.089	0.083
	(4.246)	(5.561)	(0.335)	(0.121)
Intellect	3.355	0.137	0.069	−0.051
	(2.739)	(3.589)	(0.221)	(0.089)
DEFAULT x Intellect	−8.946**	−0.822	−0.127	0.069
	(3.990)	(5.377)	(0.323)	(0.117)
Need for cognition	−1.353	5.350	−0.012	0.227***
	(2.333)	(3.280)	(0.210)	(0.080)
DEFAULT x Need for cognition	−0.173	−8.155	−0.364	−0.199*
	(3.969)	(5.385)	(0.337)	(0.119)
Reactance	1.385	0.269	−0.069	0.038
	(2.292)	(3.823)	(0.234)	(0.082)
DEFAULT x Reactance	−0.562	2.266	0.093	−0.007
	(3.890)	(5.345)	(0.323)	(0.119)
Regret	−1.352	−4.663	−0.133	−0.108
	(2.147)	(3.139)	(0.196)	(0.067)
DEFAULT x Regret	0.655	−4.236	−0.022	−0.022
	(4.141)	(5.057)	(0.310)	(0.106)
IQ	4.810**	4.289	0.103	0.070
	(2.030)	(2.870)	(0.189)	(0.064)
DEFAULT x IQ	−8.124**	−8.132*	−0.074	−0.184*
	(3.740)	(4.564)	(0.285)	(0.098)
Gender=Male	−4.488	4.374	−0.534	0.314*
	(4.599)	(7.710)	(0.453)	(0.162)
DEFAULT x Gender=Male	−11.787	−21.852**	−0.901	−0.368
	(7.885)	(11.139)	(0.652)	(0.229)

Table 4.4 continued

	Giving DG I	Giving DG II	Giving DG II Yes/No	Giving DG II
	(1) OLS	(2) OLS	(3) Logistic	(4) Gamma GLM
CONTROL Interactions	–	Yes	Yes	Yes
Observations	258	448	448	295
R^2 / Pseudo R^2	0.120	0.130	0.132	0.160

Note.— Significance levels: *p<0.1; **p<0.05; ***p<0.01. Regression models on giving in Dictator Stage I and II; the dependent variable in the first column is giving in DG I, the dependent variable in the second column is giving in DG II (both of these models are estimated with OLS). The latter two columns provide models to account for zero-inflated continuous data. First, we fit a Logistic regression to predict the probability of giving or not giving in DG II. Next, we estimate a Gamma GLM to predict the mean of giving for the non-zero data. Estimates in column (3) are on a Logistic scale, estimates in column (4) are on a log-scale. The intercepts denote choices of female subjects in NO DEFAULT at mean levels of continuous variables. Wald contrasts testing for differences in giving to the recipient between DEFAULT and CONTROL conditions are not statistically significant. Robust standard errors are in parentheses.

These findings parallel Altmann and Falk (2009) who find that participants with high cognitive-reflection test scores stay with the default less often.[24]

If we look at the sample of men only, we find significant negative interactions with higher scores on intellect from the BIG-5 inventory and higher scores in the IQ-test with giving in the DEFAULT treatment.[25] Hence, in our sample the men with higher intellect may have realized that they are nudged into pro-social behavior, which subsequently lead to an objection of the default, thus weakening its effect. These findings are somewhat in line with Balafoutas, Kerschbamer, and Sutter (2017) who show that men are more likely to complain than women when being overcharged for a taxi ride.

If we look at the sample of women only, there are no significant interactions with higher scores on intellect from the BIG-5 inventory and higher scores in the IQ-test with giving in the DEFAULT treatment.[26] Hence, based on this observation, we may speculate that women in our sample behave differently when facing a default decision, i.e., they seem to object less to the libertarian paternalistic intervention per se. Other findings in the

[24] Note that we find no significant differences among the distribution of intellect scores for the female and male subsamples across conditions ($Chi^2(39) = 31.79$, $p = .787$ (Women), $Chi^2(27) = 32.16$, $p = .509$ (Men)). The same holds for IQ scores ($Chi^2(27) = 17.03$, $p = .930$ (Women), $Chi^2(27) = 21.99$, $p = .579$ (Men)).

[25] Male subjects in DEFAULT with higher than average scores on the BIG-5 intellect inventory give approximately 11 points less to charities. Male subjects in DEFAULT with higher than average scores in the IQ-test give approximately 8 points less to charities. See column (2) in Table 4.A.2 in the appendix.

[26] See column (1) in Table 4.A.2 in the appendix.

literature seem to confirm this view (e.g., Ghesla, Grieder, and Schubert, 2017, in Chapter 3 of this thesis). These results are also in line with previous research that has documented the existence of gender differences in other regarding preferences (e.g., Croson and Gneezy, 2009; Kamas and Preston, 2015; List, 2003). For instance, women tend to be more inequity averse and give more in standard dictator games. Specifically, Croson and Gneezy (2009, p. 448) points out that "social preferences [of women] are more malleable", an observation that conforms with our data.

4.4.4.2 Individual characteristics and giving in Dictator Stage II

Model (2) in Table 4.4 examines the influence of different characteristics of subjects in all four conditions on giving to a recipient subject in Dictator Stage II.[27] The analysis reveals an interesting set of results for participants' sex. First, we observe that women in DEFAULT give significantly higher amounts to recipients in Dictator Stage II than women in NO DEFAULT (48.62 | 35.42, $t(142) = -2.18$, $p = .031$). Hence, without controlling for income or altruism, a default in Dictator Stage I produces a positive spillover on giving in Dictator Stage II, as women in DEFAULT give on average 37% more than women in NO DEFAULT. Men, however, share statistically equal amounts of points with their recipients in the two treatments (28.42 | 36.49, $t(112) = 1.09$, $p = .280$).

Using our control conditions, we further assess the 'true' direction of the spillover effect for women in our sample.[28] Estimates of giving in Dictator Stage II in model (2) indicate that women in NO DEFAULT give less than women in the control conditions. However, the differences are only directional and not statistically significant.

Models (3) and (4) account again for the inflation of zeros in the continuous data of giving in Dictator Stage II in our data. The Gamma GLM reported in column (4) complements the findings of model (2) and provides evidence that women who give a positive amount in Dictator Stage II in NO DEFAULT give significantly less than women in the two

[27] Note that each of the two control conditions is interacted with the characteristics as well (CONTROL Interactions = Yes), however none of the interactions yield significant results.

[28] By design, our control conditions were initially not intended to grant such a comparison, as the matching of income does not respect differences in gender per se. Therefore, we compare the income distributions before Dictator Stage II for women and men in treatments and controls and check for their equality. We find no evidence that these distributions differ significantly from each other. Kolmogorov-Smirnov tests, all $p - values > 0.05$. Please refer to Table 4.A.3 for detailed results of the Kolmogorov-Smirnov tests and details on the matching procedure.

control conditions (as evidenced by the significant positive coefficients for the two control conditions in model (4).). Thus, these outcomes point to moral licensing on behalf of this subgroup in our data. However, their giving in DEFAULT is not significantly different from the two control conditions. Hence, we argue that the default seems to avoid moral licensing tendencies for women who give a positive amount in Dictator Stage II. For the men in our sample no such effect can be observed.

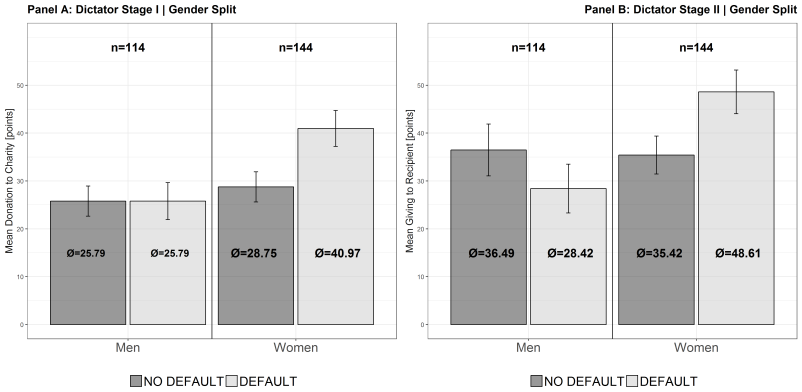

Figure 4.2: Choices in Dictator Stage I and II | Gender split

Note.— Panel A in Figure 4.2 shows donation decisions (mean points donated to charities) in Dictator Stage I for NO DEFAULT and DEFAULT for the subsamples of women and men. Panel B shows benevolent giving (mean points given to recipient) in Dictator Stage II for NO DEFAULT and DEFAULT for the subsamples of women and men. Error-bars denote plus/minus one standard error of the mean.

Figure 4.2 presents a graphical illustration of these results for the gender differences in our sample. Panel A shows the effect of the default on giving in Dictator Stage I between women and men. Panel B shows the behavior in Dictator Stage II between women and men. Note that the control conditions are not depicted in Panel B.

4.5 Discussion and conclusions

This study assesses how choice defaults affect subsequent behavior. We examine pro-social behavior with and without a default and test for spillover effects on subsequent decision making. Consecutively, we contrast our results to specifically designed control conditions

checking for countervailing effects of different income levels and altruistic utility stemming from the initial behavior. Additionally, we elicit participants' individual characteristics in terms of personality (BIG5-characteristics, need for cognition, psychological reactance, regret aversion and IQ-score) and socio-demographics in order to detect relevant discrepancies in behavior for different subgroups in our sample.

We find that although the default has a significant and positive effect on pro-social giving in the initial behavior, there are no positive or negative effects on subsequent behavior. The result holds when controlling for income and for altruism respectively. Thus, a pro-socially set default does not affect subsequent pro-social decision-making in our setting. This could be interpreted as an encouraging finding for policy makers who want to stimulate pro-social behavior via defaults, but fearing subsequent adverse effects.

Moreover, additional interesting results emerge when exploring the influence of individual heterogeneity. We find that gender seems to play a decisive role in our experiment. Women in NO DEFAULT give directionally less in the second decision, while giving of women in DEFAULT matches with giving in the control conditions. It seems that the default prohibits licensing. For the group of women who give positive amounts in the second decision, these findings are even more pronounced. Hence, in conclusion, none of our findings indicate subsequent licensing when applying a pro-socially set default in an initial decision.

Many topics remain to be understood. In our experimental design the default is implemented as a subtle nudge with almost non-existing transaction costs to opt-out. While such a design adheres to the guiding principles of libertarian paternalism, future research may look at whether varying the position of the default or increasing the cost to opt-out could lead to different results in subsequent pro-social behavior.

Arguably, subsequent behavior may be due and exposed to a large variety of contextual factors. The concept of spillover effects in general remains ambiguous in the current literature. In particular, the setup of sequential behavior paradigms may differ between contexts, and a relevant dependency of initial and subsequent behavior may be defined diversely within different research areas. Mullen and Monin (2016, p. 361) even characterize relevance of behavior in a spectrum between "domain specific" and "global", thus essen-

tially allowing for almost any behavior to be related. Evidently, this circumstance proves to be challenging for producing universally valid statements. We have discussed at some length the strengths and limitations of our design towards its generalization, and thus encourage further research in the laboratory and in the field to stringently test whether and under which conditions nudges could have adverse effects on subsequent behavior.

This study is a first step into the analysis of the use of pro-social defaults and their effects on subsequent pro-social behavior. Pinning down the interplay of other contextual factors remains a grand task for coming experimental and empirical research. So far, we have started to enhance our understanding of the behavioral mechanisms at play in subsequent behavior when initially using well-intended defaults. Presently, we have no substantiated reasons to believe that pro-socially set defaults impose any undesirable effects on subsequent pro-social decision-making.

Appendix

4.A Appendix

4.A.1 Sample characteristics: Full sample | Women | Men

Table 4.A.1: Sample characteristics

	NO DEFAULT	DEFAULT	CONTROL-Income	CONTROL-Passive Giving
N	129 | 72 | 57	129 | 72 | 57	98 | 58 | 40	92 | 43 | 49
Age	23.1 | 23.6 | 22.4	24.0 | 23.2 | 25.0	22.8 | 22.7 | 22.9	22.6 | 21.7 | 23.3
Income	84% | 79% | 89%	82% | 81% | 84%	85% | 81% | 90%	88% | 86% | 90%
Education	92% | 92% | 93%	95% | 97% | 93%	96% | 98% | 93%	95% | 93% | 96%
Extraversion	3.25 | 3.32 | 3.15	3.06 | 3.23 | 2.84	3.17 | 3.13 | 3.23	3.20 | 3.08 | 3.31
Agreeableness	4.12 | 4.33 | 3.85	4.09 | 4.29 | 3.85	4.16 | 4.36 | 3.89	3.96 | 4.13 | 3.82
Conscientiousness	3.56 | 3.64 | 3.46	3.59 | 3.73 | 3.40	3.65 | 3.67 | 3.63	3.69 | 3.74 | 3.63
Neuroticism	2.83 | 3.64 | 3.46	2.57 | 3.73 | 3.40	2.79 | 3.67 | 3.63	2.69 | 3.74 | 3.64
Intellect	3.73 | 3.64 | 3.89	3.88 | 3.73 | 3.97	3.86 | 3.67 | 4.10	3.71 | 3.74 | 3.78
Need for cognition	3.58 | 3.53 | 3.65	3.66 | 3.58 | 3.77	3.54 | 3.48 | 3.61	3.43 | 3.41 | 3.45
Reactance	3.04 | 3.06 | 3.02	2.89 | 2.88 | 2.90	2.86 | 2.91 | 2.80	2.89 | 2.89 | 2.89
Regret	3.37 | 3.31 | 3.44	3.21 | 3.11 | 3.35	3.33 | 3.22 | 3.48	3.35 | 3.41 | 3.30
IQ	8.23 | 8.11 | 8.39	8.37 | 8.51 | 8.19	8.57 | 8.41 | 8.80	8.32 | 8.23 | 8.39

Note.— Sample characteristics are shown for the four conditions NO DEFAULT, DEFAULT, CONTROL-Income, and CONTROL-Passive Giving. The first sub-column indicates data for the full sample, the subsequent for the women in the sample, and the last column for the men in the sample. Age is depicted as the mean, with a range between 18 and 63 years. Income denotes the share of subjects with a monthly income below CHF 2,000. Education denotes the share of subjects with A-levels or higher. Extraversion, Agreeableness, Conscientiousness, Neuroticism, and Intellect are the five elements of the BIG5-inventory and are elicited on a 5-point Likert-scale. Scores are denoted as means. Need for cognition is denoted on a 5-point Likert-scale. Reactance is also a 5-point Likert-scale on the Psychological Reactance Scale. The same metric applies to Regret. IQ is measured with 16 items belonging to the IQ-test after Cattell (1940). IQ is given as a mean score. Contingency tests performed for the complete sample, and for the samples of women and men show no signs of significant differences in characteristics across treatment and control conditions.

4.A.2 Regressions: Giving in DG I and DG II | Women | Men

Table 4.A.2: Regression models | Giving in Dictator Stage I and II, separate results: women and men

	Giving DG I (1) Women	Giving DG I (2) Men	Giving DG II (3) Women	Giving DG II (4) Men
Intercept	28.889***	26.220***	36.496***	36.546***
	(3.037)	(3.047)	(3.995)	(5.284)
DEFAULT	13.140**	2.362	11.253*	−4.209
	(5.116)	(4.983)	(6.145)	(8.038)
CONTROL-Income			10.458	−3.521
			(7.524)	(9.540)
CONTROL-Passive Giving			13.267*	−4.570
			(6.866)	(9.122)
Extraversion	−5.917*	−2.938	−7.124*	−14.973**
	(3.082)	(2.838)	(4.156)	(6.319)
DEFAULT x Extraversion	1.504	8.728*	−4.382	21.360**
	(4.559)	(4.976)	(6.637)	(9.060)
Agreeableness	4.804	−2.065	7.203*	9.077
	(3.549)	(3.620)	(4.275)	(6.415)
DEFAULT x Agreeableness	0.327	−8.499	−11.722*	−14.394
	(5.016)	(5.721)	(6.955)	(8.770)
Conscientiousness	−5.795**	2.659	−6.108	10.132**
	(2.442)	(2.762)	(4.044)	(5.133)
DEFAULT x Conscientiousness	−2.778	4.170	5.718	−2.710
	(5.511)	(4.787)	(6.929)	(7.548)
Neuroticism	−0.564	3.022	−2.204	5.611
	(2.946)	(3.095)	(4.420)	(6.985)
DEFAULT x Neuroticism	−5.608	9.673	4.003	2.732
	(5.757)	(6.143)	(7.518)	(9.287)
Intellect	0.151	6.874*	1.832	−2.408
	(3.639)	(3.586)	(4.574)	(5.732)
DEFAULT x Intellect	−7.908	−11.024*	−4.033	2.418
	(5.252)	(5.673)	(6.942)	(8.445)
Need for cognition	−3.637	0.941	4.603	4.061
	(3.234)	(3.715)	(4.036)	(6.721)
DEFAULT x Need for cognition	7.198	−3.713	−3.523	−9.289
	(6.104)	(5.720)	(6.860)	(9.301)
Reactance	2.135	0.815	−0.408	3.990
	(3.256)	(3.247)	(4.417)	(7.484)
DEFAULT x Reactance	−2.741	0.300	5.172	−6.491
	(5.764)	(4.944)	(7.069)	(8.888)
Regret	−2.234	−2.842	−3.781	−5.937
	(3.129)	(2.919)	(4.852)	(5.423)
DEFAULT x Regret	6.155	−5.491	−9.447	0.581
	(6.398)	(4.950)	(7.549)	(8.302)
IQ	3.995	3.198	3.187	2.908
	(2.912)	(2.953)	(3.670)	(5.103)
DEFAULT x IQ	−7.023	−8.492*	−2.750	−10.574
	(5.427)	(4.691)	(5.952)	(6.827)

Table 4.A.2 continued

	Giving DG I (1) Women	Giving DG I (2) Men	Giving DG II (3) Women	Giving DG II (4) Men
CONTROL Interactions	–	–	Yes	Yes
Observations	144	114	245	203
R^2	0.213	0.237	0.200	0.161

Note.— Significance levels: *p<0.1; **p<0.05; ***p<0.01. Regression models on giving in Dictator Stage I and II; the dependent variable in the first two columns is giving in DG I, the dependent variable in the second two columns is giving in DG II (the models are estimated with OLS). Models separate the analysis of Table 4.4 in results for women and men. The intercepts denote subjects' choices in NO DEFAULT at mean levels of continuous variables. Robust standard errors are in parentheses.

4.A.3 Matching procedure of income: control conditions

We apply the same income matching to both control conditions, CONTROL-Income and CONTROL-Passive Giving. The main principle is to make sure that subjects in treatments and controls arrive in Dictator Stage II with, on average, the same distributions of income levels.

We first elicit some sessions of the treatment conditions, NO DEFAULT and DEFAULT, in order to gain relevant distributions of income levels after Dictator Stage I. We map these distributions to subjects' participation fees in the two control conditions. Specifically, subjects in CONTROL-Income and CONTROL-Passive Giving receive a lump-sum payment (denoted $\hat{X}$) on top of their participation fee that matches with donation decisions of subjects in NO DEFAULT and DEFAULT. For instance, if a subject in DEFAULT decides to donate 40 points to any charity, its remaining income is 60 (100-40) points. This means that this subject arrives with 160 points (including the participation fee of 100 ECU) in Dictator Stage II. Likewise, a subject in CONTROL-Income/CONTROL-Passive Giving receives an additional 60 points to its participation fee, before being directed to Dictator Stage II.

As we have more subjects in NO DEFAULT and DEFAULT than in either control condition, income matching cannot be executed perfectly. However, the aim of the procedure is to ensure that the distribution of incomes after Dictator Stage I do not differ significantly between treatment and control conditions. Please note that in the main text (see footnote 15) we also discuss the robustness of this procedure in terms of whether other covariates may significantly vary across treatment and control with the means of propensity score matching. However, our findings do provide evidence that this is not the case.

Therefore, our control conditions should provide stable control conditions for the comparison of behavior in Dictator Stage II. Table 4.A.3 shows test statistics for the two-sample Kolmogorov-Smirnov-tests for differences in income distributions after Dictator Stage I. The null hypothesis is that the distributions are equal and that the test statistic D is not statistically different from zero.

Table 4.A.3: Kolmogorov-Smirnov test statistics

Distribution Comparison	D	p-value
Complete sample		
NO DEFAULT | CONTROL-Income	0.068	0.957
NO DEFAULT | CONTROL-Passive Giving	0.078	0.899
DEFAULT | CONTROL-Income	0.064	0.978
DEFAULT | CONTROL-Passive Giving	0.054	0.998
Women		
NO DEFAULT | CONTROL-Income	0.071	0.999
NO DEFAULT | CONTROL-Passive Giving	0.095	0.935
DEFAULT | CONTROL-Income	0.142	0.538
DEFAULT | CONTROL-Passive Giving	0.216	0.160
Men		
NO DEFAULT | CONTROL-Income	0.064	0.999
NO DEFAULT | CONTROL-Passive Giving	0.142	0.661
DEFAULT | CONTROL-Income	0.114	0.919
DEFAULT | CONTROL-Passive Giving	0.146	0.621

Note.— Test statistics (D) and p-value for Kolmogorov-Smirnov tests for comparison of income distribution equality among treatment and control conditions. Low values of D suggest that distributions of income do not differ between the corresponding groups. P-values below conventional levels would lead to the rejection of the hypothesis that the underlying distributions are equal.

4.A.4 Experimental instructions

Note: This set of translated instructions was used for respondents in NO DEFAULT and DEFAULT. Differences in DEFAULT are highlighted in grey color. In CONTROL-Income Dictator Stage I was omitted and participants solely received information about their endowment (= participation fee). The remainder of the experimental instructions was identical to NO DEFAULT/DEFAULT. In CONTROL-Passive Giving participants again received information about their endowment (= participation fee). Subsequently, they were presented with the same instructions as in Dictator Stage I. However, they were told that independent of their income an amount between 0 and 100 points would be donated to charity of their choice.[29] *Hence, they could read all the information about the charities and pick one to which the money was donated. Participants were also able to let the computer decide randomly on the choice of a charity. The amount of the donations could not be influenced by the subjects. Subsequent to their decision of choosing a charity, they received feedback about the amount of points that was donated. Afterwards the instructions were identical to those in NO DEFAULT/DEFAULT.*

General Explanations for Participants

Welcome to the experimental laboratory. Today you are taking part in a scientific study, in which you can earn a certain amount of money, which will be handed to you in cash. How much money you earn, is dependent on your decisions and the decisions of other participants. Therefore, please read these instructions carefully.

The set of instructions is for your private use only. Please do not communicate with other participants during the experiment. If you have questions, give a hand signal and the experimenter will come to your desk to answer your questions. Non-observance of this rule will lead to the exclusion of the experiment. During the experiment you will receive information on your computer screen.[30] You take your decisions with keyboard and mouse. Your inputs are completely anonymous. The experimenter knows your identity, however we are not able to relate your decisions with your identity.

Please only use the buttons within the experimental window. With the button 'Continue' and respectively 'Back' you are able to change between the next and the previous page (if possible).

This study consists of **five parts**, in which you receive information and need to make decisions, which may influence your payoff. Your payoff will be calculated in points and converted according to the following rule:

10 Points = 1 Swiss Franc

How much you can earn in each of the parts will be stated in the instructions, which will be shown for each part separately on the screen. At the end of the study, the points you have earned will be converted to Swiss Francs and paid out in cash to you.

The study ends with a short questionnaire. As soon as each participant has completed this questionnaire, the pay-out will be started. You will be called for pay-out by your seat number. Expected processing time for the study is between 45 to 60 minutes.

[29] This procedure follows the instructions by Gneezy et al. (2012) for a 'costless' donation.

[30] Note that these instructions are supplemented with figures from the actual program, as we did not use paper instructions.

Dictator Stage I

In this part of the study you receive **200 points**. 100 points thereof are your participation premium, which you can keep with certainty. The other 100 points are at your disposal for your decision in this part of the study. You can thereby decide how to allocate these 100 points (in increments of 10 points) between yourself and a charity. You can keep all points for yourself and give no points to a charity; you can devote all points to a charity and keep no points for yourself; or you can keep a certain amount of points for yourself and pass the remaining points to a charity. The amount of your donation can be specified with in the input field 'Ihre Spende [in Punkten]' (Your Donation [in Points]).[31]

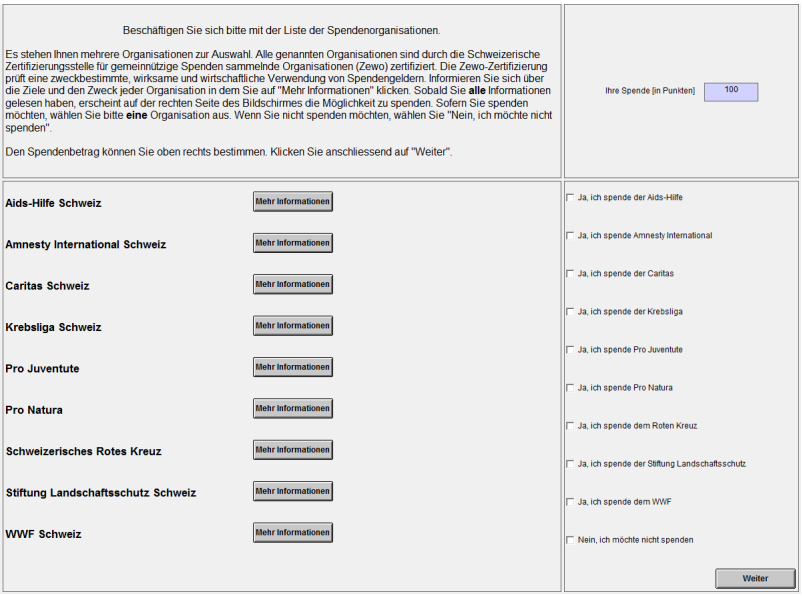

Figure 4.A.1: Sample screen of a decision task in Dictator Stage I.

Note.— The default is simply implemented by pre-specifying the input field to '100'. In NO DEFAULT this field initially remains blank.

There are **nine charities** available for selection, which will be described on the left-hand side of the screen. All charities are certified by the 'Swiss Zewo Foundation'. The 'Zewo Foundation' testifies a purposive, effective and economic use of donation money. Inform yourself on the goals and purpose of each charity by clicking on 'Mehr Informationen' (More Information). The button 'Mehr Informationen' changes its color from red to grey, once you have read the information about a charity.

As soon as you have read the **complete** set of information about each charity, you will be able to select a charity to donate to on the right hand side of the screen. In case that you

[31] See Figure 4.A.1 for a decision screen.

want to allocate points between yourself and a charity, please select **one** charity. You can only choose one charity to donate to. With clicking on "Weiter" (Continue) you donate 100 points to a selected charity. You can specify a different donation amount in the upper right corner. If you do not want to allocate points between yourself and a charity, please choose "Nein, ich möchte nicht spenden." (No, I do not want to donate.)

Please note that points, which you keep for yourself, will be paid out in cash at the end of the study. Points, which you allocate to a charity will be donated by the experimenter to the chosen charity. If you donate, you will receive an official letter by the Chair of Economics at ETH Zurich with your pay-out that the chosen amount will be transfered to the corresponding charity. In order to familiarize yourself with the decision task, please answer the following questions: Person A donates 40 points to a charity.

(1) How many points will person A receive at the end of the study with this decision?
(2) How many points will the charity receive at the end of the study with this decision?

Filler task: Shortened IQ-test after Cattell (1940)

Note: The IQ-test was divided into two parts, which share exactly the same instructions. In each part, subjects had to identify a subset of eight figures. Exemplarily, we show a figure of each subset.

Section 2 consists of a shortened version of an intelligence test. The tests is divided into two parts. For each part you receive further information. The figure shown below (see Figure 4.A.2 and 4.A.3 for an example in each part) gives you an example of the exercise you have to solve in part one (or two). You have to decide which of the squares on the right hand side follows logically the squares on the left (fits logically into the larger square on the left). You make your choice by clicking on the button below the squares. In this example you should choose 'c' ('b'), because the circles in the squares get smaller from square to square (because it fits exactly with the smaller upper right square).

The test starts as soon as you click the button 'Start'. You have 1 minute and 30 seconds to answer each part. Probably, the amount of time allowed is not sufficient to answer all questions. Do not let yourself discourage by this. Simply work as correctly and as fast as possible.

Figure 4.A.2: Sample exercise in part one of the IQ-test

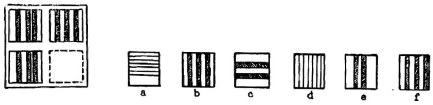

Figure 4.A.3: Sample exercise in part two of the IQ-test

Dictator Stage II

In this section of the study you have to decide on the distribution of **200 points** between yourself and a randomly allocated person in this room. This allocation is anonymous. Neither you nor the other person gets to know your mutual identities during or after the study. You decide in anonymity. Your own decision can not be influenced by the other person.

You have to decide how many points you want to give to the other person. You have 200 points for your decisions. You can keep all points for yourself and give no points to the other person; you can give all points to the other person and keep no points for yourself; or you can keep a certain amount of points (in increments of 10 points) and pass the remaining points to the other person. You can specify the number of points you want to give to the other person in the designated input field.

The other, randomly allocated, person has the same decision task as you and needs to decide how many of the 200 points she or he wants to give to you. **However, only one of these two decisions will be implemented, i.e., the 200 points will be distributed among you and the other person only once.** Which of these two decisions is relevant will be determined randomly by the computer. If the computer (with a probability of 0.5) randomly determines that your decision will be implemented, the other participant will receive the points that you have decided to give to her or him. If the computer (with a probability of 0.5) randomly determines that the decision of the other participant is implemented, you will receive the points that the other participant has decided to give to you. As you are unable to determine whether the computer selects your or the other person's decision, you should carefully consider the decision task.

If your decision is implemented, you will receive the points, which you have kept for yourself and these points will be paid out in cash at the end of the study. If the decision of the other person is implemented, you will receive the points that the other person has given to you, and the other person keeps the remaining points.

In order to familiarize yourself with the decision task, please answer the following questions.

Person A gives 70 points to person B. Person B gives 10 points to person A. The computer implements the decision of person B.

(1) How many points will person A receive at the end of the study with this decision?
(2) How many points will person B receive at the end of the study with this decision?

Note: After this stage subjects receive information on their pay-offs in Dictator Stage I and II. The final sum for pay-out is displayed in points and Swiss Francs. Subjects then answer a questionnaire to complete the study.

Chapter 5

Conclusion

The use of libertarian-paternalistic instruments in public policy has seen a continuous growth in recent years. Insights from behavioral economics have inspired policy makers in several domains, such as tax compliance, financial regulation, public health, or environmental and resource conservation to apply behaviorally-oriented regulations (for an overview see Chetty, 2015; Lunn, 2014; Madrian, 2014; Sousa Lorenco et al., 2016). Particularly, the powerful effects of choice defaults on decision making, i.e., that individuals often stick to a default option, have been demonstrated in several areas, ranging from increasing retirement savings, eating healthier, augmenting organ donor rates or using more environmentally friendly energy sources.

The use of choice defaults in public policy aims at promoting 'good' behavior. The case of green electricity defaults presented in this thesis does not make an exception to this imperative. Letting households switch to more environmentally friendly electricity contracts by default stimulates demand for green electricity, and therefore contributes to mitigation efforts with respect to global warming. However, the use of behavioral interventions is said to also improve individuals' decision making. Green electricity defaults enable win-win situations, contributing to a public good and enhancing individual welfare by 'optimizing' individual choice behavior. The latter argument is the central starting point of this thesis, as it remains debatable and unclear whether a choice architecture involving green defaults is always and unequivocally beneficial for individuals.

Therefore, this thesis set out to deepen the understanding of the implications of green electricity defaults. I sought to answer several research questions. The goal was, first, to

experimentally test whether choices for an electricity contract under a green electricity default regime match or mis-mismatch with individual preferences in an active choice.

> **Research Question 1.A:** Do currently applied green electricity defaults match individual preferences in residential electricity markets?
>
> **Research Question 1.B:** What are the implications for the design and the application of defaults when relative prices for green electricity increase?

Second, the analysis was extended to a field setting in order to identify potential costs and benefits of green electricity defaults for different groups in society.

> **Research Question 2.A:** Why do people stay with green electricity defaults?
>
> **Research Question 2.B:** Does a default distort choices of certain groups in society differently?
>
> **Research Question 2.C:** What are the effects on individual welfare?

Third, the investigation looked into potential spillover effects on subsequent behavior when using behavioral interventions in a specific area.

> **Research Question 3:** Do pro-socially set choice defaults stipulate subsequent beneficial or adverse behavior?

The assessment of my research questions aims at providing a broader and deeper understanding of several elements in behaviorally-oriented policy making in residential electricity markets that have not been fully accounted for in the preceding academic discourse.

To derive these answers, I use, throughout this thesis, an empirical approach of collecting revealed preference data in tightly controlled economic laboratory settings. Additionally, I enrich these findings with stated preference data from the field, which in total provides, in what I believe, a balanced set of results to the questions at hand.

Section 5.1 summarizes the answers to the aforementioned questions and lays out how they may contribute to and complement the existing literature in behavioral environmental economics. I detail several relevant insights for policy in Section 5.2. Section 5.3 outlines potential pathways for future research, which not only address several limitations of this thesis, but which may encourage researchers in this field to build their own work on.

5.1 Findings and contribution to research

The contribution of this thesis is threefold. First, I assessed whether green electricity defaults correspond with their libertarian paternalistic intentions comparing individual behavior under the presence and absence of a default option. Second, I provided novel empirical evidence that leads to a better understanding of the distributional costs and benefits of behaviorally-oriented interventions for different groups in society. Third, I adopted a broader view on the implications of pro-socially set choice defaults by highlighting their influence on subsequent decision making, thus providing insights for the emerging literature on spillover effects in behavioral environmental economics.

In particular, in assessing Research Question 1.A, I provided an experimental design that tries to scrutinize preferences for green electricity in absence and presence of a default option. I concluded that, resting on the central assumption that individuals in a choice frame without a default nudge perfectly reveal their preferences, choices in a green electricity default regime do not unequivocally correspond to people's actual preferences. Hence, the implicitly assumed win-win situation of libertarian paternalistic interventions may not be granted. In addition, I showed that the match between actively revealed preferences and preferences induced by a default may differ depending on the relative price for green electricity (Research Question 1.B). In line with previous research in the domain of pension scheme defaults, my results suggest that if choice defaults are inevitable in a market, setting them on a more costly contract may improve the match with actively revealed preferences, as individuals opt out more often and select options corresponding to their 'true' preferences, while still attaining considerable contributions for new renewable electricity capacity.

Next, in addressing Research Questions 2.A, 2.B, and 2.C, I analyzed the implications of green electricity defaults on costs and benefits for different groups in society in a field study with a representative data set of households of a Swiss city. As I was unable to employ a revealed choice frame in this field study, I made use of and acknowledged the advances in stated preference survey techniques. Specifically, I implemented a renowned experimental protocol that allows for a coherent elicitation of hypothetical preferences towards green electricity. The approach enabled me to start filling the void in the current

literature on the distributional consequences of green electricity defaults. I found that a green electricity default can distort choices in essentially two directions and that these distortions have substantial negative consequences for consumer welfare. In this study a moderately green and moderately expensive electricity default was set. This default prevents, on the one hand, choices to even greener contracts on behalf of households who have pro-environmental preferences, but who forgo an improvement of their choices due to informational constraints. On the other hand, households with a low socio-economic status are more prone to stay with the default, although they would prefer a cheaper contract. I showed in a cost-benefit framework that the choice default curbed a significant amount of harmful greenhouse gases. However, emission abatement seems to come at costs for consumers that are higher than recent estimates of the social cost of carbon. In sum, this evidence unfolded potential caveats for green electricity defaults and may stipulate a discussion on how to design more socially tenable nudges in residential electricity markets.

Much of the literature in behavioral economics concerning choice defaults has focused on the default effect in isolation, disregarding potential behavioral spillover effects on subsequent behavior in a similar area. I started filling this gap by answering Research Question 3, hence assessing the directions of potential spillovers induced by a preceding choice default. The analysis used an experimental design that closely adheres to frameworks in other disciplines—for instance in experimental psychology—that have been used to examine the effects of one behavior on a related and subsequent other behavior. I employed a control strategy to remove potential confounding factors in the initial behavior, thus enabling a precise analysis of the effects of a pro-socially set choice default on subsequent pro-social behavior. I showed that the choice default, as intended, promoted pro-social behavior in an initial choice and that it did not negatively spillover on subsequent choice behavior. I explored how these results may diverge when accounting for individual heterogeneity in the sample, thereby assisting the general understanding of the consequences of subsequent behavior caused through nudging.

Summing up, the key contributions of Chapters 2 to 4 add to the literature in behavioral environmental economics, highlight the applicability and issues of the design of green defaults in residential electricity markets, assess distributional consequences of green choice

defaults, and analyze potential behavioral spillover effects of default nudges. Building on these findings the next section discusses several direct and indirect lessons that choice architects in residential electricity markets could consider when applying choice defaults.

5.2 Implications for choice architects in residential electricity markets

Market liberalization has brought extensive choice to consumers in retail electricity markets. For instance, about half of the federal states in the United States offer choice of electricity contracts (American Coalition of Competitive Energy Suppliers (ACCES), 2017). Similarly, consumer policies in the EU (European Commission, 2017a), the United Kingdom, Australia, and several other countries (International Energy Agency (IEA), 2005) have strengthened opportunities for consumer choice of electricity contracts. Accordingly, the growth in number and variety of electricity products, ranging from low-priced and little environmentally friendly, to high-priced and strongly environmentally friendly, has been tremendous.[1] With an increasing number of contracts, the role of the choice architects of the market design becomes more important and their responsibility to understand the true potential of behaviorally-oriented interventions grows.

Placing a choice default has direct consequences for electricity contract choice. In particular, nudging consumers to more environmentally friendly choices with a simple change in choice architecture can substantially increase green electricity uptake (e.g., Ebeling and Lotz, 2015). However, taking my results into consideration, it is important to acknowledge that such market interventions seem to come at a cost for consumer welfare. Therefore, behaviorally-guided choice architecture needs to be closely scrutinized in terms of its design as well as its economic and behavioral consequences.

The first important recommendation for choice architects to be drawn from my results is the potential role of relative prices of green electricity defaults. Currently, it seems

[1] The US Department of Energy (2016) estimates that approximately 850 utilities offer different green electricity contracts for consumers to choose from; the Agency for the Cooperation of Energy Regulators (ACER) (2014) reports that over 280 electricity suppliers provide more than 690 different electricity contracts. See also Section 3.3 in Chapter 3 of this thesis for a more detailed description of the liberalization efforts in residential electricity markets.

attractive for choice architects in residential electricity markets to use rather low-priced hydro-power to market the additional environmental value of a green electricity default.[2] Apparently, the small cost increase on households' electricity bills seems to be a major advantage in terms of the public acceptance of such a default design. However, and as Chapter 2 shows, low priced green electricity defaults can pose challenges for residential electricity market policy making in several ways. First, my results indicate that when using defaults with low price increases for green electricity (as compared to a base price of a conventional electricity contract), consumers tend not to make their choices according to their own preferences for electricity contracts, thus infringing with a central premise of the policy approach of libertarian paternalism. Second, while—in general—consumer demand shifts to more environmentally friendly hydro-power consumption, low-priced green electricity defaults do not incentivize the demand for higher priced *new* renewable electricity capacity, such as solar, wind, or biomass. Given that several governments have ambitious targets for increasing the share of new renewable electricity capacity in order to replace more conventional technologies, such as hard coal, lignite, or nuclear (see for instance, Federal Department of the Environment, Transport, Energy and Communications (DETEC), 2017; European Commission, 2016b; National Conference of State Legislatures (NCSL), 2017), such a policy may impede these important steps towards an overall more environmentally friendly electricity system. Taking into account the currently higher costs of generating new renewable electricity from solar, wind, and biomass (International Energy Agency (IEA), 2015a), I argue to place green electricity defaults on these technologies or a mix thereof. Given my experimental results, this change in design may still increase investment signals for the capacity development of green electricity, whereas it will also lead to consumers making choices according to their own preferences.

It may also be worthwhile to consider designing electricity contracts which allow for a mixture of products. Often, consumers' choice is restricted between a choice of a 100% green electricity contract or a 100% conventional alternative. Such a design may prohibit consumers to make choices according to their own preferences. In fact, the results in

[2] Jansen, Drabik, and Egenhofer (2016, p. 5) report that the "the average price of GOs [...] is rather marginal, typically well below 1€/MWh", which can only be achieved with electricity production from hydropower.

Chapter 2 show that allowing for an incremental choice of the share of green electricity, rather than a 'one-size-fits-all approach', can lead to a better match in consumer preferences when comparing decisions under a default regime with choices in an active decision frame.

The second important implication for choice architects in residential electricity markets is the role of information that is attached to a green default electricity contract. Generally, and as evidenced in Chapter 3, there is a considerable fraction of consumers who possesses only a limited understanding of the functioning of retail electricity markets. While green electricity defaults lead to an increased demand for green electricity, it seems that most households are unaware of what they actually pay for. This is particularly a problem for households with pro-environmental preferences, because, as the results in Chapter 3 suggest, these households forgo choices to even more environmentally-friendly contracts due to ill-informed knowledge about the (environmental) consequences of staying with the default option.

Therefore, it is important for these consumers to understand the notion of 'additionality', i.e., the additional environmental benefit that is marketed with a green electricity contract, in order to be able to appreciate the potential influence of their own choices on the total electricity system. Better and potentially more targeted information could help consumers with pro-environmental preferences to 'better' decide in their own interests. Proposing more informative disclosure of the environmental benefits of an electricity contract is in line with recent advances in the European Union providing guidelines for the reliable disclosure of the environmental impact of electricity products (Intelligent Energy Europe, 2015). Specifically, it may be advocated to not only report the environmental benefit (in terms of emissions curbed) of a given electricity contract, but to indicate its potential influence on the achievement of certain policy goals, such as for instance the mandated shares of new renewable electricity in the Swiss electricity mix by 2035 (Federal Department of the Environment, Transport, Energy and Communications (DETEC), 2017).

Third, when drafting behaviorally-guided interventions in electricity markets, choice architects need to take into account the potentially detrimental effects of green electricity defaults on households with lower socio-economic status, such as lower income or poorer

education. The results in Chapter 3 point to the need of applying supporting mechanisms in order to avoid curbing harmful emissions from electricity demand at the expense of poorer households. In this vein, for instance, the community choice aggregation scheme in the United States (Local Power Inc, 2017), i.e., a scheme that bundles household electricity demand for a region and supplies green electricity, offers small-scale subsidies to low-income households.

Fourth, and finally, policy makers have been concerned with potential rebound or negative behavioral spillover effects of policies aimed at pro-environmental behavior, which could diminish initial positive effects on decision-making (Gillingham, Rapson, and Wagner, 2016; Sorrell, Dimitropoulos, and Sommerville, 2009; Tierney, John, 2011). I tested in a rigorously designed experimental framework whether to expect such spillover effects when initially issuing a pro-social choice default. I found that a pro-social default did not lead to negative subsequent pro-social behavior, thus leading to the preliminary conclusion that choice architects may set their focus on getting the design of choice defaults 'right', neglecting for the moment their potential consequences on subsequent behavior. I need to acknowledge, however, that a change in decision context or a different apprehension of the choice default itself could change these results. Thus, choice architects should remain aware of potential detrimental effects of an intervention on individuals' subsequent behavior and I advocate to come up with more empirical proofs of potential counterintuitive effects of defaults on subsequent behavior in different contexts.

To conclude, while there are certain limitations towards the assumptions stipulated in the research questions, towards the experimental designs, and towards the analyses provided, the findings in Chapters 2–4 have generated several novel and relevant insights in terms of behaviorally-guided policy making in residential electricity markets. It is an objective of this thesis that these insights contribute to helping choice architects in drafting more socially and economically tenable designs of choice defaults for an increased green electricity uptake. Still, several important avenues for future research remain, which I briefly address in the final section of this thesis.

5.3 Outlook: potential pathways for future research

Whereas Chapters 2–4 cast new light on the design and effectiveness of green electricity defaults for behavioral environmental policy making, several areas for further research remain. I will now discuss some potential areas of application, which I deem markedly relevant to build further extensions of analyses or potential new research on.

Designing default options

The implementation of a choice default comes at low costs and at little administrative burden. However, and as evidenced in Chapters 2 and 3, even small adjustments in relative prices for green electricity or the precise position of the default in a choice set can have significant negative implications for consumer welfare.

Given heterogeneous preferences for different sources of electricity production, the question remains how to design a nudge that *truly* makes choosers better off—as judged by themselves. Chesterley (2017) points out that there are two options for choice architects to encourage 'better' individual choices through the means of defaults.

First, for those individuals who are encouraged by a default to opt out and select an option that better suits their preferences, it is important to minimize the costs of opting out and selecting actively. A 'radical' approach to animate people choosing in their own interest is to abandon defaults altogether. Carroll et al. (2009) in the domain of pension plan savings, Keller et al. (2011) in the area of consumer adherence to medication, and Spital (1995) in the field of organ donation, have shown that mandating active choices can indeed be a favorable design in contrast to choice defaults. In the context of residential electricity markets, such an approach seems less feasible, as most countries stipulate binding rules for utilities that even if no choice is made, households must receive a basic supply of electricity, which is—of course—tantamount to inevitably setting a default. However, an interesting pathway for future research could be to experimentally test whether animating households to choose actively their electricity contract once every year (e.g., through informational campaigning), may lead to contract choices that align 'better' with their own interests.

Second, for those individuals who stay with a default, choice architects should aim at designing defaults in ways that reduce potential losses from not opting away. In this con-

text, several authors have suggested to use so-called 'smart' (Allcott and Kessler, 2015) or 'personalized' (Sunstein, 2012) defaults that—in a data-driven approach—intelligently adapt to people's preferences. Such policy proposals seem especially appealing as they advocate public policy making that is tailor-made for each individual in society. However, the findings in Chapter 3, for instance, show that publicly available data, such as household demographics, only poorly predict electricity contract choice. Choice architects in residential electricity markets often lack more in-depth data on people's actual preferences for an electricity contract, which makes the precise targeting of default options towards different consumer groups still a challenging research avenue. Presently, it remains unclear what data should be used as the basis for the customization of choice defaults at the household level. Additionally, there may be concerns towards the use of private and personal data for public purposes. Therefore, further research may need to make use of interdisciplinary studies in the fields of data science, law, and public policy.

Another important area for future research could be aspects of the design of the choice architecture, rather than the default option itself. In Chapter 3 the default is set on an intermediately green and intermediately expensive electricity contract. Future studies may, for instance, stringently test the consequences of simplifying choice sets to fewer selectable contracts, or to assess the implications of changing the choice default to a different contract. Methodologically, such research seems feasible to be executed with lab-in-the-field-studies that combine both, the advantages of the strict control of economic behavior with the richer context of meaningful subject pools (for an overview see Gneezy and Imas, 2017). Ideally, such questions may also be tackled in randomized controlled trials, however, researchers should be aware that considerable efforts at persuasion may be needed to implement such designs with partnering utilities.

Behavioral consequences of nudging

There is only a handful of articles that aim at looking into behavioral consequences of using nudges in public policy (e.g., d'Adda et al., 2017, Chapter 4 in this thesis). However, from the perspective of the choice architect it seems important to know more about the effects of behaviorally oriented interventions on subsequent conduct.

An interesting path of research that follows from the findings of Chapter 4 could be to examine whether differently designed defaults induce different subsequent behavior. Defaults with higher costs to opt out may lead to different subsequent reactions than defaults that are perfectly aligned with individuals' own interests. Generally, a testable hypothesis could be that as long as choice defaults are designed in a way that they correspond to people's preferences 'as judged by themselves', spillover effects may be of minor concern.

More broadly, it also seems of relevance for further research to test other behavioral interventions, such as feedback, reminders, or goal setting on their effects on subsequent behavior. As of today, there is rather little evidence documenting potential ramifications of such nudges (for a further overview see Truelove et al., 2014). Further work in the laboratory and in suitable field settings could provide valuable insights to these questions.

Cost-benefit evaluation of nudges

The evaluation of the economic effectiveness of choice defaults is paramount in order to advise public policy on the usefulness of behaviorally-guided interventions. However, there are several caveats that need to be taken into account for future research.

Policy analysis in neoclassical economics derives its objectives and the evaluation thereof from individual choices, taking the position that revealed preferences guide the analysis of which policy proposal is better in terms of social welfare (Samuelson, 1948). In light of the rich evidence of behavioral economics on biases and anomalies in individual decision-making (for a comprehensive overview see Chapter 19 in Dhami, 2016), an important question that remains is: can we use revealed choices in order to judge whether nudges make consumers better off or not?

The answer depends on whether we are able to identify the correct baseline of choices, which can be contrasted to the individual choices under the regime of a default. 'Behavioral Welfare Economics', an approach pioneered by Bernheim and Rangel (2005), tackles this issue by providing insights under which assumptions a choice-oriented framework of consumer welfare can render meaningful results. Bernheim (p. 16, italics in original, 2016) argues that three important preconditions need to hold in order to assess welfare on the basis of choices:

> *"Premise 1: Each of us is the best judge of our own well-being.*
> *Premise 2: Our judgments are governed by coherent, stable preferences.*
> *Premise 3: Our preferences guide our choices: [...] we seek to benefit ourselves."*

Evidently, if baseline choices correspond to these premises, they allow for a direct evaluation of the consequences of a nudge in terms of individuals' welfare. Whereas Bernheim (2016) argues that premises 1 and 3 seem to hold even when considering the empirical findings of behavioral economics, premise 2 poses several challenges for future research.

The use of active choices as a baseline to assess individuals' welfare is intuitively appealing, as it pushes people "to explicitly state their preferences" without the need to worry about potential bias in preference revelation due to a default nudge (Beshears, Choi, Laibson, and Madrian, 2008, p. 1791). A common assumption is that an active choice reveals 'true' (and stable) preferences. Thus, preceding studies on the welfare implications of nudges (such as Allcott and Kessler, 2015; Bernheim et al., 2015; Chetty et al., 2009), and also this thesis, make the assumption that premise 2—in general—holds.

Future research could study the implications for a cost-benefit evaluation of nudges when this assumption is relaxed. Fudenberg and Levine (2006) argue, for instance, that individuals may have multiple targets—each contributing to overall individual utility—towards which they may adjust their decision-making. In simple terms, two or more versions of preference orders could qualify as 'true' preferences, which adds complexity to the question on how to use revealed preferences for the assessment of overall welfare. Ariely et al. (2003) argue that preferences may be constructed at the moment of choice, which renders them sensitive to context, and thus potentially unstable. This may have implications for the assessment of welfare based on individual choices. Therefore, more work on identifying theoretical frameworks and empirical mechanisms that consistently allow for the elicitation of revealed preferences in the light of multiple objective functions or respectively constructed preferences is an interesting and important area for future work.

Another pathway for future research that addresses the evaluation of nudges is to delve more into the cognitive foundations of individual decision processes in choice environments with defaults or other nudges. There is very little evidence on the existence and magnitude

of individual cognitive or emotional costs and benefits of using nudges in public policy. Yet these costs and benefits could play an important role in the overall evaluation of a behaviorally oriented policy. Some scholars have proposed to measure the cognitive costs and benefits with the help of indicators of individual happiness with a decision (for an overview see Angner, 2010). Yet, more research in this direction is desirable, as there are several conceptual challenges to the measurement of the concept of happiness itself, such as for instance, the methodological constraints in eliciting self-reported life satisfaction (see e.g., Layard, 2010).

In conclusion, with only few behavioral nudges evaluated on their implications for consumers' or social welfare (e.g., Allcott and Greenstone, 2017; Allcott and Kessler, 2015; Allcott and Taubinsky, 2015; Chetty et al., 2009; Damgaard and Gravert, 2017), more research making use of choice (and non-choice) data in order to assess the welfare consequences of choice defaults seems desirable. In particular, the evaluation of pro-environmentally targeted nudges may ask for comprehensive cost-benefit analyses that balance the social planner's interest of internalizing externalities with their direct impacts on consumers' welfare. This may unveil new insights for the design of behaviorally-guided interventions and their application in the field of behavioral environmental economics.

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