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M. Sc. Sophia Elke Hilde Wingen

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Referent: Prof. Dr. Erik Hölzl, Universität zu Köln

Co-Referentin: Prof. Dr. Lea Ellwardt, Universität zu Köln

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ESSAYS IN ECONOMIC PSYCHOLOGY

Within an economic system, individuals can take on multiple roles, for example, they purchase goods as consumers, save and invest their capital, act as employees or entrepreneurs, or contribute to economic legislation. Economic psychology is thereby indispensable for understanding the multitude of factors that influence human behavior in economic contexts and for understanding how humans shape the economy (Kirchler & Hoelzl, 2017). While the integration of psychological insights into the field of economics received prominent recognition with the awarding of the Nobel Prizes to Daniel Kahneman in 2002 (*Daniel Kahneman – Facts.*, 2026) and Richard H. Thaler in 2017 (*Richard H. Thaler – Facts.*, 2026), significant challenges remain unresolved or have arisen anew. Megatrends such as demographic shifts, the technology-driven transformation of life in general and work specifically, more complex and diverse societies, globalization, and connectivity (Zukunftsinstitut, 2025) lead to constant change in the economic conditions under which individuals operate. Understanding the psychological factors that shape humans' reactions to such changed environments, as well as their inclination to contribute to changing their environment themselves, has motivated me to conduct the research in this dissertation. To cover the broad field of economic psychology, I focus on two subdomains, namely consumer psychology in Projects 1 and 2 and organizational psychology in Project 3. In the consumer psychology projects, I investigated the implementation of artificial intelligence (AI) in consumers' everyday lives, specifically in AI-enabled checkout-free grocery stores, as the sole author of Project 1 and co-authored with Professor Erik Hoelzl in Project 2. It is a common trend for payment service providers and retailers to offer increasingly invisible and straightforward payment and checkout options, such as buy now, pay later (Maesen & Ang, 2025) or mobile payments (Falk et al., 2016). An extreme manifestation of this are checkout-free stores, which operate without a checkout process, with an autonomous AI system

invisibly handling checkout and payment (e.g., *Just Walk Out*, n.d.). Retailers hope to leverage this technology to gain a competitive advantage by improving the customer experience while reducing labor costs. However, consumer adoption remains a major challenge (Day, 2024; Kumar, 2024). In Project 1, I therefore investigated whether consumers feel capable of using checkout-free stores and to what extent this perceived self-efficacy relates to their intentions to initially patronize them. While my findings can inform retailers on how to increase intentions to patronize checkout-free stores, they also raise concerns about the consequences of patronizing these stores for consumers. In Project 2, Professor Erik Hoelzl and I therefore investigated unwanted side effects of checkout-free shopping, specifically whether checkout-free shopping biases consumers' price perception and reduces their price recall. The findings of this project not only inform consumers and policymakers about the unintended consequences of checkout-free shopping but also enhance theoretical understanding of how checkout characteristics contribute to payment transparency and downstream consumer cognitions.

While the integration of AI into supermarkets may fundamentally change consumer experiences, the integration of AI across all sectors shapes the general conditions of the labor market. Particularly, while the development of generative AI has led to large-scale layoffs (Davenport & Srinivasan, 2026; Galetti, 2026), it also enables organizations to redeploy freed-up human resources by developing their employees to take on new tasks and roles. The third project of this dissertation, therefore, concerns managerial coaching as an important tool for organizations to develop their workforce. More specifically, my co-authors, Dr. Thomas Graczyk, Dr. Tobias Wingen, Professor Simone Dohle, and I investigated mindsets as a motivating factor in participants' decision to coach. We further investigated which cognitive processes mediate its effect and whether the relationship between mindsets and willingness to coach depends on the coachee's past performance. The findings of this project inform

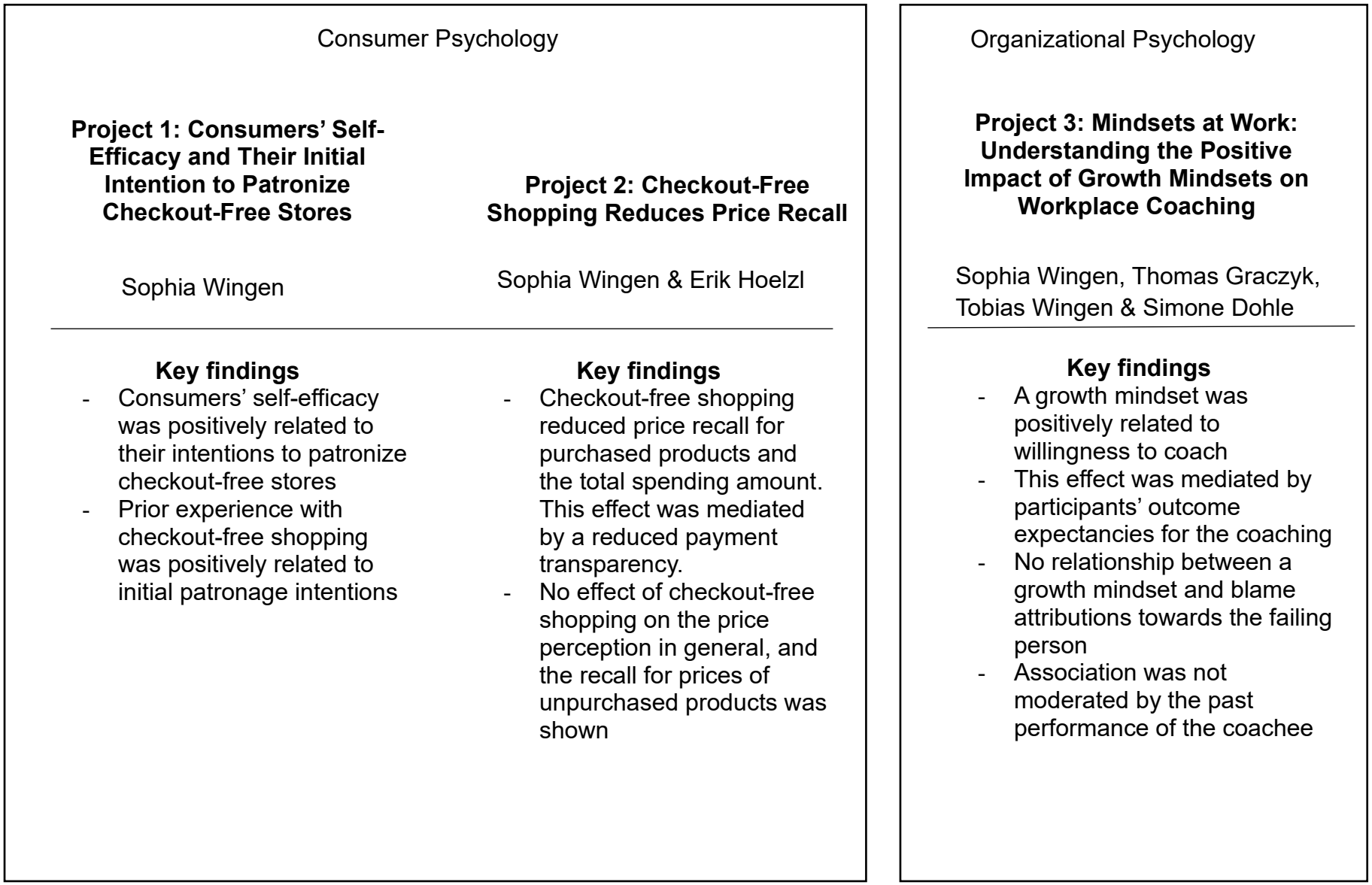
organizations on how to foster a coaching culture among their managers and further enhance theory on the interpersonal consequences of a growth mindset.

Taken together, the findings of this dissertation provide valuable insights into how humans respond to changing economic environments (Projects 1 and 2) and how willing they are to contribute to changing their environment by coaching a subordinate (Project 3). See Figure 1 for an overview of the dissertational projects.

The remainder of this dissertation is structured as follows: The first part of the dissertation is the introductory text, consisting of two chapters (consumer psychology and organizational psychology) in which research projects 1 – 3 are theoretically contextualized, linked to one another, and their findings are discussed in detail. The second part consists of the full manuscripts of Projects 1 and 2 and the published article of Project 3. The manuscripts and the article follow journal-specific requirements, particularly regarding length and the presentation of results.

Figure 1

Dissertational Overview and Key Findings of Research Projects 1 – 3



CHAPTER I: CONSUMERS' INTENTIONS TO PATRONIZE CHECKOUT-FREE STORES AND THEIR PRICE PERCEPTION AND PRICE RECALL ACCURACY AFTER CHECKOUT-FREE SHOPPING

Grocery retail must quickly adapt to new challenges, as customer groups become more diverse, new competitors enter the market, and global crises such as climate change further disrupt its operations (Edsall et al., 2023). Additionally, retailers are under high pressure to meet heightened consumer expectations for a seamless, frictionless, and personalized shopping experience (Mansukhani et al., 2025).

To overcome these challenges, grocery retailers pin their hopes on AI technologies (Edsall et al., 2023; Mansukhani et al., 2025; Wolniak et al., 2024). AI is defined as "a system's ability to correctly interpret external data, to learn from such data and to use those learnings to achieve specific goals and tasks through flexible adaptation" (Kaplan, 2020; Kaplan & Haenlein, 2019, p. 17). More than 90% of interviewed industry experts believed that AI technologies can secure retailers a competitive advantage, and 63% reported that AI is already core to their business (Mansukhani et al., 2025). In the context of grocery shopping, AI technologies currently implemented include intelligent shopping assistants, inventory management systems, dynamic pricing, autonomous checkouts, in-store navigation apps, customer sentiment analysis, fraud detection and prevention, and automated stock replenishment (Wolniak et al., 2024).

One technology that is particularly promising here is autonomous checkout (van Esch et al., 2021b, 2021a), also known as checkout-free shopping. In checkout-free shopping, customers must register upon entering a store using a smartphone app or biometric data (Schultz & Paetz, 2025). AI-supported sensor technology automatically detects and tracks when products are taken from or returned to the shelves. After product selection, customers can leave the store without stopping at checkout to pay. The AI system will process the

payment autonomously, and the amount will be deducted from customers' connected accounts. Customers receive a digital receipt via their smartphone app or email (e.g., Rewe, n.d.).

This technology is exciting to retailers, as it allows them to streamline the checkout, a painful phase of the shopping trip for consumers (Reshadi & Fitzgerald, 2023), with multiple sources of frustration, including handling products and their packaging, waiting in lines, unwanted social interactions with staff and other customers, and processing the payment. Checkout-free shopping can therefore be a win-win for customers and retailers alike. While customers benefit from a seamless, convenient, and likely even fun checkout experience, retailers can optimize processes, reduce labor costs, and collect valuable data on shopping habits (Wolniak et al., 2024).

However, integrating technology at the checkout can also make both the retailer and the customer vulnerable. First, while other store technologies, such as recommender systems or complementary apps, can be avoided, the checkout process is unavoidable on every shopping trip. So, if consumers reject the technology, they will consequently avoid checkout-free stores altogether. This is indeed becoming apparent in the market, as consumer adoption does not match retailers' expectations (Kumar, 2024). Second, the checkout process may serve a valuable function for consumers by helping them keep an overview of their budget. The salience of a payment (Soman, 2003) can influence customers' product selection (Bagchi & Block, 2011; L. Huang et al., 2024; Thomas et al., 2011), total amount spent (Prelec & Simester, 2001), perception of prices (Chatterjee & Rose, 2012; Falk et al., 2016), and recall of prices (Gafeeva et al., 2018; Soman, 2001). Streamlining the checkout process reduces the salience of the payment and may ultimately change consumers' financial control.

The research of this dissertation aims to contribute to understanding the psychological implications of checkout-free shopping at two stages of the customer journey. First, I

investigated whether consumers' self-efficacy influences their intentions to initially patronize a checkout-free store (Manuscript 1). Second, together with my co-author Professor Erik Hoelzl, I investigated the extent to which shopping in a checkout-free store influences payment transparency and, thereby, price perception and recall (Manuscript 2).

CheckoutTechnologies

As checkout is a common source of customer frustration and labor-intensive for retailers, they have been implementing innovative technologies to optimize the process (Wolniak et al., 2024). Self-checkouts, in which consumers use designated terminals, vending machines, or handheld devices to process payments themselves, were an initial attempt to streamline the checkout process (Wolniak et al., 2024). However, consumers' reactions to them were ambivalent. On the one hand, they indeed increased checkout speed, convenience (Bulmer et al., 2018), and satisfaction with the overall shopping experience (Fernandes & Pedroso, 2017). Additionally, some consumers valued avoiding unwanted social contact and the anonymity when buying personal items (Bulmer et al., 2018). On the other hand, consumers felt pressured to be responsible for the checkout process and its speed, especially when inconveniences arose (Bulmer et al., 2018). Further, authors argued that self-checkouts lead to new forms of retail criminality, for example, by customers becoming frustrated with system errors and stealing out of resignation, or by customers trying to compensate for the additional effort required or the societal implications of self-checkouts, such as employee layoffs (Taylor, 2016).

While self-checkout leads to ambivalent consumer reactions (Bulmer et al., 2018) and may even attenuate consumers' shoplifting tendencies (Taylor, 2016), checkout-free shopping appears to be a suitable solution for creating an ambient, seamless checkout experience (Edsall et al., 2023), and retailers worldwide are placing their hopes in checkout-free shopping (Wolniak et al., 2024). For example, in 2024, Rewe Group opened Europe's

largest checkout-free grocery store (Rewe, 2024). Amazon's *Just Walk Out* and competitors' *Cloudpick* technology power small convenience stores at gas stations, universities, stadiums, and in many other sectors worldwide (*AI-Powered Unmanned Retail and Warehouse Solutions*, 2024; *Just Walk Out*, n.d.).

Checkout-free shopping operates autonomously, meaning that it "actively perform[s] shopping tasks and decisions without the consumer's intervention" (Sohn, 2024, 2.1. Autonomous retail technology). Further, checkout-free stores can be unmanned, as consumers can complete their shopping trips without direct interaction with staff. Unmanned stores can take simpler forms in terms of the technology they employ, such as vending machines or self-checkout. However, checkout-free stores represent the most advanced, technology-equipped form of unmanned stores (Nam et al., 2025).

At first glance, checkout-free shopping appears intuitive, simple, and convenient; consequently, consumers' low adoption (Kumar, 2024) remains an open question for retailers and researchers alike (e.g., Hazée et al., 2025; Nam et al., 2025; Pillai et al., 2020). From a psychological perspective, many aspects of checkout-free shopping may pose barriers to consumer adoption. The novelty of AI technology can be exciting to consumers and indeed lead to a feeling of arousal (Cui et al., 2022); however, it has also been shown to threaten some consumers (van Esch et al., 2021b), and consumers expressed concerns about its reliability (Hazée et al., 2025). Additionally, the extensive use of cameras in stores raised concerns about data privacy (Hazée et al., 2025). Furthermore, while some consumers appreciated the option to avoid unwanted social contact, others worried about insufficient customer service (Hazée et al., 2025) and preferred stores with on-site staff (Benoit et al., 2024). Lastly, in a checkout-free store, consumers do not experience the actual payment or have a chance to verify the final spending amount. Consumers reported feeling payment unease in checkout-free stores, defined as the "perceived loss of control over the payment"

(Hazée et al., 2025, p. 995), and expressed a preference to verify the final amount spent at checkout (Benoit et al., 2024). In line with this, fully automated payment methods were associated with lower intentions to patronize the respective stores (Benoit et al., 2024; Nam et al., 2025). In conclusion, checkout-free shopping may seem threatening to consumers (van Esch et al., 2021b). They may perceive a lack of available support (Hazée et al., 2025) and anticipate a loss of oversight and control over spending (Benoit et al., 2024; Hazée et al., 2025).

Initial Patronage Intentions Towards Checkout-Free Stores

Managers have a variety of tools at their disposal to ensure repeat patronage of their stores, such as product quality, service, store atmosphere, and price (Burlison & Oe, 2018). However, if consumers never visit their stores in the first place, they will not experience any of these assets. Thus, for innovative store concepts such as checkout-free shopping, the initial decision to patronize is most crucial to retailers.

Despite the importance of the initial patronage decision, research on checkout-free shopping has been focused merely on related but conceptually different behaviors, such as the intention to purchase specific products (Cui et al., 2022; van Esch et al., 2021a, 2021b), and a general intention to use these stores (Hazée et al., 2025; Nam et al., 2025; Sohn, 2024). Other research included aspects such as word-of-mouth (Benoit et al., 2024; Pillai et al., 2020) or the intention to adopt such a system as a shop owner (Pillai et al., 2020). These studies examined how general attitudes towards checkout-free stores (Pillai et al., 2020), specific reasons for patronizing the stores (Hazée et al., 2025), and store characteristics (Benoit et al., 2024) relate to patronage or shopping intentions. Regarding general attitudes, perceived ease of use and usefulness of the technology, expected shopping enjoyment, and the potential to create a customized, interactive shopping experience were associated with higher intentions to patronize these stores (Pillai et al., 2020). Regarding specific reasons,

Hazée et al. (2025) identified a multitude of reasons for shopping in checkout-free stores, including payment, product selection, and appreciation for the limited social judgment and reduced need for social interaction. Reasons against shopping in checkout-free stores were, for example, expected inconveniences and a loss of control over expenditures (Hazée et al., 2025). Regarding store characteristics, consumers preferred onsite staff support, disliked using their credit cards for check-in, and preferred to verify their shopping baskets rather than use a fully autonomous checkout process (Benoit et al., 2024). Another study identified a clean and tidy environment, a diverse product selection, and competitive prices as positive influences on consumers' store choice (Nam et al., 2025).

Crucially, the findings show that understanding patronage intentions is difficult, as a multitude of attitudes (Hazée et al., 2025; Pillai et al., 2020) and store characteristics (Benoit et al., 2024) influence them. Further, consumer characteristics shape the influence of the aforementioned (Cui et al., 2022; Sohn, 2024; van Esch et al., 2021a, 2021b). For example, checkout-free shopping increased shopping intentions for specific products compared to human checkout. If consumers did not perceive AI as threatening, checkout-free shopping also increased these shopping intentions compared to self-checkout (van Esch et al., 2021b).

While these studies provide valuable insights into what may shape general intentions to patronize checkout-free stores, they did not capture participants' *initial* patronage intentions, which require a series of behaviors, such as finding out where the store is, downloading the app to register, walking by the store, and finally entering to purchase something. Only after completing these steps, other aspects, such as the intention to purchase certain products, become relevant. Despite the enormous relevance of the initial patronage decision, to date, no research has investigated how it is shaped. One such factor could be participants' self-efficacy (following Sohn, 2024; van Esch et al., 2021a).

Self-Efficacy Regarding Checkout-Free Shopping

Self-efficacy influences which situations people put themselves into, or rather, which situations they avoid. In particular, people tend to avoid situations they do not believe they can handle effectively and to approach situations they are confident they can handle (Bandura, 1977, 1982). In the consumer context, self-efficacy influenced a range of behaviors, including responsible financial behavior (Gulati & Singh, 2024), preferences for green products (Susanty et al., 2021; Zhang et al., 2023), pro-environmental behavior (H. Huang, 2016), or physical activity and healthy eating behavior (Parkinson et al., 2017). Crucially, self-efficacy seems to play a role when consumers face challenges such as the increased need to save for retirement, protect planetary resources, or achieve long-term fitness and health. Complementing this, self-efficacy also predicted the extent to which consumers adopt new technologies, including retirement apps (Hentzen et al., 2022), smartwatches (Mani & Chouk, 2017), and tablet devices (Kulviwat et al., 2014). Consumers with higher self-efficacy found new technologies easier to use (Cebeci et al., 2020; Kulviwat et al., 2014; Zhao et al., 2008), more useful (Cebeci et al., 2020; Kulviwat et al., 2014), and pleasurable (Kulviwat et al., 2014).

Self-efficacy thereby varies in its degree of generality. While self-efficacy can be specific to a task, it can also be generalized, referring to universal life skills that a person believes in having and can apply to a multitude of situations (Bandura, 1977; Smith, 1989). Thus, consumers can differ in their self-efficacy even in unfamiliar situations, either by assessing their specific self-efficacy for similar situations or by assessing their general self-efficacy for coping with new challenges.

Regarding new checkout methods, a longitudinal study on self-checkouts found that self-efficacy was particularly important for the initial intention to use them. In contrast, satisfaction and habits were more important for continued use (C. Wang et al., 2013).

In the context of checkout-free shopping, self-efficacy is the belief in one's ability to use checkout-free shopping effectively (following Bandura, 1982). It was shown that self-efficacy towards checkout-free shopping did not appear to be uniformly high (Sohn, 2024; van Esch et al., 2021a), and low self-efficacy was associated with higher perceived risks of checkout-free stores. These perceived risks, in turn, were associated with lower patronage intentions (Sohn, 2024). Additionally, self-efficacy qualified the positive effect of checkout-free shopping (vs self-checkouts) on attitudes toward the stores and shopping intention. Only consumers high in self-efficacy perceived checkout-free shopping as more convenient and reported higher purchase intention than those using self-checkouts. In contrast, participants low in self-efficacy did not show this difference (van Esch et al., 2021a). Thus, self-efficacy may be crucial in shaping consumers' initial intentions to patronize checkout-free stores.

Payment Transparency of Checkout-Free Stores

If consumers patronize checkout-free stores, the question arises of how they perceive the checkout, particularly whether they find the payment process transparent. Payment transparency refers to: "the relative salience of the payment, both in terms of physical form and the amount […]" (Soman, 2003, p. 175). Extensive research has investigated the difference in payment transparency of varying payment methods, such as cash versus card (e.g., Kamleitner & Erki, 2013; Soman, 2003; Thomas et al., 2011), or newer payment methods, such as mobile payment (Falk et al., 2016; Y. Liu & Dewitte, 2021).

This research demonstrated that low payment transparency can influence consumers' emotions (Kamleitner & Erki, 2013; Shah et al., 2016), cognitions (Falk et al., 2016; Gafeeva et al., 2018), and behavior (Bagchi & Block, 2011; Prelec & Simester, 2001; Thomas et al., 2011). In particular, it led to consumers spending more impulsively (Prelec & Simester, 2001; Soman, 2003; Thomas et al., 2011), having less financial overview (Gafeeva et al.,

2018; Soman, 2003), and feeling less connected to the purchased products (Kamleitner & Erki, 2013; Shah et al., 2016).

The theorized mechanism behind the effect of payment transparency is the perceived pain of payment (Zellermayer, 1996). The pain of payment is an adverse affective reaction caused by the actual or anticipated loss of financial resources. While the experience of *immediate* pain of payment felt after the purchase can shape how consumers feel towards products (e.g., Kamleitner & Erki, 2013; Reshadi & Fitzgerald, 2023; Shah et al., 2016) or their recall of prices (Gafeeva et al., 2018; Soman, 2001), the *anticipated* pain of payment, felt before the purchase, can curb spending, serving as an important tool for self-control (e.g., Reshadi & Fitzgerald, 2023; Soman, 2003; Thomas et al., 2011). A prerequisite for experiencing the pain of payment is actually noticing the loss of financial resources (Reshadi & Fitzgerald, 2023). As consumers perceive the outflow of money less vividly in checkout-free stores, the payment process should be perceived as less transparent, thereby influencing both the anticipated and immediate pain of payment and making expenses feel pain-free. Consequently, it is highly relevant to investigate payment transparency and its consequences for price perception and memory in checkout-free stores.

Price Perception in Checkout-Free Stores

Consumers' price perceptions can be captured in their overall store price image, which "reflects customers' beliefs about a store's overall price level relative to competition" (Falk et al., 2016, p. 2417). This estimation is highly relevant to managers and consumers. Competitive prices can influence consumers' store choice (Burlison & Oe, 2018; Nam et al., 2025) and their overall spending (Falk et al., 2016). Since it is challenging for retailers to actually offer competitive prices, they instead use a wide range of strategies to influence how consumers perceive prices (Ahmetoglu et al., 2014). For example, by giving a reference or base price to anchor consumers at that price and later revealing additional charges, or by

priming consumers with the word “free”, as in buy-one-get-one-free campaigns (Ahmetoglu et al., 2014). Despite those price-framing strategies, retailers can exploit payment transparency to shift consumers’ price perception (Chatterjee & Rose, 2012; Falk et al., 2016). Non-transparent payment mechanisms decouple the benefits of a purchase from its costs, leading consumers to focus more on benefits when the payment method is less transparent (e.g., credit card) and on costs when it is more transparent (e.g., cash) (Chatterjee & Rose, 2012). A biased perception of prices was also shown after paying with more innovative payment methods, such as mobile payments. In particular, participants reported a lower perceived price when the payment method was less transparent (mobile payment versus card/cash) (Falk et al., 2016). Despite the relevance of consumers’ price perceptions to store choice, no research has examined whether the non-transparent payment process distorts price perception in checkout-free stores, leading consumers to perceive prices as lower than in competing stores.

Price Recall After Checkout-Free Shopping

Not only price perception but also recall of product prices are highly relevant to consumers. Consumers rely on price recall to manage their budgets (Hernandez et al., 2017) and use recalled past expenses as decision criteria for future purchases (Soman, 2001). This recall is not only dependent on memory-relevant criteria, such as the number of prices one tries to recall (Estelami & Lehman, 2001), but also on situational factors, such as the payment method used (Gafeeva et al., 2018; Soman, 2001). It has been shown that less transparent payment methods, or those that do not include price rehearsals, increase errors in the recall of expenditures (Gafeeva et al., 2018; Soman, 2001). High payment transparency at conventional checkouts can improve price memory in two ways. First, the repetition of prices at the checkout should improve memory, as classical learning theory suggests that repetition enhances learning (e.g., Cepeda et al., 2006). Second, the highly transparent checkout may

lead to more experienced pain of paying, and such emotional arousal can support the formation of long-term memories (McGaugh, 2004). In line with that, price recall accuracy should be lower in checkout-free shopping than in conventional shopping. Given consumers' ability to anticipate the pain of paying (Reshadi & Fitzgerald, 2023), the checkout method should influence the formation of price memory during product selection and checkout. First, the lower *anticipated* pain of paying should shift consumers' focus during product selection away from prices (in line with Chatterjee & Rose, 2012), thereby increasing recall errors for product prices, regardless of whether the product was purchased. Second, the lower *immediate* pain of paying should further diminish the accuracy of price recall for purchased products (in line with Gafeeva et al., 2018). As reduced recall accuracy of prices can impair consumers' financial overview (Hernandez et al., 2017) and shape their spending (Soman, 2003), it is highly relevant to investigate consumers' price recall after checkout-free shopping.

Research Manuscripts Overview

In Project 1 of this Dissertation, I aimed to contribute to understanding why consumers initially patronize checkout-free stores. I therefore investigated self-efficacy as a direct antecedent of the initial patronage intention in three preregistered online studies with N = 905 participants. The studies consistently showed a positive association between consumers' self-efficacy and their initial patronage intentions. In Project 2, we aimed to close the knowledge gap on payment transparency in checkout-free stores and its effect on price perception and recall. The findings of a preregistered laboratory experiment (N = 150) showed that checkout-free shopping reduces payment transparency and thereby participants' price recall for purchased products and the total spending amount.

Extended Abstracts

Manuscript 1: I Can Do, so I Will? Consumers' Self-Efficacy and Their Intention to Patronize Checkout-Free Stores.

Sophia Wingen

Submitted to the Journal of Retailing and Consumer Services (2026)

Retailers worldwide are implementing checkout-free technology in their stores to improve the customer experience, save on labor costs, and collect valuable data (Wolniak et al., 2024). However, consumer adoption remains below expectations (Day, 2024; Kumar, 2024). It is therefore crucial for retailers to understand the factors that influence consumers' intentions to patronize checkout-free stores. Due to the relevance of this question, existing literature has focused on consumers' intention to patronize checkout-free stores or related concepts (e.g., Benoit et al., 2024; Cui et al., 2022; Hazée et al., 2025; Nam et al., 2025; Pillai et al., 2020; Schultz & Paetz, 2025; Sohn, 2024; van Esch et al., 2021a, 2021b). These studies show how store characteristics (Benoit et al., 2024; Nam et al., 2025), general attitudes (Pillai et al., 2020), or specific reasons, such as expected convenience or limited social judgment (Hazée et al., 2025), relate to patronage intentions. Furthermore, another stream of research has demonstrated that checkout-free shopping can positively affect consumers' perceptions of the stores and their patronage intentions. However, these positive effects were shaped by consumer characteristics such as their perception of AI technologies (van Esch et al., 2021b) or their belief in their ability to shop successfully in a checkout-free store, that is, their self-efficacy (van Esch et al., 2021a). While these articles demonstrate that understanding general patronage intentions is already complex, they did not specifically capture participants' initial patronage intentions. However, the initial patronage decision is most crucial because, without it, all other efforts towards pricing, store atmosphere, or assortment become meaningless, as

consumers will never experience them. For checkout-free stores, initial patronage requires a series of behaviors, such as downloading the app to register, which are not covered by existing research.

As research showed that self-efficacy leads to more positive perceptions of checkout-free stores (Sohn, 2024; van Esch et al., 2021a) and is positively related to adoption intentions for various types of innovative technologies (Hentzen et al., 2022; Kulviwat et al., 2014; Mani & Chouk, 2017; C. Wang et al., 2013), it is most likely influencing consumers' decisions to initially patronize checkout-free stores. To date, no research has investigated self-efficacy as a direct antecedent of initial patronage of checkout-free stores. I hypothesize that self-efficacy positively relates to participants' initial patronage intentions toward checkout-free stores. Further, I hypothesize that the salience of different characteristics of checkout-free shopping influences consumers' self-efficacy and patronage intentions, and that the experience of (a lack of) knowledge about checkout-free stores increases (decreases) consumers' self-efficacy and downstream patronage intentions. To test my hypotheses, I conducted three preregistered online studies using the platform *Prolific* for data collection. In Study 1 ($N = 197$), I assessed the relationship between participants' self-efficacy and their patronage intentions for checkout-free stores in a cross-sectional survey. In Study 2 ($N = 401$), I conducted a between-subject online experiment to test whether the salience of different characteristics (high convenience, the use of innovative AI technology, low payment transparency, and the absence of human staff) of checkout-free stores influences participants' self-efficacy and patronage intentions. In Study 3 ($N = 307$), I conducted a between-subjects online experiment in which I manipulated self-efficacy by asking participants either complex or simple questions about checkout-free shopping to demonstrate a causal effect on patronage intentions. All three studies consistently showed a positive medium-to-large correlation between participants' self-efficacy and their initial patronage intentions. Other than

hypothesized, there was no effect of the salience of different characteristics of checkout-free shopping on participants' self-efficacy or initial patronage intentions (Study 2). In Study 3, I successfully manipulated self-efficacy; however, the effect on patronage intentions was not significant. In conclusion, consumers' self-efficacy appears to motivate their initial intention to patronize checkout-free stores. Self-efficacy was not so much influenced by the salience of different store characteristics as by personally experiencing sufficient (lacking) knowledge about checkout-free shopping. These findings underscore the relevance of consumers' self-efficacy to their adoption of new technologies, even when the technology operates autonomously, and consumers are not required to operate it. Retailers can use this knowledge to increase patronage intentions for their checkout-free stores. First, they could target high-self-efficacy consumers, for example, those who have previously used another innovative store technology. They could also support low-self-efficacy consumers through supportive staff members or bring-a-friend campaigns. Consumers can reflect on their self-efficacy to more objectively understand their adoption decision of checkout-free stores.

I am the sole author of this article. Professor Erik Hoelzl offered helpful guidance and support.

The scales for measuring self-efficacy and patronage intentions were developed as part of a master's thesis that I closely supervised.

A very similar version to this extended abstract was submitted and accepted to the *31st International Congress of Applied Psychology*, which will take place between 21st – 25th of July, 2026.

Manuscript 2: Checkout-free Shopping Reduces Price Recall

Sophia Wingen & Erik Hölzl

In Preparation to be Submitted to Marketing Letters

Retailers worldwide are implementing checkout-free shopping technology in their stores (e.g., Amazon Go in the US, Rewe Pick&Go in Germany). Companies hope that checkout-free shopping can increase profits by decreasing labor costs. The gathered data can also help them tailor their marketing and product range to customers' shopping behavior. Additionally, they want to remove customer hassles at the checkout, thereby improving the customer journey (Wolniak et al., 2024).

Despite the widespread accessibility of checkout-free shopping, research on its psychological consequences for consumers' cognitions and behavior remains scarce. Initial research primarily focuses on consumers' shopping or patronage intentions (e.g., Cui et al., 2022; Hazée et al., 2025; Nam et al., 2025; van Esch et al., 2021a, 2021b), overlooking the psychological consequences of such a shopping experience for consumers. One crucial psychological concept currently overlooked is *payment transparency*, that is, the salience of giving up financial resources when paying (Soman, 2003). The effect of payment transparency was originally investigated in the context of different payment methods, such as comparing cash versus card payments (e.g., Chatterjee & Rose, 2012; Soman, 2003), and mobile payments (Falk et al., 2016). We extend that concept to different types of *checkout* and propose that checkout-free shopping reduces payment transparency compared to conventional shopping. That is because a conventional checkout offers multiple opportunities for customers to interact with product prices and the payment itself: Customers place their selected products on the belt and see them being scanned. They see the individual prices and the total price on the checkout display. They also choose their payment method and pack

their products into a shopping bag. Eliminating these steps should reduce payment transparency in checkout-free stores.

Decreased payment transparency can influence consumer cognitions during and beyond the shopping trip. Research on payment methods suggests that reduced payment transparency can lead to a biased price perception (Chatterjee & Rose, 2012; Falk et al., 2016) and adversely affect consumers' price recall (Gafeeva et al., 2018; Soman, 2001). In line with that, we propose that reduced payment transparency in checkout-free stores influences consumers' price perception. More precisely, consumers should perceive prices in checkout-free stores as lower than those in other stores (in line with Falk et al., 2016). Further, we propose that reduced payment transparency in checkout-free stores diminishes the recall accuracy of prices (in line with Gafeeva et al., 2018). Price recall accuracy should be diminished not only for purchased products but also for those that were not purchased. That is because anticipating the reduced payment transparency should shift consumers' focus away from prices during the product selection and payment phases (in line with Chatterjee & Rose, 2012).

To test our predictions, we conducted a pre-registered, incentivized, between-subjects experiment. We collected data from $N = 150$ participants to investigate whether checkout-free shopping compared to conventional shopping reduces payment transparency, which then leads to a more favorable price perception (i.e., prices are perceived as being cheaper than in other stores) and reduces recall accuracy of prices (i.e., the total spending amount, prices of purchased products, and prices of unpurchased products).

Participants were randomly assigned to shop in either a simulated conventional or checkout-free store. Therefore, we built a store in our laboratory that could be converted from conventional to checkout-free and vice versa. The participants' task was to go on a shopping trip using a general shopping list that contained the three product categories available in the

store (chocolate, strawberry jam, spaghetti). According to that shopping list, participants should purchase one item from each product category. Participants received either a payment card or a virtual payment account with a €10 budget for their expenses. To incentivize participants to make realistic choices, 10% could win their shopping basket, and the remaining budget. After their shopping trip, participants returned to the reporting room, where they had to recall the prices of all individual products and their total spending amount. To additionally incentivize participants to recall prices as accurately as possible, the top 10% received a €10 bonus. Further, participants had to complete items on price perception (Falk et al., 2016) and payment transparency.

We analyzed the data using multi-level analyses. Results indicate that checkout-free shopping significantly increased recall errors for the prices of *purchased* products and the total spending amount. For purchased products, the price recall error was 7 percentage points higher in the checkout-free shopping group than in the conventional checkout group. For total spending, the recall error increased by 10 percentage points in the checkout-free shopping group. Furthermore, 24 participants in the conventional checkout group could recall their exact total spending. In comparison, only 4 participants in the checkout-free shopping group could do so. Results further indicated that the effect of checkout-free shopping on recall errors for the prices of purchased products and the total spending amount was significantly mediated by reduced payment transparency in checkout-free stores. However, the results did not support the proposed effect of checkout-free shopping on price recall for unpurchased products or on overall price perception.

Our findings have implications for theory and practice. First, we extend the payment transparency literature by showing that not only the actual payment (Chatterjee & Rose, 2012; Falk et al., 2016; Gafeeva et al., 2018; Soman, 2001) but also the entire checkout process matters for payment transparency. Second, we provide several practical implications.

Reduced payment transparency in checkout-free stores leads to biased memory of expenditures; however, consumers need an accurate memory of expenditures to monitor their budgets. Policymakers should protect consumers from losing their budget overview. Additionally, consumers could use tools such as budget planners to improve their financial overview. Managers should clarify if consumers avoid checkout-free stores altogether to control their spending. To counteract this, managers could implement measures to improve consumers' budget awareness, for example, by using haptic feedback in their checkout-free shopping app to inform customers about their purchase. Thereby, consumers' acceptance of checkout-free shopping could be increased.

Professor Erik Hoelzl and I jointly developed the research project idea. I secured research funding, developed the method, programmed the study, collected and analyzed the data, and received feedback from Professor Erik Hoelzl. I wrote the first draft of the manuscript, and Professor Erik Hoelzl reviewed the analyses and edited the manuscript into a final draft. Professor Erik Hoelzl and I then jointly edited the final manuscript.

A very similar version of this extended abstract was submitted and accepted to the *49th IAREP International Conference* in Tartu, Estonia (2025), where Professor Erik Hoelzl presented it. Further, I presented this research at the DGPS Fachgruppentagung AOWI 2025 in Lüneburg, Germany, and at the *International Association of Applied Psychology's 2025 Early Career Marathon: Integrating Research and Practice*, where it received a Runner-Up Award.

Discussion

Checkout-free shopping poses significant challenges for retailers and consumers. While retailers are under pressure to gain a competitive advantage and turn their investments into profits, consumers aim for convenience but still want financial control and oversight. These differing interests create a space of tension where psychological research can provide knowledge to minimize risks and unlock potential, crucially by placing consumers' perspectives on checkout-free shopping at the center of the discussion. Despite increasing efforts over the past five years (e.g., Cui et al., 2022; Sohn, 2024; van Esch et al., 2021a, 2021b), research on checkout-free shopping remains in its infancy, and the findings from Projects 1 and 2 help close important knowledge gaps. First, while a body of research has examined various conceptualizations of patronage and/or purchasing intentions (e.g., Cui et al., 2022; van Esch et al., 2021a, 2021b), the initial patronage intentions of consumers have been overlooked to date. In Project 1, I demonstrated that, although checkout-free shopping was designed to blend seamlessly into consumers' daily lives, consumers do not necessarily perceive themselves as capable of using it. This low perceived self-efficacy was associated with reduced initial intentions to patronize these stores.

Second, while most research in the field of checkout-free shopping focuses on when or why consumers choose to shop in checkout-free stores (Cui et al., 2022; van Esch et al., 2021b, 2021a), little is known about the consequences for consumers. Our findings show that the completely invisible checkout process makes the payment non-transparent and distorts consumers' price memory for purchased products. Combining the findings of Projects 1 and 2 leads to the conclusion that consumers' concerns about their self-efficacy may not be unfounded, as checkout-free shopping indeed creates new challenges that require consumers' competencies in budget oversight.

Limitations

This research has important limitations that must be considered before drawing theoretical and practical implications. The first limitation concerns the generalizability of the findings. In the studies, we collected data from participants in the UK and Germany, two important emerging markets for checkout-free shopping. Nevertheless, checkout-free shopping is still relatively new in these countries, and consumers from countries where checkout-free shopping is more widespread, such as the USA, may show different effects. Additionally, research showed that payment methods carry different cultural meanings. For example, a positive effect of cash payment on psychological ownership was found only among non-Asian participants, but not among Asian participants (Kamleitner & Erki, 2013). Thus, the findings may not generalize to consumers from other countries and cultures. Additionally, in the studies, we specified a particular type of checkout-free shopping that is closely related to the operations of Amazon Go and Rewe Pick&Go. However, checkout-free stores can take different forms; for example, they can be powered by additional autonomous technologies, such as product-finding technologies or robotic shopping assistants (Pillai et al., 2020), or may still have staff or be operated unmanned (Nam et al., 2025). These specific characteristics may shape the relevance of self-efficacy, as it may be more important for an unmanned store than for a store where some staff members are present to assist customers with issues (following Hazée et al., 2025).

Second, although I experimentally manipulated self-efficacy in Project 1, Study 3, the effect was small and did not influence patronage intentions. Thus, my findings rely solely on correlations, and causality cannot be definitely assumed. Third variables, such as whether consumers feel threatened by AI (van Esch et al., 2021b), may have reduced both self-efficacy and patronage intentions. Additionally, according to cognitive dissonance theory, individuals are generally motivated to reduce conflicting cognitions (Festinger, 1964). In line

with that, participants who stated a high initial patronage intention should also have been motivated to report high self-efficacy, and vice versa.

Third, in Project 2, we conducted a controlled laboratory experiment that created a rather artificial environment, which may limit the external validity of our findings. The laboratory store contained few stimuli besides products and prices. In actual stores, customers are confronted with other customers, ambient lighting, music, advertisements, and many more distractions. Thus, in an actual store, the effect of checkout-free shopping on price memory may be amplified as consumers become more easily distracted by other factors, thus needing the checkout process even more for budget oversight.

Lastly, participants in Study 2 did not receive a paper receipt (conventional shopping) or a digital receipt (checkout-free shopping). This receipt may be used to restore lost price memories, and our findings do not allow us to conclude that. However, it has been proven that the temporal proximity between the purchase decision and the perceived payment is essential to consumers (Prelec & Loewenstein, 1998; Soman, 2001). Particularly, the immediate depletion of wealth after a purchase decision increased price recall accuracy and reduced future purchase intentions for products in the same category (Soman, 2001). Thus, even though the receipt could restore missing memories, the timely separation in checkout-free stores between the purchase and the *perceived* depletion of wealth can lead to further biases.

Theoretical Implications and Suggestions for Future Research

The findings of Project 1 are consistent with the predictions of self-efficacy theory, that people tend to approach situations in which they feel high self-efficacy and to avoid those in which they feel low self-efficacy (Bandura, 1977, 1982). In line with that, self-efficacy was a consistent and robust predictor of participants' initial patronage intentions. Thereby, the findings demonstrate the applicability of self-efficacy theory to an innovative,

highly automated technology. It may seem intuitive that handing over all tasks to autonomous technology makes consumers' self-efficacy obsolete, as they have very little to do. The findings of my study, however, show that this is not the case. Even for highly automated technologies such as checkout-free shopping, consumers' self-efficacy was a crucial predictor of the intentions to patronize.

This finding raises important follow-up questions. First, the sole correlational nature of Project 2's findings raises the question of the causal effect self-efficacy has on patronage intentions. Future studies should employ more effective self-efficacy manipulations to test its causal effect. This could be done through field studies and actual training in checkout-free stores. Future research could also build on other sources of self-efficacy, such as verbal persuasion in the form of a strong marketing message or vicarious experiences, for example, by watching videos of others successfully using checkout-free shopping (in line with Bandura, 1977).

My research also raises the question of which aspects of checkout-free shopping unsettle consumers. Although the salience of different characteristics in my study did not influence participants' self-efficacy, the reviewed literature (e.g., Benoit et al., 2024; Bulmer et al., 2018; Nam et al., 2025; van Esch et al., 2021a) still makes it plausible to assume that different aspects, such as the non-transparent payment process (Hazée et al., 2025; Nam et al., 2025) or the absence of human staff (Benoit et al., 2024; Hazée et al., 2025), may exert varying degrees of influence on self-efficacy. Research should use controlled laboratory settings to manipulate specific characteristics of checkout-free shopping and investigate their effect on self-efficacy.

Additionally, this research investigated patronage intentions, and consumers may not follow through on their intentions to use checkout-free stores because of strong shopping habits that lead them to patronize other stores. It has been shown that consumers often revert

to their habits even when they intend to try new products (Labrecque et al., 2017). Other factors could include consumers changing their minds or forgetting their intentions, for example, due to excessive effort required to set up the app, or marketing messages from other retailers. They may also have limited control over the store choice, for example, if other household members disagree with using checkout-free shopping (following Ajzen, 2020). Future research should investigate the relationship between patronage intentions and actual initial patronage, as well as the factors that close the gap between them.

Lastly, future research should investigate how initial patronage can be turned into long-term loyalty. While self-efficacy was crucially influential for initial patronage intentions, research on self-checkouts indicates that, for continued use, emotional and habitual processes may be more important (C. Wang et al., 2013). Future research should employ longitudinal designs to investigate the role of established factors of continued store patronage, such as product quality, service, store atmosphere, and price (Burlison & Oe, 2018; Nam et al., 2025), as well as specific characteristics of checkout-free shopping. These could be the convenience (van Esch et al., 2021a), ease of use, usefulness, or enjoyment of shopping in such stores (Pillai et al., 2020), as well as consumers' concerns about the societal implications of this checkout method (Bulmer et al., 2018).

The findings of Project 2 provide valuable insights into what could be unsettling for consumers in checkout-free stores, as participants perceived the checkout process as non-transparent. As the reduced payment transparency diminished participants' price recall, research should further investigate whether this holds across all consumer groups. Consumers exhibit chronic differences in their sensitivity to the pain of paying (Rick et al., 2008). While the so-called spendthrifts feel a little pain of paying and tend to spend more than their rational selves would, the so-called tightwads feel intense pain of paying and tend to spend less than their rational selves would (Rick et al., 2008). Tightwads made more impulsive purchases

when paying by credit card than when paying in cash. Spendthrifts' impulsivity, on the other hand, was not affected by the mode of payment (Thomas et al., 2011). Other researchers argue that tightwads compromise more when making cash payments. When they use less painful payment methods, they choose the products they really like (Hung et al., 2025). It should be investigated whether checkout-free shopping makes spendthrifts even more frank about their spending, allows tightwads to follow their preferences without feeling the pain of paying, or makes one of the two groups particularly vulnerable to overspending and loss of memory of their expenditures. Further, for less painful payment methods, financial literacy could serve as a substitute for pain. In particular, research showed that less painful payment methods were particularly associated with overspending among young consumers with low financial literacy (Sjam, 2025). Thus, checkout-free shopping could put consumers with low financial literacy particularly at risk of overspending and price-memory loss.

The findings of Project 2 are consistent with prior research indicating that payment transparency reduces the accuracy of price recall (Gafeeva et al., 2018; Soman, 2001). Our results further extend these findings by demonstrating that not only the moment of payment but also the overall checkout process matters for the formation of price memory.

This finding raises the follow-up question of whether checkout-free shopping only impairs consumers' memory for that specific shopping trip, or if frequent visits to checkout-free stores can have a long-term effect on consumers' financial overview. Particularly, the question arises whether long-term exposure to non-transparent checkouts will impair consumers' anticipation of the pain-of-payment, a necessary emotion that helps curb consumer spending (Reshadi & Fitzgerald, 2023; Soman, 2001). In the face of the general trend toward invisible, seamless payment technologies, it is imperative to investigate the extent to which transparent checkouts are important for experiencing the pain of payment and learning to anticipate it.

Additionally, the question arises whether non-transparent payments and the loss of budget oversight are reasons consumers avoid checkout-free stores. This reasoning aligns with a recent development in marketing research that questions the hedonic principle that consumers seek solely to maximize pleasure (Kastanakis et al., 2026). Instead, this line of research argues that pain is sometimes sought for greater reward (Kastanakis et al., 2026). Future research should investigate whether consumers actively seek the pain of payment to gain the overall benefits of budget control and oversight.

Besides price recall accuracy, research on payment methods indicates that non-transparent checkouts may have a variety of consequences for consumers' emotions, cognitions, and behavior. In principle, it was shown that low payment transparency loosens consumers up regarding their spending, meaning they spend more (Boden et al., 2020; Y. Liu et al., 2021; Moore & Taylor, 2011; Prelec & Simester, 2001; Raghubir & Srivastava, 2008) and purchase more unhealthy products (Thomas et al., 2011). Additionally, after the purchase, consumers felt less connected (Shah et al., 2016) and less ownership towards their products (Kamleitner & Erki, 2013). Taken together, research should investigate if the non-transparent checkout process leads to careless consumer behavior that increases short-term sales but, in the long term, disconnects consumers from products and stores.

Another currently overlooked aspect is the extent to which the low payment transparency in checkout-free stores can influence the relationship between retailers and the brands they sell. The low level of payment transparency at checkout-free stores may tempt consumers to purchase more unhealthy products (Thomas et al., 2011). Further, lower payment transparency reduced the compromise effect (Simonson, 1989), which refers to the phenomenon that "a brand's market share will be greater when it is the middle option in a choice set and will be smaller when it is the extreme option" (Hung et al., 2025, Compromise effect, payment forms, and the pain of paying). Therefore, the market share of products and

brands positioned as more utilitarian, or that benefit greatly from the compromise effect, could be reduced in checkout-free stores. Future research should examine brand managers' reactions to their cooperating retailers' implementation of checkout-free stores.

Lastly, the findings of this project question whether the concept of payment transparency (Falk et al., 2016; Gafeeva et al., 2018; Kamleitner & Erki, 2013; Soman, 2003) can fully grasp the changing environment in which consumers purchase groceries, or if it needs to be extended to a concept of *purchase transparency.* By showing that elements of the entire checkout process mattered for these perceptions of transparency, it becomes apparent that, for consumers, it is not only about noticing the payment but about noticing the purchase itself. Future research should deepen the understanding of purchase transparency by investigating which elements of the checkout process most strongly shape this perception. A controlled laboratory store setting can be helpful, allowing researchers to add or remove elements of the checkout process to assess their effects on purchase transparency. For example, a study could investigate the effect of a simple exit gate as a confirmation of purchase, or the role it plays to pack products from a cart into a personal bag.

Practical Implications

The findings of Projects 1 and 2 hold valuable implications for retailers, consumers, and policymakers. Retailers are under intense pressure to compete with one another and with the convenience and product selection offered by online shopping. Industry experts, therefore, argue that retailers should leverage their physical stores as a strength by making them destinations that offer experiences unique to in-store shopping (Edsall et al., 2023). The findings from Projects 1 and 2 question whether checkout-free shopping transforms stores into destinations consumers desire to visit.

First, not all consumers believed in their capabilities to use checkout-free stores, and, following that, expressed low initial patronage intentions. Retailers should consider this and

conduct large-scale customer surveys to investigate concerns about self-efficacy. Building on that, they should develop self-efficacy interventions, such as a supportive staff member helping customers set up the checkout-free shopping app. To build self-efficacy, customers can also benefit from positive experiences with checkout-free shopping (in line with Bandura, 1977, 1982). Retailers should therefore develop marketing campaigns that nudge participants to initially patronize checkout-free stores, despite low self-efficacy. Exceptionally high discounts or bring-a-friend campaigns, where customers get rewarded for referring new customers, could achieve this. Consumers must then have a positive experience with checkout-free shopping, as a negative experience can undermine consumers' self-efficacy if it is not yet very robust (Bandura, 1977, 1982). Thus, retailers need to ensure the technology is reliable before launching such campaigns. Additionally, retailers should design their marketing messages to foster self-efficacy. Research showed that the fluency of a marketing message can increase self-efficacy and downstream behavioral intentions (Kim & Jang, 2018). Additionally, retailers could implement specific self-efficacy messages in their campaigns, such as "You can do it".

Lastly, retailers could target high-self-efficacy consumers, for example, by offering personalized coupons to customers who have successfully used other innovative technologies, such as the store's app, and may therefore have high self-efficacy for new technologies.

The findings from Project 2 add to that understanding by demonstrating that in checkout-free shopping, it is challenging for consumers to maintain budget oversight. Research showed that consumers actively selected payment methods that support them with budgeting. More precisely, consumers were more likely to use cash payments if they found it more helpful for budgeting, while those who found debit cards more helpful for budgeting were more likely to use debit cards (Hernandez et al., 2017). Following that, consumers may

avoid checkout-free stores to protect their financial oversight. Retailers can support customers with budget oversight, as *Amazon* already did by implementing its smart shopping carts, which show the running total of all items in the cart on a screen (Kumar, 2024), or *REWE,* which gives customers the option to verify the total spending amount and initiate the payment at a terminal before leaving the store (Rewe, n.d.).

The price-memory effect of checkout-free shopping also raises concerns about consumer welfare and offers valuable implications for consumers. As an accurate and unbiased recall of expenditures is necessary for consumers to monitor their budgets effectively (Hernandez et al., 2017; Soman, 2001), consumers should be aware of that risk. If they use checkout-free shopping, they could implement expense-tracking measures, such as an expense app or a budget book. The digital receipt could be used to track expenses in the app or the book. They could also establish a habit of using checkout-free shopping only for regular, well-known expenses.

Lastly, the findings hold valuable implications for policymakers. There is a general industry trend toward designing seamless, invisible, and decoupled payment and checkout methods, such as checkout-free shopping, mobile payments (Falk et al., 2016), or buy-now-pay-later (Maesen & Ang, 2025). The aim is to make spending pain-free, decouple product costs from their benefits, or increase the customer's available monetary resources. Policymakers should be aware that this movement can harm consumers by making payments less transparent, which consequently leads to diminished price recall. Thus, policymakers should consider measures to protect consumers, such as an upper limit on the quantity and amount for such payment and checkout methods.

Lastly, while Projects 1 and 2 shed light on various aspects of the retailer-customer interaction, a vast, equally important dimension of integrating checkout-free shopping into stores remains unaddressed. This is the perspective of the frontline employees. Retailers are

among the largest employers in many countries (Mansukhani et al., 2025), and the automation of tasks performed by human staff can lead to massive layoffs at worst (Kaplan, 2020) or new task allocation at best. It will surely require research, ethical company practices, and fast-moving policymakers to utilize checkout-free shopping to increase workers' satisfaction while protecting their jobs. If companies want to implement AI while retaining their employees, they need to rapidly develop their workforce into new roles and responsibilities (Mansukhani et al., 2025).

CHAPTER II: GROWTH MINDSET AND WILLINGNESS TO COACH

Organizations are constantly facing new challenges in managing their workforce and attracting, retaining, and developing high-quality, skilled workers. For example, demographic change and a decline in the pool of available workers have challenged human resource managers for decades (Hennings et al., 2025). The COVID-19 pandemic forced organizations to quickly adjust employees' working conditions and to manage the required flexibility and adaptability among them (Carnevale & Hatak, 2020). At the time of writing this dissertation, developments in generative AI pose a major challenge and have led some organizations to large-scale layoffs (Davenport & Srinivasan, 2026; Galetti, 2026). Instead of leveraging AI's potential by reducing the workforce, organizations may also reallocate freed-up human resources by developing employees into new roles and responsibilities. To achieve this and be prepared for future challenges in human resource management, organizations must invest in human resource development. Organizations have several tools at their disposal to develop their workforce, such as formal talent development programs that enhance general skills and knowledge (Garavan et al., 2012). Such training can take the form of lectures, conferences, or self-instruction material (W. Arthur et al., 2003). Organizations can also employ more relationship-based development interventions such as mentoring or coaching (Clutterbuck, 2009; Garavan et al., 2012). Mentoring thereby takes a holistic perspective on a person's development, focusing on the "Who or what do you want to become?" (Clutterbuck, 2009, p. 479). Mentoring, therefore, can have a broad scope, focusing on networking and career development rather than only on task- and performance-related outcomes. Following that, mentoring relationships are typically longer than coaching relationships (Clutterbuck, 2009). Coaching, on the other hand, is a shorter, more focused process aimed at enhancing specific work performance (Clutterbuck, 2009). In particular, managerial coaching, where a manager coaches a subordinate (Dahling et al., 2016; Jones et al., 2016), is a cost-effective tool for

improving individual and organizational performance. However, research shows that not all managers are equally motivated to coach their subordinates (Heslin et al., 2006). In particular, their mindset, that is, their belief in the changeability (growth mindset) or stability (fixed mindset; Dweck et al., 1995; Dweck & Yeager, 2019), of personal attributes, is a crucial motivator for them to coach their subordinates (Heslin et al., 2006). While initial research confirmed a positive effect of a growth mindset on managers' coaching quantity and quality (Heslin et al., 2006), research on mindsets of personality (Ryazanov & Christenfeld, 2018) suggests that it may depend on the subordinates' past performance. A growth mindset may only lead to more coaching for subordinates who failed a single time, but to less coaching for a subordinate who failed continuously. This is complemented by recently developed dual pathway models (Burnette et al., 2017; Dohle et al., 2022; Graczyk et al., 2024) that predict that a growth mindset fosters two counteracting cognitions: first, the belief that helping, such as coaching, can be effective, and second, the belief that others are to blame for their conditions, such as their failure at work (Burnette et al., 2017; Dohle et al., 2022; Graczyk et al., 2024). While the belief in coaching's effectiveness should motivate managers to coach, blaming a suffering subordinate may cause them to withdraw from coaching that person. It remains unclear to what extent these two counteracting effects shape managerial coaching, and crucially, under which conditions a growth mindset leads managers to coach more or less.

The research of the third project of this dissertation aims to close these knowledge gaps by first applying the dual-pathway model of mindsets (Graczyk et al., 2024) to the context of managers' mindsets and their willingness to coach, and second, investigating coachees' past performance, that is, single or continuous failure, as a moderator of the relationship between mindset and willingness to coach.

Managerial Coaching

Coaching is a viable tool for organizations to develop their workforce into new job roles. It has been shown that coaching activities are positively related to organizational performance (Zuñiga-Collazos et al., 2020), employees' goal attainment (Dahling et al., 2016), and employees' workplace satisfaction, skills, competencies, and productivity (Jones et al., 2016). Coaching can be conducted by external coaches or consultants, who do not have managerial responsibility for the coachee and are not employed by the same organization (Jones et al., 2016). Coaching can also be conducted internally, meaning that employees of organizations coach others either on the same (Fey et al., 2022) or at a different hierarchy level (Dahling et al., 2016; Jones et al., 2016). Peer coaching, thereby, involves a coach and coachee working at the same organizational level (Fey et al., 2022), whereas managerial coaching refers to a manager coaching a subordinate (Dahling et al., 2016). Coaching constitutes a "collaborative, goal-oriented helping relationship between a coachee and a coach" (S. Wingen et al., 2024, p. 382; see also Bozer & Jones, 2018; Smither, 2011). Although there is disagreement about the exact definition of managerial coaching, common elements include an open exchange culture between managers and their subordinates, behavioral modeling by managers, and the setting of challenging, engaging goals to change subordinates' behavior (Dahling et al., 2016).

Managerial coaching is particularly attractive to organizations, as managers-as-coaches have internal knowledge of the required skills and desired performance and can provide opportunities to apply learnings directly in the work environment, thereby maximizing learning transfer (Burke & Hutchins, 2007). Additionally, a meta-analysis demonstrated that internal coaching was more effective than external consultant-coaching (Jones et al., 2016).

Given the potential of managerial coaching to develop the workforce and contribute to organizational success (Dahling et al., 2016; Jones et al., 2016), it is highly relevant to investigate the factors that motivate managers to coach their subordinates.

Managers' Mindsets

Mindsets (also called implicit theories) are human core assumptions about their own and other people's traits (Dweck, 1999; Dweck et al., 1995). A growth mindset refers to the belief that traits are malleable and can be changed, whereas a fixed mindset refers to the belief that traits are fixed and unchangeable (Dweck, 1999; Dweck et al., 1995; Dweck & Yeager, 2019). Mindsets, however, are not dichotomous; they refer to people falling along a continuum between fixed and growth mindsets (Burnette et al., 2023; Dweck, 1999). Further, mindsets can be domain-specific and relate, for example, to the changeability of intelligence (Dweck, 1999), body weight (Burnette et al., 2017), or empathy (Ryazanov & Christenfeld, 2018) of the self or others (Dweck, 1999). However, they can also refer to more general attributes, such as *the kind of person* someone is, without considering specific characteristics (Dweck, 1999; Heslin et al., 2006).

Mindset influences how information about the self and others is understood and provides a framework that fosters cognitions and behaviors consistent with it (e.g., Dweck, 1999; Erdley & Dweck, 1993; Heslin et al., 2006). A growth mindset for example leads people to focus more on psychological explanations for behavior, expect less consistency in behavior across situations and over time and motivates people to help and educate others, while a fixed mindset leads people to focus on trait-based explanations for behavior, expect high behavioral consistency across situations and over time and motivates people to punish others in case of behavioral transgressions or failure (Dweck, 1999; Dweck et al., 1995; Erdley & Dweck, 1993; Gervey et al., 1999). As mindsets constitute a motivational framework in interpersonal encounters, it is no surprise they were applied to the

organizational setting, to investigate both the effect of subordinates' (e.g., Caniëls et al., 2018; Lin et al., 2017; Z. Wang & Chen, 2026) as well as managers' mindsets (Heslin et al., 2005, 2006; Heslin & VandeWalle, 2011; Kouzes & Posner, 2019) on variables relevant for organizational success, such as managers' performance appraisals and coaching (Heslin et al., 2005, 2006).

Research has shown that a growth mindset improves managerial performance on relevant leadership tasks. First, managers were able to assess their subordinates' performance more precisely, particularly after they changed. While managers with a fixed mindset anchored their performance appraisals more to the initially observed performance, managers with a growth mindset were better able to adjust their appraisals when their subordinates changed (Heslin et al., 2005). This ability to adjust performance appraisals in response to behavioral changes seems to be perceived positively by subordinates. Research showed that participants perceived the procedures their managers used to evaluate their performance as fairer (i.e., procedural justice) when their managers had a growth mindset than when they had a fixed mindset (Heslin & VandeWalle, 2011).

A growth mindset not only enabled managers to view subordinates' performance less biased (Heslin et al., 2005) but also motivated them to engage in more meaningful leadership behavior, such as inspiring a shared vision, challenging established processes, and helping their subordinates to achieve the desired outcomes (Kouzes & Posner, 2019). Believing that subordinates' characteristics are changeable also seems to motivate managers to coach their subordinates. Subordinates reported receiving more coaching from their managers when the managers had a growth mindset, rather than when the managers had a fixed mindset. Additionally, fixed-mindset managers who received a growth-mindset training were more willing to coach subordinates and provided more and higher-quality coaching suggestions (Heslin et al., 2006).

In conclusion, managers with a growth mindset were better able to detect behavioral changes in subordinates' performance (Heslin et al., 2005) and were more just in their performance appraisals (Heslin & VandeWalle, 2011). Seeing a potential for change further motivated managers to coach their subordinates (Heslin et al., 2005; Kouzes & Posner, 2019).

A Dark Side of the Growth Mindset? The Dual Pathway Model

The positive effects of a growth mindset on managerial coaching have led to recommendations to recruit growth-mindset managers or train fixed-mindset managers to adopt a growth mindset (Kouzes & Posner, 2019). While this strategy sounds promising at first glance, empirical literature also suggests it may not be without risks. This research describes the interpersonal effects of a growth mindset as being paradoxical (Graczyk et al., 2024) or double-edged (Hoyt et al., 2023). Particularly, work on mindsets of bodyweight, health and poverty (Burnette et al., 2017; Dohle et al., 2022; Graczyk et al., 2024) suggest that there is a dark side to growth mindsets, that is increased blame attributions towards others who are obese (Burnette et al., 2017), mentally or physically ill (Dohle et al., 2022), live in poverty (Graczyk et al., 2024) or show negative character traits such as aggression a lack of empathy or motivation (Ryazanov & Christenfeld, 2018). The reasoning behind these findings is that a growth mindset is strongly associated with control attributions towards the undesired attribute (Dohle et al., 2022; Ryazanov & Christenfeld, 2018). Within the framework of a growth mindset, it is reasonable to expect a person to change after experiencing setbacks, as the attribute is generally perceived as malleable. The resulting control attributions should lead to blame attributions towards others who do not change (Ryazanov & Christenfeld, 2018).

These increased blame attributions had further negative interpersonal implications, such as decreased social support for ill people (Dohle et al., 2022, Study 1, but not Study 2), or body shame and anti-fat attitudes towards obese people (Burnette et al., 2017).

Therefore, the question arises: why do some studies report positive consequences of a growth mindset in social interactions (e.g., Heslin et al., 2006; Heslin & VandeWalle, 2011), while others report negative consequences (e.g., Burnette et al., 2017; Dohle et al., 2022; Graczyk et al., 2024)? Research suggests that this is because mindsets elicit two counteracting interpersonal effects (Burnette et al., 2017; Dohle et al., 2022; Graczyk et al., 2024). First, it was shown that the consequences of a growth mindset for stigmatizing obese people are asymmetric (Burnette et al., 2017; Hoyt et al., 2017). Precisely, participants with a growth mindset of bodyweight believed in stronger responsibility for their own and other people's overweight and, as a consequence of that, experienced more body shame and anti-fat attitudes; however, they also believed in stronger offset efficacy, that is, the belief in another person's ability to successfully engage in activities that result in future weight loss. As a consequence, participants reported less body shame and less anti-fat attitudes. The two effects counteracted each other, so that there was no direct effect of a growth mindset on body shame or anti-fat attitudes (Burnette et al., 2017).

Similar counteracting effects were observed in the domain of mindsets about poverty, which the authors refer to as the "dual pathway model of interpersonal consequences of mindset", short: DUPA model (Graczyk et al., 2024, p. 630). On the one hand, a growth mindset intervention increased blame attribution towards people living in poverty, thereby decreasing helping intention. On the other hand, the growth mindset intervention also increased outcome expectancy, that is, the belief in the effectiveness of helping the person, and thereby helping intentions. The two opposing effects canceled each other out, resulting in no total effect of mindset on helping intentions (Graczyk et al., 2024). Another study on mindsets on poverty revealed that, particularly, a growth mindset on poverty as a trait, that is, someone's economic status and opportunities, was related to increased blame attributions. A growth mindset of poverty at the societal level, that is, that a society has a certain level of

poverty, about which little can be done, was not related to blame attributions towards individuals living in poverty (Hoyt et al., 2023).

Taken together, this research suggests that fostering a growth mindset among managers can be risky, as it may lead to unwanted interpersonal consequences, such as blaming subordinates who fail and being less willing to coach. Previous research showed that the two opposing paths cancel each other out (Dohle et al., 2022; Graczyk et al., 2024); however, in the managerial coaching research, there were positive effects of a growth mindset for managerial coaching found (Heslin et al., 2006; Kouzes & Posner, 2019). Thus, the question arises whether there is also a negative effect of a growth mindset among managers and, most crucially, whether there are situations in which the negative effect cancels out the positive effect, leading to more blame and less coaching.

This may, in fact, depend on the coachee's past performance. Research has shown that while single transgressions or failures do not seem to bother people with a growth mindset too much, they feel particularly disturbed by chronic failures (Ryazanov & Christenfeld, 2018). The authors showed that those with a growth mindset attributed more blame to another person who was consistently unempathetic, unmotivated, or aggressive. In their fourth study, they investigated how participants' mindsets shape their reactions toward the person who was either once or consistently unempathetic. It was shown that the mindset did not influence blame attributions toward the person who was unempathetic once. However, for the consistently unempathetic person, a growth mindset led to more blame attributions (Ryazanov & Christenfeld, 2018). Investigating continuous failure, in particular, is crucial, as change is difficult and individuals may fail to change (e.g., Ajzen, 1991; Heatherton & Nichols, 1994). Additionally, subordinates with a history of continuous failure may be in particular need of coaching. Blaming such subordinates and excluding them from the coaching process could harm the overall organization's performance.

In conclusion, a manager's growth mindset may lead to them having high outcome expectancies for their coaching, that is, the expectation that coaching will be successful, which will then motivate them to coach more. A growth mindset, however, may also lead managers to blame their subordinates for their failures, demotivating them to coach. The coachee's past performance may determine which of the two opposing paths is superior. For a coachee who has failed once, the outcome expectancy should cancel out the blame attributions, resulting in a greater willingness to coach. For a coachee who has continuously failed, blame attributions should cancel out outcome expectancy, resulting in a lower willingness to coach. Despite the relevance of understanding these effects of a managerial growth mindset, no research has yet investigated the applicability of the DUPA model (Graczyk et al., 2024) or the role of single versus continuous failure (Ryazanov & Christenfeld, 2018) in the context of managerial coaching.

Extended Abstract

Article 3: Mindsets at Work: Understanding the Positive Impact of Growth Mindsets on Workplace Coaching

Sophia Wingen, Thomas Graczyk, Tobias Wingen & Simone Dohle

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Coaching, and thereby developing subordinates, is becoming increasingly important in organizations. Employees who face challenges and fail can particularly benefit from such coaching. Coaching can be carried out by managers, colleagues, or external coaches and requires deciding whether to invest resources and effort in coaching another person. The mindset coaches or managers hold about whether or not others can change influences their decision to coach (Heslin et al., 2006; Kouzes & Posner, 2019). According to this perspective, coaches may either hold a growth mindset, which denotes the belief that personal attributes are malleable and can be developed over time, or a fixed mindset, which is characterized by the belief that personal attributes cannot be changed despite efforts for change (Dweck, 1999; Dweck & Yeager, 2019). In fact, previous findings showed that a growth mindset increases the quantity and quality of coaching (Heslin et al., 2006; Kouzes & Posner, 2019). However, a recently developed dual-pathway model of mindsets suggests that a growth mindset not only has positive interpersonal consequences but triggers two opposing effects (Dohle et al., 2022; Graczyk et al., 2024). According to this model, a growth mindset can increase the outcome expectancy for helping others, leading to more prosocial behavior. On the other hand, a growth mindset can also attenuate prosocial behavior by increasing blame attributions (Dohle et al., 2022; Graczyk et al., 2024). It is crucial to investigate the applicability of the dual-pathway model in coaching to better understand whether a growth mindset elicits not only positive but also negative reactions toward coachees. In addition, it is unclear under which conditions a growth mindset amplifies or attenuates the willingness to

coach. Research suggests that individuals with a growth mindset may have particularly adverse reactions toward others who continuously fail (Ryazanov & Christenfeld, 2018). Thus, the coachee's past performance (i.e., single or continuous failure) could shape whether a manager's growth mindset increases or decreases willingness to coach. There is a lack of research on how a coachee's past performance, such as continuous failure, influences the relationship between a growth mindset and the willingness to coach that person.

We aimed to make two important theoretical contributions to the coaching literature by deepening our understanding of how mindsets relate to willingness to coach. First, we investigated the roles of the two opposing underlying mechanisms, outcome expectancy and blame attributions. We hypothesized a positive indirect relationship between a growth mindset and the willingness to coach, mediated by outcome expectancy, and an opposing negative indirect relationship mediated by blame attributions. Secondly, we tested whether the strength of the two opposing paths was moderated by the coachee's past performance (i.e., single versus continuous failure). We expected the indirect path via outcome expectancy to be weaker for a coachee who had failed continuously than for one who had failed only once. We also expected the indirect path via blame attributions to be stronger for a coachee who failed continuously than for one who failed only once. Thus, we expected an overall negative relationship between a growth mindset and the willingness to coach a coachee who had failed continuously and an overall positive relationship for a coachee who had failed once.

We conducted four preregistered online experimental studies (Total $N = 2{,}293$) with German students (Study 1) and US non-student participants (Studies 2-4). In Studies 1, 2, and 4, we first measured the participants' mindset using the 'implicit kind of person theory' scale (Dweck, 1999). In Study 3, we additionally manipulated participants' mindsets (following Chiu et al., 1997). Second, participants were asked to imagine themselves as a coach in a

student peer-coaching program (Study 1) or a medium-level manager in a company (Studies 2–4). We then randomly assigned participants to read a short description of a coachee who either failed once or continuously. While the coachee failed for competence-related reasons in Studies 1-3, they failed for morality-related reasons in Study 4. Third, we measured participants' willingness to coach this person (Heslin et al., 2006) and the two mediating variables, blame attributions and outcome expectancy (Dohle et al., 2022).

Contrary to our expectations, we found no evidence that the coachee's past performance (single/continuous failure) moderated the relationship between the mindset and the willingness to coach across the four studies. Instead, a growth mindset was consistently associated with a greater willingness to coach. Regarding the hypothesized mechanism of outcome expectancy, mediation analysis showed a significant positive indirect path. Specifically, a growth mindset was associated with higher outcome expectancy, which, in turn, was linked to a greater willingness to coach. We did not find evidence regarding the hypothesized mechanism of blame attributions. Lastly, we could not show an effect of the mindset manipulation on outcome expectancy, blame attributions, or willingness to coach.

In conclusion, the dual-pathway model of mindsets could not fully be supported in the domain of coaching. In coaching, estimating the likelihood of success seems to be the key driver for the decision to coach. Taken together, our findings suggest that mindsets can be crucial in coaching. Organizations should promote growth mindsets among their managers and employees. In this way, the outcome of organizational coaching could be improved.

Professor Simone Dohle originated the research project and secured funding. I programmed the studies, collected and analyzed the data, and received feedback from Dr. Tobias Wingen and Professor Simone Dohle. I wrote the first draft of the manuscript, and Thomas Graczyk reviewed the analyses and edited the manuscript into a final draft. All authors then critically edited the final manuscript.

Study 1 of this article was part of my Master's Thesis, submitted at the Faculty of Human Sciences, at the University of Cologne, Germany. The research project extends the Master's thesis by conducting Studies 2–4 and publishing an article in a peer-reviewed journal.

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A very similar version of this extended abstract was submitted and accepted to the 47th IAREP SABE International Conference (2023) in Nice, France, where I presented it. Additionally, a blog post about this research was published in In-Mind magazine's blog, a leading science communication journal of the psychological research community.

Discussion

Constant challenges, such as demographic change (Hennings et al., 2025), a global pandemic (Carnevale & Hatak, 2020), or the spread of generative AI (Davenport & Srinivasan, 2026; Galetti, 2026), require employers to develop their workforce quickly and efficiently. One tool they have at hand to do so is managerial coaching. Despite existing research indicating that a growth mindset among managers can improve the quantity and quality of their coaching (Heslin et al., 2006), evidence from mindsets on other domains suggests that a growth mindset among managers may also lead to more blame towards struggling subordinates and subsequently less coaching (e.g., Dohle et al., 2022; Graczyk et al., 2024). This negative effect may be especially pronounced for subordinates with a series of failures, compared to those who failed only once (Ryazanov & Christenfeld, 2018). Despite the dramatic consequences that blame attributions and a lack of willingness to coach would have for organizational success, there was no research investigating the dual-pathway model of mindsets (Graczyk et al., 2024) in the coaching domain that also considered the coachee's past performance. In Project 3, we contributed to closing that gap by investigating the relationship between a growth mindset and willingness to coach, while also considering two counteracting mediators: blame attributions and outcome expectancies. Additionally, we experimentally manipulated the coachee's past performance to test whether the effects would differ between coachees who failed once and those who failed continuously. The results show that across all four studies, a growth mindset was positively related to willingness to coach, and that this relationship was mediated via outcome expectancies for coaching. We could not demonstrate a relationship between a growth mindset and blame attributions, nor a moderation of the positive association between a growth mindset and willingness to coach by history of failure (single vs. continuous), regardless of whether the coachee failed due to competence- or morality-related reasons.

Limitations

Before drawing theoretical and practical implications, several limitations of this research have to be considered. The first limitation refers to the generalizability of our findings. In our studies, we collected data from participants who reported, on average, having some coaching experience. We further described the person's past performance in a way that reflects office work. Our findings, therefore, cannot unambiguously be applied to other organizational settings. For example, highly experienced managers may base their outcome expectancies and willingness to coach more on their past coaching experience, while managers with little experience may rely more on their mindsets when making those decisions. Additionally, in settings other than office work, where responsibility for human lives is involved, such as doctors or pilots, managers with a growth mindset may be less forgiving of chronic failure, particularly after moral transgressions, and may then blame the failing person more.

Second, we used vignettes to describe the coachee's past performance to the participants. While such vignettes can increase internal validity and are recommended for variables that cannot be easily manipulated, they also limit external validity (Aguinis & Bradley, 2014). Additionally, research suggests that vignettes can amplify effect sizes, as the researcher can design powerful manipulations and the relevant information in the vignettes is highly salient (Murphy et al., 1986). Thus, the strength of the positive association between a growth mindset and willingness to coach, as well as its applicability to actual coaching relationships, needs further exploration. At this point, the study conducted by Heslin et al. (2006) provides valuable insights. Even though they did not investigate the applicability of the DUPA model (Graczyk et al., 2024) to the context of managerial coaching, they demonstrated the positive effect of a growth mindset on coaching in a more naturalistic setting using real manager-subordinate dyads and behavioral observations.

Lastly, while we did manage to experimentally change participants' mindsets in Study 3, this changed mindset did not have a direct effect on blame attributions, outcome expectancy, or willingness to coach. This effect may not be surprising, as a recent meta-analysis on mindset interventions showed that most interventions have a strong effect on the most proximal outcome, which is the mindset, and weaker effects on more distal outcomes (Burnette et al., 2023), which are, in our case, outcome expectancy, blame attributions, and willingness to coach. Thus, one explanation for this non-significant effect of the experimental manipulation could be that it was simply not strong enough to affect the mediator and outcome variable in our study. Another reasonable explanation is that through our manipulation, we primed a state-like mindset rather than shifting the underlying trait-like mindset. While mindsets are a stable, trait-like characteristic (Dweck et al., 1995), they also have a state-like aspect that can be situationally primed through experimental methods (e.g., Dohle et al., 2022; Graczyk et al., 2024; Ryazanov & Christenfeld, 2018). It is likely that the brief experimental manipulation in our study only primed a state-like mindset, which did not alter participants' fundamental motivational system, promoted through their stable mindsets. Potentially, the positive effect of a growth mindset on willingness to coach depends more on trait-like, stable mindsets, as it requires strong motivational processes rather than situationally primed mindsets. In line with that, research on mindsets about poverty has shown that a brief shift in mindsets does not necessarily lead to different motivational processes and is thus not necessarily related to participants' beliefs and attitudes (Hoyt et al., 2023).

Theoretical Implications and Suggestions for Future Research

The findings of this research are consistent with mindset theory (e.g., Dweck, 1999; Dweck et al., 1995) in general and findings on managers' mindsets (Heslin et al., 2006; Kouzes & Posner, 2019) in particular, which demonstrated that having a growth mindset motivates managers to invest in coaching their subordinates. Additionally, the findings show

a mechanism explaining this effect: increased outcome expectancies for coaching. The effect was robust, as it was replicated across four studies and not altered by subordinates' past performance. The findings thereby further emphasize the positive effect of a growth mindset in interpersonal encounters (e.g., Dweck et al., 1995), including professional ones (Heslin et al., 2005, 2006; Kouzes & Posner, 2019). While the findings strengthen the evidence for a positive effect of growth mindsets on the willingness to coach, they question the applicability of the DUPA model (Dohle et al., 2022; Graczyk et al., 2024) and conceptually similar models (Burnette et al., 2017; Hoyt et al., 2017, 2023) to the context of managers' mindsets and coaching.

Research on the detrimental interpersonal effects of growth mindsets is an emerging field. Although it has been shown that a growth mindset can amplify blame attributions towards others (Burnette et al., 2017; Dohle et al., 2022; Graczyk et al., 2024), it remains unclear what specific contextual requirements are necessary for this negative effect. Future work is needed to investigate those contextual requirements to fully understand the interpersonal implications of growth mindsets in general, and in the domain of coaching in particular.

Research suggests that it may depend on the type of help offered (Graczyk et al., 2024). In particular, it was shown that the positive effect of a growth mindset on helping intentions via outcome expectancies decreased when participants could only provide direct help (e.g., food, clothing, or money) rather than help to self-help (e.g., job opportunities). As coaching is a form of help to self-help, outcome expectancies may be more important to managers. For direct help, such as providing better work tools, the positive effect of growth mindsets may be attenuated. Future research should investigate whether considering to provide direct help to a subordinate leads to stronger blame attributions toward the subordinate, particularly after continuous failure. Another characteristic shaping the positive

effect of a growth mindset on prosocial behavior could be whether the behavior is altruistic or motivated by shared interests. While donating to a poor person (Graczyk et al., 2024) or supporting a stranger with health issues (Dohle et al., 2022) may be altruistically motivated, coaching within an organization is most likely not. Coaching a subordinate may increase individual and team performance, which is highly relevant to managers. In cases of such shared interests, people with a growth mindset may focus more on the potential outcomes of helping, in our case, coaching, and less on emotional processes, such as blaming the failing person.

Additionally, a subordinate's past coaching history may shape managers' attributions of blame. In actual manager-subordinate dyads, some form of coaching has likely already occurred but has been ineffective in improving the subordinate's performance. The findings of our study show that a growth mindset leads to a higher expectation that coaching will be effective. Thus, ineffective past coaching can conflict with the framework through which growth-mindset managers interpret the world, leading to negative responses, such as blaming the subordinate or withdrawing from further coaching. Future research should investigate whether a subordinate's coaching history moderates the association between growth mindsets and willingness to coach.

Lastly, research suggests that explaining a person's failure can alter the effect of a growth mindset on blame attributions (Graczyk et al., 2024). While this research focused on attenuating the effects of a growth mindset and thus explained to the participants that a person living in poverty is not responsible for their condition (Graczyk et al., 2024), it is possible that such an assumption is the default in an organizational setting. However, if a growth mindset manager learns that a subordinate is responsible for their failure, they may be more likely to blame them and, as a consequence, reduce their willingness to coach. Future

research should investigate the extent to which assigning responsibility claims for failure is related to growth-mindset managers blaming their subordinates.

Even if the effects of managers' growth mindsets on their blame attributions toward their subordinates are not found, future research should continue to investigate potential other downsides of managerial growth mindsets. First, the question arises whether the commitment of growth-mindset managers to coaching may escalate (see also Heslin et al., 2006). In our studies, growth-mindset managers were even more willing to coach someone who had repeatedly failed due to moral transgression. It is questionable to what extent coaching such a person, instead of dismissing them, is beneficial to the organization. More precisely, future research should investigate whether growth-mindset managers can accurately predict the effectiveness of coaching and withdraw resources if future change is unlikely. Such research could, for example, prompt participants to choose between coaching a subordinate and hiring a new person.

In addition, research should examine where different types of managers allocate their resources. As managerial resources are limited and managers often face competing priorities (Mariano, 2025), investigating willingness to coach in isolation cannot fully answer whether a growth mindset leads to greater managerial success. Investing time and effort in coaching subordinates will leave fewer resources available for other tasks. Additionally, a manager's growth mindset may also shape their ability to complete other managerial tasks. For example, research shows that having a growth mindset can shape a person's morality. Participants with a growth mindset cheated more in a logic task and made more discriminating decisions in a personnel selection task than participants with a fixed mindset (N. Huang et al., 2017). Another study explained a shift in moral awareness as a function of different moral schemas, depending on the mindset. While fixed-mindset participants held moral schemas related to fulfilling duties, they more strongly recognized duty-based violations as immoral. Growth-

mindset participants, on the other hand, held moral schemas more closely related to the general rights of the people involved and were more likely to identify right-based transgressions as immoral (Wurthmann, 2017). Thus, research should investigate the extent to which growth-mindset managers act ethically but also respond to ethical transgressions by their subordinates. Another important managerial task could be making financial decisions. It has been shown that a growth mindset can make people more risk-seeking in financial decision-making, as it emphasizes a promotion focus. A fixed mindset, on the other hand, emphasized a prevention focus, leading to lower financial risk-taking (Rai & Lin, 2019). Another study emphasizes that this pattern may reverse if the financial situation becomes more ambiguous, as fixed-mindset individuals were better able to integrate ambiguity when making financial decisions further (Montford et al., 2019). To fully understand the effect of a growth mindset on managerial performance, research should consider competing priorities and different tasks simultaneously. Field research with actual managers could help understand the relationship between growth mindsets and the completion of a wide range of managerial tasks.

Additionally, future research should investigate the extent to which growth mindsets managers can deliver *effective* coaching. Growth mindsets are lay theories of the changeability of attributes that do not necessarily reflect the actual changeability of those attributes (Dweck, 1999). Following that, growth-mindset managers may overemphasize changing the relevant attribute rather than subordinates' behaviors to best fulfill their work requirements, even when the attribute is not particularly pronounced. Take, for example, cognitive abilities. Research shows that cognitive ability is a very stable attribute (Breit et al., 2024). Managers with a growth mindset about cognitive ability may focus too much on developing their subordinates' cognitive abilities rather than their behavioral skills, such as structuring, planning, and using appropriate tools, to fulfill their work requirements. Such an

escalation of commitment can waste important managerial resources and can impose additional personal and psychological costs (Bolino et al., 2015; Bolino & Turnley, 2005).

Lastly, most managers in larger organizations are so-called middle managers, meaning they are responsible for a team but are also supervised and managed by someone at a higher level. Thus, research should investigate how managerial mindsets shape middle managers' roles as subordinates to their managers. In particular, it has been shown that subordinates' mindsets influence how well they react to their coaches' leadership style (Caniëls et al., 2018) or goal setting (Lin et al., 2017). For example, a coach with a promotion focus, meaning that they focused on ideal goals in relation to growth and extension, could increase performance, particularly for growth-mindset subordinates, as the framework of a growth mindset fits with that focus (Lin et al., 2017). Additionally, while a promotion focus was beneficial for both growth- and fixed-mindset subordinates, growth-mindset subordinates showed particularly low performance when the coach adopted a prevention focus, which prioritizes preventing failure (Sue-Chan et al., 2012). Thus, future research should consider the situation of middle managers and the extent to which their growth mindset simultaneously affects their roles as managers and subordinates.

Practical Implications

The findings of this study hold important practical implications. First, they demonstrate that a growth mindset can motivate managers to coach their subordinates. Following that, organizations should foster growth mindsets among their managers. They could do so through two strategies: First, strategic hiring and promoting personnel with a growth mindset. Numerous established mindset measures (Dweck, 1999) are available and can be easily employed in assessment centers. Second, they could also train managers with a fixed mindset to adopt a more growth-oriented mindset. In their study, Heslin et al. (2006) trained fixed-mindset managers in a 90-minute growth-mindset workshop. Even 6 weeks

after the workshop, those managers had a stronger growth mindset than those in the control workshop. This demonstrates that, although mindsets reflect chronic tendencies to interpret the world (Dweck, 1999; Dweck et al., 1995), they can be strategically altered through training. However, the effect of such training may differ across managers and contexts, and it should not be applied universally but targeted at managers who can benefit from it (Burnette et al., 2013). In particular, a recent meta-analysis indicates that growth-mindset interventions are most effective when individuals actually have some potential to improve (Burnette et al., 2023). Applying this to managerial coaching, particularly managers with struggling subordinates, may benefit from growth mindset training, whereas those managing high-performing subordinates may not be suitable candidates. Further, managers who are not coaching at an optimal level could be suitable targets.

Our findings also emphasize that organizations need to closely monitor their growth-mindset managers. As a growth mindset was associated with a higher willingness to coach, even towards a subordinate who failed due to persistent immoral behavior, there is concern that growth-mindset managers' commitment to coaching may escalate. In some cases, it may be more beneficial to a company's success to dismiss an unethical employee rather than invest further resources in coaching them. Even though our findings cannot draw a definitive conclusion on this, they suggest that organizations should monitor the coaching activities of their growth-mindset managers and provide them with additional guidance if dismissing an employee seems more rational than continuing coaching.

CONCLUSION

Regardless of how rapidly the economic conditions under which individuals operate change, the individuals themselves do not seem to change that quickly. The research included in this dissertation shows that fundamental human needs, such as the desire to feel a high sense of self-efficacy (Bandura, 1977, 1982) and the belief that a behavior can effectively shape an outcome (Bandura, 1977; Graczyk et al., 2024) motivate individuals to approach highly digitized environments, such as checkout-free shopping (Project 1) or to engage in changing an environment by coaching a subordinate (Project 3). Thus, to feel motivated to contribute to changing economic conditions, individuals need to feel capable of, or in control of, such change. Such a feeling of competence is a fundamental human need (Deci & Ryan, 2012) and may not disappear in a world of highly autonomous AI-enabled technologies. Additionally, this research indicates that, although some elements of the traditional world, such as checkouts, may seem cumbersome at first glance, they may serve a valuable function for consumers. In our case, the cumber at checkout seems to help consumers pause and focus on product prices, so they can later recall them correctly. Future developments in the digitization of work and consumption should adopt a psychological perspective, asking to what extent the implementation of new technologies overlooks fundamental human needs, which were satisfied by the original non-digital solution. Thereby, it can be more clearly understood to what extent consumers and employees will accept the new digital solution and whether it will have detrimental effects on them. Only by adopting that perspective will the rapid development of new technologies have a sustainable, positive effect on the economy and the individuals within it.

FULL RESEARCH MANUSCRIPTS OF PROJECTS 1 AND 2 AND THE ARTICLE OF PROJECT 3

I CAN, SO I WILL? CONSUMERS' SELF-EFFICACY AND THEIR INITIAL INTENTION TO PATRONIZE CHECKOUT-FREE STORES.

Sophia Wingen[1]

[1] University of Cologne,

Albertus-Magnus-Platz, 50923 Köln, Germany

sophia.wingen@wiso.uni-koeln.de

 0000-0001-8734-9026

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Abstract

Retailers worldwide are implementing checkout-free technology in their stores to improve the customer experience, save labor costs, and collect valuable data. However, consumer adoption remains below expectations. It is therefore crucial for retailers to understand the factors that influence consumers' initial intentions to patronize checkout-free stores. In Study 1 (N = 197), I therefore assessed the relationship between participants' self-efficacy and their patronage intentions for checkout-free stores in a cross-sectional survey. In Study 2 (N = 401), I conducted a between-subject online experiment to replicate Study 1's findings and test whether the salience of different characteristics of checkout-free stores influences participants' self-efficacy and patronage intentions. In Study 3 (N = 307), I conducted a between-subjects online experiment in which I manipulated self-efficacy by asking participants either complex or simple questions about checkout-free shopping to assess the causal effect on patronage intentions. A meta-analysis across all three studies shows a consistent, medium-to-large positive relationship between participants' self-efficacy and their initial patronage intentions. Additionally, prior experience with checkout-free shopping was positively related to self-efficacy and patronage intentions. The salience of different characteristics of checkout-free shopping did not influence participants' self-efficacy or their patronage intentions (Study 2). In Study 3, I successfully manipulated self-efficacy; however, the effect on patronage intentions was not significant. In conclusion, consumers' self-efficacy motivates their initial intentions to patronize checkout-free stores. Self-efficacy can be increased by experiencing success in a task related to checkout-free shopping.

Keywords: Self-efficacy, checkout-free shopping, patronage intentions, AI-enabled checkout, just-walk-out technology

Highlights

- A checkout-free shopping-specific measure of initial patronage intentions
- Reliable association between self-efficacy and patronage intentions of $r = .44$
- Positive association between prior experience and patronage intentions

I Can, so I Will? Consumers' Self-Efficacy and Their Initial Intention to Patronize Checkout-Free Stores.

Imagine going into a grocery store, grabbing the items you need, and just leaving without any of the typical issues: Waiting in line at checkout, unpacking and repacking products, interacting with cashiers, or handling payment. This frictionless shopping experience was Amazon's vision for introducing checkout-free shopping in its *Amazon Fresh* grocery stores (Wingfield, 2018). Six years later, in 2024, however, there seems to be disagreement in the retail sector about the potential of checkout-free shopping. While industry experts still regard it as the most important innovative technology in the grocery retail sector (Edsall et al., 2023), and large retailers continue to invest in it, even in large-scale stores (e.g., Rewe, 2024), Amazon changed its strategy by removing checkout-free technology from its large-scale grocery stores (Day, 2024; Kumar, 2024). The hype around Amazon Go has subsided, and consumer adoption of Amazon Fresh stores has remained below expectations (Kumar, 2024). This raises the question of whether and how consumer adoption of checkout-free shopping can be ensured, thereby enabling retailers to turn their investment into profit.

The very first patronage decision is thereby critical because if consumers never patronize a store, all in-store efforts towards convenience, store atmosphere, product range, and pricing become pointless. Initial research has shown that understanding patronage intentions towards checkout-free stores is complex, as they are influenced by store characteristics (Benoit et al., 2024; Nam et al., 2025), general attitudes (Pillai et al., 2020), or specific reasons for the adoption, such as convenience expectations or expected reduced social judgment (Hazée et al., 2025). Further, research indicates that consumers' reactions strongly depend on their own characteristics (Cui et al., 2022; Sohn, 2024; van Esch et al., 2021a, 2021b). For example, self-efficacy, that is, consumers' estimation of their ability to

shop successfully in these stores (Bandura, 1977, 1982; Cassidy & Eachus, 2002), seems to shape consumers' perception of these stores (Sohn, 2024; van Esch et al., 2021a). It is reasonable to assume that self-efficacy also affects consumers' initial decision to patronize checkout-free stores, as it plays a crucial role in consumers' adoption of new technologies (e.g., Hentzen et al., 2022; Kulviwat et al., 2009; Mani & Chouk, 2017; C. Wang et al., 2013). However, so far, self-efficacy has been investigated only as a predictor of risk- and convenience perceptions of checkout-free stores (Sohn, 2024; van Esch et al., 2021a) or as a moderator of the positive effect of checkout-free shopping relative to other checkout methods on shopping intentions (van Esch et al., 2021a), but not as a direct antecedent of patronage intentions towards checkout-free stores. Investigating this is crucial, as managers may be investing in store design, product assortment, and marketing, while, in fact, consumers' basic assumption about whether they can successfully use checkout-free stores is decisive for their initial patronage. Managers could use this knowledge to run more targeted campaigns, such as segmenting customers effectively or boosting self-efficacy among potential customers. In this research project, I investigated the association between consumers' self-efficacy and patronage intentions in three studies with over 900 participants. Further, and to this end, I developed a checkout-free shopping-specific measure of initial patronage intentions that captures a variety of relevant behavioral aspects. Additionally, I provided insights into the characteristics of consumers who report lower self-efficacy.

Literature Review

Checkout-Free Shopping and Consumers' Patronage Intentions

Retailers particularly invest in innovative *checkout* technologies, as checkout is a significant pain point for customers. At conventional checkouts, they must wait in line, handle product packaging, endure unwanted social interactions, and process payment

(Wolniak et al., 2024), all of which can cause frustration and an uncomfortable feeling about spending money (Reshadi & Fitzgerald, 2023).

Self-checkouts emerged as an initial attempt to improve the checkout experience. At self-checkouts, customers use designated terminals or handheld devices to scan their selected products (Wolniak et al., 2024). Although self-checkouts offer customers autonomy, reduce unwanted social contact, and save time, they are not necessarily more convenient, as customers must handle more tasks at the checkout than in conventional stores (Wolniak et al., 2024). Additionally, they bear the burden of responsibility for the checkout process and speed. Consequently, consumers felt ambivalent about self-checkouts (Bulmer et al., 2018).

While self-checkout seems to leave consumers torn between convenience and frustration (Bulmer et al., 2018; Van Steenburg et al., 2013), checkout-free shopping may resolve that ambiguity by handing over the checkout process to an autonomous AI system rather than to customers. Checkout-free shopping uses AI-enabled cameras and vision-sensor technology to track customers' purchases and autonomously process checkout and payment (e.g., Cui et al., 2022). To use checkout-free shopping, customers must register upon entering the store using an app or biometric data (Schultz & Paetz, 2025). Subsequently, they can select their products and leave the store without stopping at a checkout to pay. The amount will be automatically charged to their connected accounts (see *Just Walk Out*, n.d.). Retailers implement checkout-free technology to enhance the customer experience in their stores, reduce labor costs, gather valuable data, and finally secure a competitive advantage (Wolniak et al., 2024).

Regardless, consumers are hesitant to adopt checkout-free technology (Kumar, 2024), suggesting that their reactions are not uniformly positive. Accordingly, retailers urgently seek to understand consumers' attitudes toward checkout-free stores and how to increase patronage.

In response, numerous research groups have published studies on consumer reactions to checkout-free stores (for an overview, see Hazée et al., 2025). The majority of articles are concerned with consumers' perception of checkout-free stores and consumer traits that shape these perceptions (Cui et al., 2022; Sohn, 2024; van Esch et al., 2021a, 2021b), and ultimately purchase and patronage intentions (e.g., Benoit et al., 2024; Nam et al., 2025; Pillai et al., 2020; Schultz & Paetz, 2025).

Research shows that checkout-free stores can indeed be perceived positively by consumers. For example, consumers perceived the store atmosphere in a checkout-free store more favorably than in a store that uses self-checkout terminals (Cui et al., 2022; van Esch et al., 2021b) or conventional checkout (van Esch et al., 2021b) and rated the store as being more convenient (van Esch et al., 2021a).

Further, these studies demonstrate that understanding consumers' patronage intentions or related concepts, such as purchase intentions of specific products, is complex, as they were shaped by attitudes towards the stores, such as usefulness perceptions (Pillai et al., 2020), various reasons for, such as reduced social judgment, or against patronage such as concerns about the technology`s reliability (Hazée et al., 2025), and store-specific characteristics such as onsite staff or the registration requirements (Benoit et al., 2024). Additionally, research shows that checkout-free shopping *can* be exciting to consumers and increase store patronage (e.g., van Esch et al., 2021a, 2021b). However, it was not well received by all consumers; some viewed the associated AI technology as threatening (van Esch et al., 2021b), attributed risks to checkout-free shopping (Sohn, 2024), or felt incapable of handling it (van Esch et al., 2021a). These individual characteristics shape the positive effects that checkout-free shopping has on consumers. For example, checkout-free shopping increased shopping intentions for specific products compared to self-checkouts only for consumers who did not perceive AI as threatening (van Esch et al., 2021b). Furthermore, checkout-free shopping did

increase purchase intentions compared to self-checkouts only among participants with high self-efficacy (van Esch et al., 2021a).

Thus, a better understanding of factors that potentially increase patronage intentions, particularly consumers' self-efficacy, remains a top research priority. Additionally, the existing studies have an important shortcoming. Patronizing a checkout-free store, especially for the first time, requires more than just the intention to purchase specific products. Initial patronage at a checkout-free store also requires prior registration with the store and installation of an app. If the store is still unfamiliar to the customer, the customer also needs to find out where it is located and walk past it. Following that, simply assessing customers' intentions to purchase products in a checkout-free store may not fully capture their initial patronage intentions. It remains unclear how all those behavioral aspects of initial patronage are shaped.

Consumers' Self-Efficacy

Self-efficacy motivates people's actions: It informs their decisions on what action to undertake, how much effort to invest, and how long to persist in the face of difficulties (Bandura, 1982). Bandura (1982) defined perceived self-efficacy as "[…] judgments of how well one can execute courses of action required to deal with prospective situations" (p.122). Following these judgments, people tend to undertake activities they believe they can manage and avoid those they believe exceed their capabilities (Bandura, 1982).

A positive relationship between self-efficacy and technology adoption has been demonstrated across various innovative technologies, including retirement apps (Hentzen et al., 2022), smartwatches (Mani & Chouk, 2017), and tablet devices (Kulviwat et al., 2009). Furthermore, a longitudinal study on self-checkouts found that self-efficacy was particularly important for the initial intention to use them (C. Wang et al., 2013).

For checkout-free shopping, self-efficacy shaped participants' perceptions of checkout-free stores, reducing perceived risks (Sohn, 2024) and increasing perceived convenience (van Esch et al., 2021a). This research demonstrates that, besides checkout-free shopping being designed to work intuitively and seamlessly, not all consumers felt capable of using it (Sohn, 2024; van Esch et al., 2021a). Nevertheless, the direct relationship between self-efficacy and initial patronage intentions toward checkout-free stores has not yet been investigated. In this article, I report the findings of three studies to close that knowledge gap.

General Approach Across all Studies

Across all studies, I used the platform Prolific to recruit UK-based participants. Participants could participate in only one of the studies. The studies were preregistered (https://aspredicted.org/ym92x7.pdf, https://aspredicted.org/rr2mu3.pdf, https://aspredicted.org/4zw8xd.pdf) and the experimental methods were approved by the Ethics Committee of the Faculty of Management, Economics and Social Sciences (ERC-FMES) at the University of Cologne, Germany. The data, analysis code, and questionnaire of the studies have been made publicly available on OSF (https://osf.io/9wm23/overview?view_only=2c71a3b01fe24541bad970621d36271d). For data analyses, I used the software *R*.

Study 1

In Study 1, I conducted a cross-sectional survey to test the hypothesized positive link between self-efficacy and patronage intentions for a checkout-free store.

Method

I recruited a sample of $N = 197$ participants after applying preregistered exclusion criteria ($M_{age} = 44.48$, $SD_{age} = 14.15$). Ninety participants reported being female, 105 reported being male, and one reported being non-binary/third gender ($n = 196$). 26

participants had some checkout-free shopping experience (M = 4.85, SD = 1.78), and 34 knew someone who did (M = 5.29, SD = 1.66).

In the study, participants first read a vignette explaining checkout-free shopping and noting that various UK retailers have already opened such stores. They then imagined a checkout-free store near their location and indicated their self-efficacy and patronage intentions in random order. Lastly, the demographic and control variables were assessed.

Material

Vignette. I used a short text to explain the concept of checkout-free shopping to the participants (see Appendix A.1.1.1).

Self-efficacy. Self-efficacy was measured using five items adapted from the Computer User Self-Efficacy scale (Cassidy & Eachus, 2002). An example item is: "I am very unsure of my abilities to use checkout-free shopping". Participants rated the items on a scale from 1 (*strongly disagree*) to 7 (*strongly agree*); the items were averaged to form a composite score (α = .87).

Initial patronage intentions. The patronage intentions scale was developed following Ajzen's (2019) recommendations, which require not only defining the action itself but also the target, context, and time of the behavior. Accordingly, four different behavioral aspects of the initial patronage intentions were specified: 'I will find out where the checkout-free store close to my location is,' 'I will download the app to visit the checkout-free store close to my location,' 'I will walk by the checkout-free store close to my location,' and 'I will enter the checkout-free store close to my location to purchase groceries'. Participants stated their inclination to perform each behavior within the *next month* on 7-point scales ("*unlikely"* to "*likely";* "*not probable"* to "*very probable"*; and "*impossible"* to "*very possible"*) adopted from the behavioral intention general scale (Bruner, 2009). Following an exploratory factor analysis, the four behaviors were averaged to form a composite score (α = .96).

Demographic and control variables. I asked participants for their understanding and immersion in the study's scenario (on 7-point scales), as well as their age, gender, and any experience they or someone they know has with checkout-free shopping (on 7-point scales). Lastly, following Meade and Craig (2012), I asked participants whether I should use their data in the analysis (*Yes/No*).

Results

Participants found the scenario easy to understand ($M = 6.71$, $SD = 0.54$) and found themselves well immersed in it ($M = 6.55$, $SD = 0.74$).

Hypothesis Test

In support of my hypothesis, a linear regression analysis showed a significant positive relationship between self-efficacy and patronage intentions ($b = 0.73$, $SE = 0.09$, 95% CI [0.55, 0.90], $p < .001$, $\beta = .50$, $R^2 = .25$).

This relationship was robust even when controlling for age, gender, experience with checkout-free shopping, and order effects of the self-efficacy and patronage intentions measurement ($b = 0.59$, $SE = 0.09$, 95 % CI [0.42, 0.77], $p < .001$, $\beta = .42$). Furthermore, age was negatively related to patronage intentions ($b = -0.03$, $SE = 0.01$, 95% CI [-0.05, -0.01], $p = .001$, $\beta = -.21$). In contrast, experience was positively related to patronage intentions ($b = 1.08$, $SE = 0.31$, 95% CI [0.47, 1.70], $p = .001$, $\beta = .21$).

Exploratory Analysis

To elaborate on which participants exhibited low self-efficacy, I predicted self-efficacy from age, gender, and experience. Age was unrelated to self-efficacy ($b = -0.01$, $SE = 0.01$, 95% CI [-0.02, 0.01], $p = 0.335$, $\beta = -.07$). Men reported higher self-efficacy than women ($b = 0.43$, $SE = 0.19$, 95% CI = [0.05, 0.81], $p = .028$, $\beta = .15$). Finally, experience with checkout-free shopping was positively related to self-efficacy ($b = 0.86$, $SE = 0.25$, 95% CI [0.37, 1.35], $p = .001$, $\beta = .24$).

Discussion

Participants' self-efficacy was positively associated with their intentions to patronize checkout-free stores. This raises the question of where participants' (un)certainties regarding their self-efficacy stem from. Since they had limited experience with checkout-free shopping, it is unlikely that they derived their self-efficacy judgments directly from positive or negative experiences (following Bandura, 1982). Instead, they must have considered other salient information to estimate their self-efficacy (following Ajzen, 1991).

Study 2

I conducted a between-subjects experiment to replicate Study 1's findings (H1) and examine the information participants may consider when making judgments about self-efficacy and patronage intentions. Based on previous literature, I identified four characteristics of checkout-free stores to be relevant here: (1) high convenience, (2) innovative AI technology, (3) low payment transparency, and (4) absence of human staff.

(1) The *high convenience* of checkout-free stores is typically the main selling point of the technology to customers (Wolniak et al., 2024), and convenience was positively associated with self-efficacy regarding checkout-free shopping (van Esch et al., 2021a). Additionally, research suggests that convenience is a criterion for participants' store choice (Pan & Zinkhan, 2006) and patronage intentions (Benoit et al., 2024). Following that, I hypothesized that highlighting the convenience of checkout-free stores increases patronage intentions (H2a) and self-efficacy (H3a) compared to a baseline. This positive effect of highlighting the convenience of checkout-free stores on patronage intentions should be mediated by increased self-efficacy (H4a).

(2) Highlighting the use of *innovative AI technology* should reduce self-efficacy, as participants have little experience with this innovation and may find it intimidating (following Bandura, 1982; van Esch et al., 2021b), and downstream patronage intentions. The

AI technology used in checkout-free stores was seen as threatening by some participants. When people perceived it as threatening, checkout-free shopping did not enhance their experience or patronage intentions (van Esch et al., 2021b). Additionally, consumers preferred stores less, as they adopted more advanced technologies (Nam et al., 2025). Thus, I hypothesized that highlighting the innovative AI technology of checkout-free stores reduces patronage intentions (H2b) and self-efficacy (H3b) compared to the baseline. This negative effect of highlighting the innovative AI technology of checkout-free stores on patronage intentions should be mediated by decreased self-efficacy (H4b).

(3) In a checkout-free store, customers neither undergo a payment process nor have the opportunity to verify the final amount. Highlighting this *reduced payment transparency* (see Soman, 2003) should reduce self-efficacy, as budget management becomes more difficult. Participants indeed reported feeling payment unease in checkout-free stores—that is, the "perceived loss of control over the payment" (Hazée et al., 2025, p. 995). This payment unease was mentioned as an important reason against adopting checkout-free stores (Hazée et al., 2025). Extending this, participants preferred to verify the final spending amount and therefore favored self-checkout over fully automated checkout methods (Benoit et al., 2024). Thus, I hypothesized that highlighting the low payment transparency reduces patronage intentions (H2c) and self-efficacy (H3c) compared to a baseline. This negative effect of highlighting the lowered payment transparency of checkout-free stores on patronage intentions should be mediated by decreased self-efficacy (H4c).

(4) Highlighting the *absence of human staff* in a checkout-free store should lower self-efficacy, as participants may be unsure about available help if problems arise. Research regarding the absence of human staff in checkout-free stores is mixed. While reduced unwanted social interaction and judgment may encourage patronage of checkout-free stores, the lack of assistance in the event of obstacles was mentioned as a reason against adoption

(Hazée et al., 2025). Additionally, Benoit et al. (2024) demonstrated that consumers valued on-site staff and showed higher patronage intentions for stores that offer them. Following that, I hypothesized that highlighting the absence of human staff reduces patronage intentions (H2d) and self-efficacy (H3d) compared to a baseline. The negative effect of highlighting the absence of human staff in checkout-free stores on patronage intentions should be mediated by decreased self-efficacy (H4d).

Method

I recruited a sample of $N = 401$ participants, after applying preregistered exclusion criteria ($M_{age} = 43.64$, $SD_{age} = 12.70$). 215 participants reported being female, 181 reported being male, 4 reported being non-binary, and 1 participant did not report their gender. 38 participants had some prior checkout-free shopping experience ($M = 3.03$, $SD = 1.70$), and 60 participants knew someone who did ($M = 3.60$, $SD = 1.72$).

Participants were randomly assigned to read one of five short texts explaining checkout-free shopping, each highlighting one of the characteristics outlined above. Subsequently, their self-efficacy and patronage intentions were measured in a randomized order, followed by the demographic and control questions.

Material

Salience Manipulation. To manipulate the salience of the different characteristics of checkout-free shopping, participants were randomly presented with one of five short texts about checkout-free stores, each highlighting a different characteristic. One group served as a baseline and received the neutrally toned text from Study 1. The other four groups received adaptations of that baseline text that either emphasized the *high convenience*, *use of innovative AI technology*, *low payment transparency*, or the *absence of human staff* in checkout-free stores (see Appendix A, Table A.2.1.1 for the complete texts).

Measures. The measures for participants' self-efficacy (α = .88) and initial patronage intentions (α = .94), along with the demographic and control variables, were the same as in Study 1. Finally, I added an attention check: Participants had to select which aspect was highlighted in the description they had read (neutral tone/convenience/innovation/low payment transparency/or absence of human staff).

Results

Participants found the scenario easy to understand ($M = 6.69$, $SD = 0.57$) and found themselves well immersed in it ($M = 6.51$, $SD = 0.75$). The attention check revealed a significant difference between the groups ($\chi^2(16, N = 401) = 461.72$, $p < .001$; see Appendix A; Table A.2.2.1). The descriptive statistics for self-efficacy and patronage intentions across the five conditions are presented in Table 1.

Table 1

Descriptive Statistics of Self-Efficacy and Patronage Intentions

		Self-efficacy		Patronage intentions	
Group	*n*	*M*	*SD*	*M*	*SD*
Baseline	81	5.36	1.42	4.43	1.90
Convenience	80	5.50	1.39	4.60	1.97
Use of innovative AI technology	81	5.36	1.37	4.23	1.97
Absence of human staff	79	5.51	1.41	4.69	1.81
Low payment transparency	80	5.26	1.34	4.44	1.66

Hypothesis Test

In support of Hypothesis 1, a linear regression analysis showed a positive and significant relationship between self-efficacy and patronage intentions (b = 0.59, SE = 0.06, 95% CI [0.47, 0.71], p < .001; β = 0.44, R^2 = .19).

This relationship was robust even when controlling for age, gender, experience with checkout-free shopping, and order effects of the self-efficacy and patronage intentions measurement (b = 0.57, SE = 0.06, 95 % CI [0.45, 0.69], p <.001, β = 0.42). Once again, age was negatively related to patronage intentions (b = -0.02, SE = 0.01, 95% CI [-0.03, -0.01], p = .005, β = -.13), while experience was positively related to patronage intentions (b = 0.51, SE = 0.23, 95% CI [0.06, 0.96], p = .026, β = 0.10).

To test Hypotheses 2 and 3, I conducted two analyses of variance (ANOVAs) with patronage intentions (H2a-d) and self-efficacy (H3a-d) as outcomes and the salience of different characteristics as predictors. The first ANOVA (patronage intentions) did not return a significant result (F(4, 396) = 0.72, p = .581, η^2 = .01, Cohen's f = 0.09). The second ANOVA (self-efficacy) also did not return a significant result (F(4,396) = 0.46, p = .763, η^2 = .00, Cohen's f = 0.07). Additionally, none of the planned contrasts returned a significant result (see Appendix A.2; Table A.2.2.2). Including whether or not participants passed the attention check (Yes = 234, No = 167), did not meaningfully alter the results (See Appendix A Table A.2.2.3). To determine what size of effect the study could have revealed, I conducted a post-hoc sensitivity analysis using G*Power (α = .05, 1-β = .80, N = 400, k = 5, Faul et al., 2007) which revealed that the study could have found even small effects (Cohen's f = 0.17; Cohen, 1992). Therefore, the non-significant finding may reflect either the absence of an actual effect or a very small effect.

Consistent with these results and contrary to Hypotheses 4a-d, mediation analysis in which self-efficacy was specified as the mediator and patronage intentions as the dependent

variable (using *lavaan;* Rosseel, 2012*, 10,000 bootstraps*) revealed that none of the indirect effects were significant (Appendix A; Table A.2.2.4).

Exploratory Analysis

I predicted self-efficacy from age, gender, and experience with checkout-free shopping. The analysis revealed a significant negative relationship between age and self-efficacy (b = - 0.01, SE = 0.01, 95% CI [-0.02, -0.00], p = .021, β = -.11). Further, men reported having higher self-efficacy then women (b = 0.49, SE = 0.13, 95% CI [0.22, 0.76], p < .001, β = .18), and participants with prior experience with checkout-free shopping also reported higher self-efficacy (b = 0.54, SE = 0.18, 95% CI [0.18, 0.90], p = .004, β = .14).

Discussion

This study replicated Study 1's finding of a positive relationship between self-efficacy and patronage intentions. Emphasizing specific characteristics of checkout-free stores did not affect either variable. The results raise questions about whether communicating store characteristics can effectively influence consumers' self-efficacy.

Study 3

In Study 2, highlighting different characteristics of checkout-free stores did not alter participants' self-efficacy. Instead, actual experiences of mastery or failure in a task related to checkout-free shopping may be more impactful (following Bandura, 1982). In Study 3, I therefore conducted a between-subjects experiment in which I manipulated self-efficacy without deception by asking participants to respond to either complex or simple questions about checkout-free stores. The aim was for participants to experience failure (complex questions) or mastery (simple questions) in a task related to checkout-free shopping, ultimately testing the hypothesis that self-efficacy increases patronage intention. A pretest (n = 139) revealed a small but significant effect of the manipulation on self-efficacy (d = 0.34, 95% CI [0.00, 0.68]; see Appendix A.3.1.1).

Method

I recruited $N = 307$ participants after applying preregistered exclusion criteria to detect a small effect ($M_{age} = 45.64$, $SD_{age} = 13.99$). 170 participants identified as female, and 137 identified as male. 23 participants had some prior checkout-free shopping experience ($M = 3.70$, $SD = 1.69$), and 37 knew someone who did ($M = 4.03$, $SD = 1.67$). Participants first read the short text about checkout-free shopping used in Study 1. They were then randomly assigned to the low- or high-self-efficacy condition and responded to the corresponding six questions. Subsequently, their patronage intentions were measured, followed by the manipulation check, and then by demographic and control questions.

Material

Manipulation of self-efficacy. Depending on their condition, participants answered either six complex (low self-efficacy) or six simple (high self-efficacy) questions about checkout-free shopping in an open-ended format. A complex example question was: "What happens if I am charged for a product I didn't take?". A simple example question was: "Do I need to install an app to use checkout-free shopping?" (see Appendix A; Table A.3.1.1 for all questions).

Measures. The measures, initial patronage intentions ($\alpha = 0.95$), demographic and control variables, and self-efficacy as a manipulation check ($\alpha = 0.88$), were the same as in Studies 1 and 2.

Results

Participants found the scenario was easy to understand ($M = 6.28$, $SD = 0.96$); however, participants in the high-self-efficacy condition rated the scenario as easier to understand ($M = 6.50$, $SD = 0.77$) than participants in the low-self-efficacy condition ($M = 6.03$, $SD = 1.08$), $t(262.03) = 4.36$, $p < .001$, $d = 0.51$, 95% CI [0.28, 0.73]). Further, participants found themselves well immersed in the scenario ($M = 6.11$, $SD = 1.15$); again,

participants in the high-self-efficacy condition perceived themselves to be more immersed in the scenario (M = 6.31, SD = 1.03) than participants in the low-self-efficacy condition (M = 5.88, SD = 1.23, $t(305) = 3.31$, $p = .001$, $d = .38$, 95% CI [0.15, 0.60]).

Manipulation Check

The manipulation functioned as intended: Participants in the high self-efficacy condition reported higher self-efficacy (M = 5.31, SD = 1.37) than those in the low self-efficacy condition (M = 4.86, SD = 1.44, $t(305) = 2.84$, $p_{one\text{-}sided} = .002$, $d = 0.32$, 95% CI [0.10, 0.55]).

Hypothesis Test

Participants in the high-self-efficacy group reported higher patronage intention (M = 4.13, SD = 2.01) than participants in the low-self-efficacy group (M = 3.76, SD = 1.92). However, this difference was not significant ($t(305) = 1.64$, $p_{one\text{-}sided} = .051$, $d = 0.19$, 95% CI [-0.04, 0.41]).

Exploratory Analysis

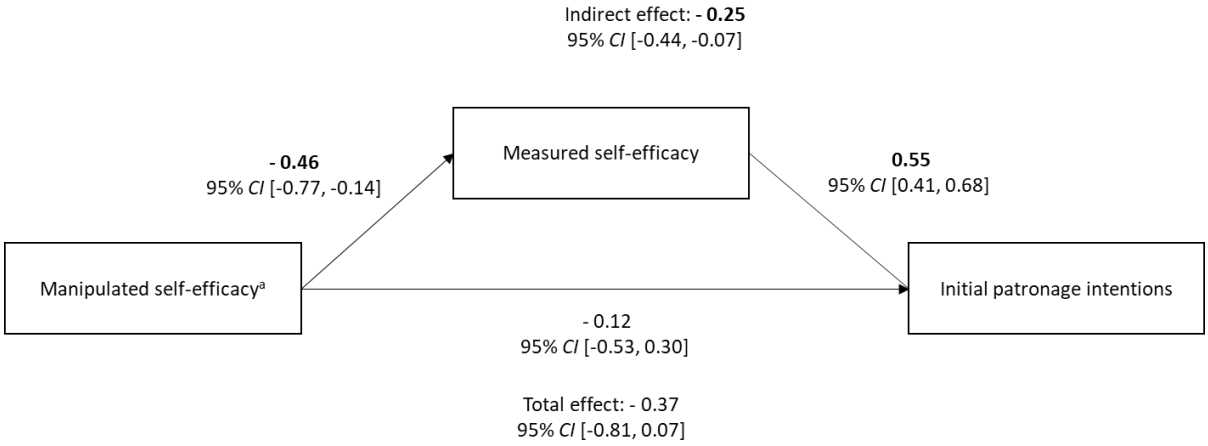
To replicate my findings from Studies 1 and 2, I analyzed whether the measured self-efficacy (measured as the manipulation check) was related to participants' patronage intentions. A linear regression analysis showed that measured self-efficacy was positively associated with patronage intentions ($b = 0.55$, $SD = 0.07$, 95% CI [0.41, 0.70], $p <. 001$, $\beta = .40$, $R^2 = 0.16$). This relationship was robust even when controlling for age, gender, experience with checkout-free shopping, understandability and immersion ($b = 0.52$, $SE = 0.09$,95 % CI [0.35, 0.69], $p <.001$, $\beta = .38$), furthermore, experience was positively related to patronage intentions ($b = 0.71$, $SE = 0.30$, 95% CI [0.11, 1.31], $p = .020$, $\beta = .12$).

I included the measured self-efficacy (manipulation check) as a mediator between the experimental group and patronage intentions. The mediation analysis (*lavaan package, Rosseel, 2012, 10,000 bootstraps*) revealed a significant indirect effect of the experimental

group on patronage intentions via measured self-efficacy (b = -0.25, SE = 0.09, 95% CI [-0.44, -0.07], β = -0.06) and no remaining direct effect of the experimental group on patronage intentions (b = -0.12, SE = 0.21, 95% CI [-0.53, 0.30], β = -.03), supporting a full mediation (See Figure 2).

Figure 2

Results of the Mediation Model with Measured Self-Efficacy as a Mediator



Note. N = 307, [a] 0 = high self-efficacy, 1 = low self-efficacy, coefficients represent b, bold coefficients differ significantly from zero (p <.05)

Lastly, I predicted self-efficacy from age, gender, and experience. This analysis revealed that men reported a higher self-efficacy than women (b = 0.37, SE = 0.16, 95% CI [0.05, 0.68], p = .025, β = .13).

Discussion

The findings show that highlighting participants' (lack of) knowledge shapes their perceived self-efficacy. The hypothesized effect on patronage intentions, however, was not significant. Nevertheless, the study replicates findings from Studies 1 and 2 by showing a positive and robust relationship between measured self-efficacy and patronage intentions.

Including the measured self-efficacy as a mediator between participants' experimental condition and patronage intentions yielded a significant indirect effect. This may suggest that answering complex or simple questions influences patronage intentions, but only among participants who experienced the intended shift in self-efficacy.

Mini Meta-Analytic Summary

Although the experimental effect of self-efficacy on initial patronage intentions was not significant, the correlation between them was consistently observed. To obtain a reliable estimate of the positive relationship between self-efficacy and patronage intentions found in Studies 1 to 3 ($N = 905$, three studies, eight groups, see Table 2 for the correlations in each condition), I conducted a multilevel meta-analysis using a random-effects model (metafor; Viechtbauer, 2010).

Table 2

Correlation between Self-Efficacy and Patronage Intentions for Studies 1 - 3

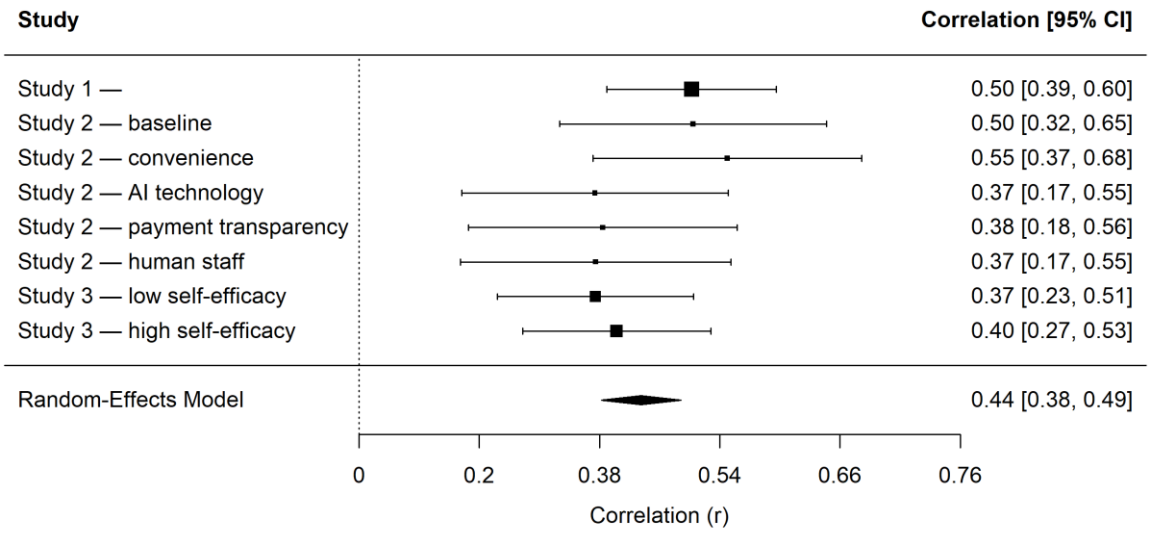
				95% *CI*		
Study	Group	*n*	$r_{(Pearson)}$	*LL*	*UL*	*p*
Study 1	–	197	.50	0.299	0.488	< .001
Study 2	Baseline	81	.50	0.322	0.651	< .001
	Convenience	80	.55	0.371	0.684	< .001
	Use of innovative AI technology	81	.37	0.169	0.547	< .001
	Absence of human staff	79	.38	0.180	0.557	< .001
	Low payment transparency	80	.37	0.167	0.550	< .001
Study 3	Low self-efficacy	147	.40	0.266	0.526	< .001
	High self-efficacy	160	.37	0.226	0.505	< .001

Note. LL = lower limit; UL = upper limit

The meta-analysis returned homogenous effects across all studies and conditions ($Q(7) = 5.84$, $p = .559$, $\tau_{between}^2 = 0.00$, $\tau_{within}^2 = 0.00$) and a mean Fisher's z of 0.47 ($SE = 0.03$, $p < .001$, 95% *CI* [0.40, 0.54]), corresponding to a medium to large correlation of $r = .44$, 95% *CI* [0.38, 0.49]. The results of the meta-analysis are presented in Figure 3.

Figure 3

Forest Plot of the Meta-Analysis of the Correlations between Self-Efficacy and Patronage Intentions of Studies 1 - 3



General Discussion

My findings show a consistent, medium-to-large positive association between self-efficacy and initial patronage intentions for checkout-free stores. Contrary to expectations, consumers' self-efficacy and initial patronage intentions were not significantly influenced by the salience of different characteristics of checkout-free stores (Study 2). Instead, and in line with expectations, answering simple questions increased self-efficacy, while answering complex questions reduced it. This manipulation, however, had no significant effect on patronage intentions (Study 3).

These findings make important contributions to existing research. First, this study is the first to investigate self-efficacy as an antecedent of initial patronage intentions for checkout-free stores and to provide a reliable estimate of this relationship. I thereby complement research demonstrating that self-efficacy is associated with greater technology adoption in general (Hentzen et al., 2022; Kulviwat et al., 2009; Mani & Chouk, 2017) and with positive perceptions of checkout-free stores in particular (Sohn, 2024; van Esch et al., 2021a). Further, this research consistently demonstrated that prior experience with checkout-free shopping was positively associated with higher self-efficacy and patronage intentions.

It is important to consider at least two limitations. First, the robust relationship between self-efficacy and patronage intentions relies on correlations, and thus, causality should be interpreted with caution. One explanation for the non-significant effect of the self-efficacy manipulation in Study 3 is that the experimental manipulation altered participants' task-*specific* self-efficacy, whereas the measured self-efficacy captured participants' *general* self-efficacy. Self-efficacy varies in its generality: While specific self-efficacy refers to a narrowly defined task or situation, generalized self-efficacy is considered more trait-like, meaning that a person believes in having a set of general life skills that can be applied to successfully master a variety of tasks and situations (Bandura, 1977; Smith, 1989). As the

self-efficacy manipulation was very specific to checkout-free shopping, it is plausible that it altered participants' *specific* self-efficacy rather than their general self-efficacy. It may be, however, that the effect of self-efficacy on patronage intentions operates at the level of stable, generalized self-efficacy. As it is unlikely that time and cost-effective manipulations, as typically used in experimental research, can effectively influence consumers' generalized self-efficacy, I suggest conducting real-world longitudinal intervention studies to investigate the causal effect of consumers' general self-efficacy on patronage intentions.

Despite this limitation, the studies conducted and the meta-analysis showed that the correlational association was highly robust across different studies and experimental conditions, even after controlling for age, gender, and experience with checkout-free shopping. This suggests that self-efficacy may be a core driver of consumers' intentions to patronize checkout-free stores.

A second limitation refers to the scenarios used in the studies to describe checkout-free shopping to participants. Depending on the study, the scenario was toned neutrally or emphasized certain characteristics of checkout-free stores. These scenarios may not have captured all aspects of checkout-free shopping that ultimately matter for self-efficacy and patronage intentions. Studies show that other factors, such as a diverse product range and competitive prices, influence consumers' store choice (Nam et al., 2025), which were not covered in the scenarios. Additionally, because a large share of participants did not pass the attention check in Study 2 (n = 167), the different scenarios may not have effectively communicated the respective characteristics of checkout-free shopping. Following that, the non-significant effects across the different scenarios cannot be taken as reliable evidence that

the communicated characteristics of checkout-free stores are irrelevant to patronage intentions and self-efficacy.

Conclusion and Practical Implications

In conclusion, self-efficacy should be considered an essential motivator for patronizing checkout-free stores; across all three studies, there was a strong positive correlation between self-efficacy and patronage intentions for checkout-free stores. Further, prior experience with checkout-free shopping was consistently positively associated with patronage intentions (Studies 1 – 3) and self-efficacy (Studies 1 & 2).

Following my findings, retailers can increase store patronage by targeting customers with high self-efficacy. They could do so through segmentation, by focusing on those who have previously used other innovative store technologies (such as apps or online delivery) and may therefore have higher self-efficacy for innovative technologies in general.

However, to avoid losing customers with low self-efficacy, retailers should go further by designing interventions that build self-efficacy. Here, it is vital that customers actually experience checkout-free shopping (following Bandura, 1982). Thus, motivating consumers to try out checkout-free shopping should be a top priority. This can be achieved by having a supportive staff member help the consumer set up the app or use checkout-free shopping for the first time, or by initiating a bring-a-friend campaign. In my study, I influenced self-efficacy by asking participants either complex or simple questions about checkout-free shopping. Retailers could adapt that method to their social media campaigns by using short, simple quizzes alongside financial incentives to encourage the use of checkout-free stores.

Lastly, managers could attract consumers with low self-efficacy by leveraging other motivators, such as consumer goals (Ajzen & Kruglanski, 2019). For example, checkout-free stores could be strategically positioned in locations where consumers aim to save time, such as train stations. Additionally, consumers seeking to purchase personal items, such as

condoms (Dahl et al., 2001), may prefer the low social presence of checkout-free stores, so stores' product range should feature such items. Furthermore, product quality and low prices were most important in store choice (Pan & Zinkhan, 2006). So, retailers should clearly communicate those assets to differentiate themselves from competitors and increase store patronage beyond self-efficacy.

Consumers should reflect on their self-efficacy to make an informed decision about whether to patronize checkout-free stores. As checkout-free shopping operates autonomously and requires no consumer intervention, consumers' self-efficacy concerns may be unfounded, and they may benefit more from the technology than initially expected.

In conclusion, my research shows that not all consumers feel capable of using checkout-free stores, and this discomfort is associated with lower intentions to patronize them. Targeting high-self-efficacy consumers or supporting low-self-efficacy consumers are suggested strategies for retailers.

CHECKOUT-FREE SHOPPING REDUCES PRICE RECALL

Sophia Wingen[1]

Erik Hoelzl[2]

[1]University of Cologne, Germany, sophia.wingen@wiso.uni-koeln.de, 0000-0001-8734-9026

[2] University of Cologne, Germany, hoelzl@wiso.uni-koeln.de, 0000-0002-5135-4728

Abstract

Checkout-free shopping, an innovative retail technology, is accessible to millions of consumers worldwide. Nevertheless, there is a lack of research on its psychological effects. We propose that checkout-free shopping reduces payment transparency, leading to a lower perceived price level and reduced price recall. We conducted an incentivized between-subject laboratory experiment (N = 150) with real purchase decisions in a simulated store. Participants were randomly assigned to shop in a simulated conventional or checkout-free store. Our results show that checkout-free shopping reduced recall of the prices of purchased products and of the total amount spent. This effect was mediated by reduced payment transparency in checkout-free stores. We could not show the effect of checkout-free shopping on price recall for unpurchased products or on price perception. Marketers should consider this adverse side effect to better understand consumers' acceptance of checkout-free stores. Policymakers should protect consumers from loss of budget overview.

Keywords: Checkout-free shopping, price perception, price recall, payment transparency

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Author contribution

All authors contributed to the study conception and design. Material preparation, data collection and analysis were performed by Sophia Wingen. The first draft of the manuscript was written by Sophia Wingen and Erik Hoelzl commented on previous versions of the manuscript. All authors read and approved the final manuscript.

Checkout-free Shopping Reduces Price Recall

Retailers worldwide are investing heavily in checkout-free technology (Amazon Just Walk Out technology; REWE in Germany; Daiei Inc. in Japan). In checkout-free shopping, customers register when entering the store. AI-supported sensor technology automatically detects and tracks when products are taken from or returned to the shelves. After shopping, customers can leave the store without checking out at a register. They receive the receipt via smartphone; the amount is automatically debited from their bank account (e.g., https://justwalkout.com/). Companies hope that checkout-free shopping can increase profits by reducing labor costs, improving inventory management, preventing fraud, and enhancing the customer experience (Herring, 2017; Wolniak et al., 2024).

However, recent developments show that consumer adoption does not match expectations. Amazon Web Services vice president Dilip Kumar (2024) reported removing checkout-free shopping from large-format grocery stores due to low consumer adoption (Kumar, 2024). Other retailers, though, are still pushing the distribution of checkout-free technology in their grocery stores forward (e.g., Rewe, 2024). These divergent strategies among retailers reflect broader uncertainty about the technology's potential. Psychological research can uncover aspects that influence consumers' attitudes, leading to more consumer-centered implementation strategies. Regardless, psychological concepts related to the patronage and perception of checkout-free stores remain under-researched (initial work: Cui et al., 2022; Hazée et al., 2025; Pillai et al., 2020; van Esch et al., 2021a, 2021b). Payment transparency (Soman, 2003) is a crucial psychological concept that remains overlooked in discussions of checkout-free shopping. Payment transparency refers to: "the relative salience of the payment, both in terms of physical form and the amount […]" (Soman, 2003, p. 175). We propose that the seamless shopping process in checkout-free stores minimizes payment transparency. Reduced payment transparency can lead to a biased perception of costs and

prices (Chatterjee & Rose, 2012; Falk et al., 2016) and adversely affect consumers' price recall (Gafeeva et al., 2018; Soman, 2001). Accurate price perception and recall are essential because consumers base their spending (Falk et al., 2016) and store choice (Burlison & Oe, 2018; Nam et al., 2025) on it. Consequently, they may avoid checkout-free stores to manage their budgets better (Hernandez et al., 2017).

This research aims to contribute to understanding the consequences of checkout-free shopping for consumers' perception and memory of prices. This knowledge can help marketing professionals to communicate checkout-free shopping effectively, retailers to develop it in a consumer-friendly way, and policymakers to protect consumers.

Literature Review

Payment Methods and Checkout Types

Checkout is a particularly painful phase of the shopping process (Reshadi & Fitzgerald, 2023). As liquidity constraints and the perceived pain of paying (e.g., Reshadi & Fitzgerald, 2023; Thomas et al., 2011) reduce spending, retailers and financial institutions have developed new payment methods to minimize liquidity concerns (e.g., card payments versus cash) and increase convenience (e.g., mobile payments versus card payments). Different payment methods influence consumer behavior beyond simply increasing the amount of available monetary resources. They were found to influence consumers' product selection (Bagchi & Block, 2011; L. Huang et al., 2024; Thomas et al., 2011) and willingness to spend (Boden et al., 2020; H. Liu & Chou, 2020; Y. Liu et al., 2021; Moore & Taylor, 2011; Prelec & Simester, 2001; Raghubir & Srivastava, 2008), their feelings after the purchase (Hahn et al., 2013; Kamleitner & Erki, 2013; Shah et al., 2016), and their memory of the purchase (Chatterjee & Rose, 2012; Gafeeva et al., 2018; Soman, 2001).

While the moment of payment was considered the most important event of the checkout process (Falk et al., 2016), it also includes other relevant aspects, such as waiting in line, unloading items, interacting with the cashier and others, and repacking products.

The advancement of AI and sensor technology presents a new, exciting opportunity to eliminate the checkout process entirely, thereby improving the customer experience. Initial studies suggest that such checkout-free stores can indeed increase convenience and spending (van Esch et al., 2021a). However, these positive effects depend on psychological factors (van Esch et al., 2021a, 2021b): only for customers high in self-efficacy did checkout-free shopping increase convenience (van Esch et al., 2021a). Further, checkout-free shopping positively influenced store atmosphere and purchase intentions, but only among consumers who did not perceive AI technology as threatening (van Esch et al., 2021b). We propose that payment transparency is an additional crucial psychological variable to consider as a consequence of the absence of a checkout process.

Payment Transparency

While payment transparency was initially studied as a consequence of using different payment methods, such as cash versus card (e.g., Soman, 2003), we propose that eliminating the checkout process in checkout-free stores should reduce payment transparency. That is because customers do not need to put their products on the belt, where they would see the total product amount; they do not see the individual products being scanned; they do not see the individual prices as well as the overall price on the checkout display; and they do not need to pack their products into a shopping bag at the checkout. All of these interactions with products and prices should increase payment transparency, and thus, payment transparency in checkout-free stores should be reduced to a minimum.

Payment transparency influences consumers' cognitions, emotions, and behavior across different stages of the customer journey. During product selection, participants

selected more unhealthy vice products (Thomas et al., 2011), a higher quantity of products (Soman, 2003), or a higher variety of products (L. Huang et al., 2024) when paying with less transparent payment methods. During actual payment, participants were willing to pay higher prices when using less transparent payment methods (Raghubir & Srivastava, 2008). After purchase, participants reported a reduced connection to (Shah et al., 2016) and lower ownership of (Kamleitner & Erki, 2013) the purchased products when the payment method was less transparent. Especially concerning is that payment transparency influenced the perception and recall of prices (Chatterjee & Rose, 2012; Falk et al., 2016; Gafeeva et al., 2018; Soman, 2001).

Price Perception and Price Recall

Consumers' price perception (i.e., as high or low) is highly relevant to both managers and consumers. Consumers base their store patronage (Pan & Zinkhan, 2006) and overall spending on their perception of a store's price level (Falk et al., 2016). Customers' price perception can be expressed through their overall store price image, which "reflects customers' beliefs about a store's overall price level relative to competition" (Falk et al., 2016, p. 2417). Consumers' price perception is influenced by the prices displayed in a store and by other situational factors, such as the payment method used (Chatterjee & Rose, 2012; Falk et al., 2016). For example, participants primed with credit cards focused more on the benefit aspects of products, whereas participants primed with cash focused more on the cost aspects of products (Chatterjee & Rose, 2012). Further, after shopping, participants reported a lower perceived price level if the payment method was less transparent (Falk et al., 2016). Falk et al. (2016) argued that this is because consumers use the transparency of the payment method as a cue for the store's price level.

In line with that, we propose that reduced payment transparency in checkout-free stores should lead to the perception that prices are cheaper than in other stores. That is

because customers anticipate the payment and therefore focus less on prices during product selection, using low payment transparency as a cue for lower prices in-store.

Not only the perception but also the recall of product prices is relevant for marketers and consumers. Consumers depend on price recall to manage their budgets and plan further expenses (Hernandez et al., 2017; Soman, 2001).

We propose that reduced payment transparency in checkout-free stores should increase price recall errors. First, during shopping, consumers' focus should shift toward being less price-sensitive (following Chatterjee & Rose, 2012), which should increase recall errors for product prices, regardless of whether the product was purchased. Second, eliminating checkout should further increase recall errors for the prices of purchased products and the total amount spent (following Gafeeva et al., 2018).

Method

To test our hypotheses, we conducted a preregistered, incentivized, between-subject laboratory experiment. We experimentally varied the checkout type (checkout-free vs. conventional) in a simulated store setup with 15 products. After selecting products and leaving the test store, participants were asked to report on product prices, total amount spent, and their perception of prices compared to other stores. Method and hypotheses were preregistered (https://aspredicted.org/t3rs-hm92.pdf) and approved by the responsible ethics committee.

Participants

Data from $N = 150$ participants was collected in the psychology lab of the Faculty of Economics and Social Sciences of the University of Cologne. Participants were, on average, 25.16 years old ($SD = 7.66$); 69 identified as female, 77 as male, two as diverse, and two preferred not to identify. Participants reported spending an average of €53.04 ($SD = €84.41$)

on groceries per shopping trip. Most participants reported going grocery shopping multiple times per week (n = 87) or once a week (n = 40).

Due to a coding error affecting three participants in the conventional checkout group, we were unable to identify the three products they purchased. We therefore could not calculate their overall spending amount and consequently their recall error for it. Thus, we used n = 147 participants for all analyses related to the recall error for the overall spending amount.

Material and Procedure

The experimental set-up consisted of two separate rooms on different floors in the same building: the test store and the reporting room. Participants signed up for a time slot and arrived at the reporting room. Based on their participation time, participants were randomly assigned to conventional or checkout-free shopping. At midday, the test store was converted from conventional to checkout-free shopping and vice versa. Participants signed the consent form and data privacy statement in the reporting room. Afterward, they read a brief description of the test store. Depending on their condition, they either read that the store uses a conventional checkout system or a checkout-free system. They also read that they will receive a payment card for their purchases (conventional checkout) or their expenses will be automatically charged to a virtual payment account (checkout-free). Participants then learned that they had a €10 budget and that their task was to choose one item from each of three categories—chocolate, strawberry jam, and spaghetti—according to a shopping list. They then received a payment card (conventional shopping) or a shopping bag (checkout-free shopping). Following that, participants proceeded to the test store. The test store contained 15 products from three categories (five products per category). The mean price per product was €1.52 (*Min* = €0.55; *Max* = €2.65). Products were presented on tables grouped by category. Each product was next to a sign indicating its price. The conventional store used a checkout

register setup in which a research staff member served as a cashier. After selecting their products, participants went to the checkout, where the cashier scanned the products. Each product and price was displayed on the checkout screen. The cashier stated the overall price, which was also shown on the screen. The participants paid by placing their payment card on the card reader. A sound indicated the successful payment, and participants could read, 'Payment received. Thank you for your purchase.' on the screen. The cashier packed all the products into a shopping bag and handed it over. Participants then proceeded to the reporting room.

In the checkout-free store, there was no checkout register. A research staff member was in the room to control for social presence, but did not interact with the participants. Participants selected their products and put them into their shopping bags before proceeding to the reporting room.

In the reporting room, a research staff member registered which product each participant had purchased, to inform our analysis. Participants then answered the post-shopping questionnaire on a computer. They were first asked to recall the prices of all products available in the supermarket in randomized order. Second, they were asked to recall the total amount they spent on their shopping trip. Third, participants answered the scales measuring price perception and payment transparency. As a last step, demographic variables and control variables were measured. As a manipulation check, we asked participants, in an open-ended question, how they would explain the checkout system to someone else. The experiment took about 20 minutes, and participants were compensated with a fixed show-up fee of €3. Additionally, 10% of participants could win their purchased products and the remaining budget to incentivize them to make realistic choices. To incentivize participants to recall prices as accurately as possible, the top 10% could win a €10 bonus. This resulted in an expected payment of € 5 for participating in the experiment.

Recall error for individual product prices. We measured recall error by asking participants the price of each product (purchased and unpurchased) in € using a drop-down menu. For each price, we calculated the recall error using the formula: % recall error = $\left|\frac{recalled\ price - correct\ price}{correct\ price}\right|$ adopted from Zeithaml (1982).

Recall error for the total spending amount. We also asked participants to recall the overall price of their shopping basket. We calculated the overall recall error using the same formula as for the individual prices.

Price perception. We measured the price perception using a German adaptation of the store price image scale by Falk et al. (2016), α = .85.

Payment transparency. We measured payment transparency using three items for the transparency of the *form* and three items for the transparency of the *amount*. One item each was a German adaptation of the items used by Falk et al. (2016). The other two items each were developed and pretested internally, α = .88.

Control variables. To control for participants' characteristics, we assessed their age, gender, and general shopping behavior. We asked them how much they spend on groceries per shopping trip and how often they go grocery shopping per month. To analyze perceptions of the experimental setting, we asked how easily participants could understand the task of the experiment and immerse themselves in the situation (1= 'very difficult'; 7 = 'very easy'), and how much effort they put into selecting products (1= 'very little effort'; 7 = 'a lot of effort'). We also asked participants for their experience with the checkout type they were assigned to (1 = 'very little experience'; 7 = 'a lot of experience'). Lastly, in line with Meade and Craig (2012), we asked them whether we should use their data in our analysis (Yes/No).

Results

Participants correctly noticed and understood the checkout system to which they were assigned (For more details, see Appendix B.1).

We used the software R to test our hypotheses. Table 3 displays descriptive statistics and correlations for the dependent variables and the mediator variable. We specified 10,000 bootstraps in our analyses to estimate 95% confidence intervals for the indirect effects.

Table 3

Descriptive Statistics and Correlations of the Dependent Variables and the Mediator Variable

			Checkout-free		*Conventional*		1	2	3	4	5
	M	*SD*	*M*	*SD*	*M*	*SD*	*r*				
Price perception[a]	3.87	0.81	3.95	0.79	3.80	0.83	1.00[a]	--	--	--	--
Recall error total spending amount [b]	0.12	0.16	0.17	0.17	0.07	0.14	.05[b]	1.00[b]	--	--	--
Recall error *purchased* products[a]	0.20	0.29	0.23	0.33	0.18	0.24	-.03[a]	.52***[b]	1.00[a]	--	--
Recall error *unpurchased* products[a]	0.32	0.33	0.31	0.35	0.32	0.31	-.06[a]	.12[b]	.18**[a]	1.00[a]	--
Payment transparency[a]	3.84	1.55	2.89	1.19	4.78	1.26	.04[a]	-.33***[b]	-.22*[a]	-.01[a]	1.00[a]

[a] $n = 150$, [b] $n = 147$. Due to coding errors, we excluded three participants from the conventional checkout group from this analysis.

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

Price perception

Price perception did not differ significantly between the two checkout groups ($M_{conventional}$ = 3.80, $SD_{conventional}$ = 0.83, $M_{checkout\text{-}free}$ = 3.95, $SD_{checkout\text{-}free}$ = 0.79, $t(147.6) = -1.14$, $p = .257$, $d = -0.19$, 95% *CI* [-0.51, 0.14]). To test the hypothesized mediating effect of payment transparency, we fitted a mediation model (lavaan; Rosseel, 2012). We found a significant effect of the checkout type on payment transparency ($b = -1.89$, $SE = 0.20$, 95% *CI* [-2.28, -1.50], $\beta = -.61$, $p < .001$, a-path). However, we could not confirm the effect of payment transparency on the price perception ($b = 0.01$, $SE = 0.05$, 95% *CI* [-0.09, 0.11], $\beta = .02$, $p = .830$, b-path) or the indirect effect of checkout type via payment transparency ($b = -0.02$, $SE = 0.10$, 95% *CI* [-0.22, 0.18], $\beta = -.01$, $p = .833$, a*b-path).

Recall Error for the Total Spending Amount

The recall error for the total spending amount was about 10-percentage points larger in the checkout-free ($M = 0.17$, $SD = 0.17$) than in the conventional shopping condition ($M = 0.07$, $SD = 0.14$, $t(142.22) = -3.71$, $p < .001$, $d = .61$, 95% *CI* [0.27, 0.94], $n = 147$; See Figure 4). Additionally, results show that in the conventional checkout group, 24 participants exactly recalled their total spending amount; however, in the checkout-free group, only 4 participants did so. A mediation model (lavaan; Rosseel, 2012, see Table 4 for details) further confirmed a significant effect of checkout type on payment transparency ($b = -1.83$, $p < .001$; a-path) and a significant negative relationship between payment transparency and the recall error ($b = -0.03$, $p = .013$, b-path). Additionally, the indirect effect of checkout-type via payment transparency was significant ($b = 0.05$, $p = .015$), while the direct effect of checkout-type was no longer significant ($b = 0.05$, $p = .14$), suggesting a full mediation. Taken together, payment transparency was reduced in the checkout-free store, and as a result, the recall error for the total spending amount increased.

Table 4

Mediation Model for the Effect of Checkout-Free Shopping on the Recall Error of the Total Spending Amount, Mediated by Payment Transparency

Predictor	b	*SE*	*z*	95% *CI*		β	*p*
				LL	*UL*		
Payment transparency							
Checkout type [a]	-1.83	0.20	-9.20	-2.21	-1.43	-.60	<.001
Recall error							
Checkout type [a]	0.05	0.03	1.47	-0.02	0.11	.15	.141
Transparency	-0.03	0.01	-2.49	-0.05	-0.01	-.24	.013
Indirect effect							
	0.05	0.02	2.42	0.01	0.09	.14	.015
Total effect							
	0.10	0.03	3.73	0.05	0.14	.29	<.001

Note. N = 147, CI = confidence interval; *LL* = lower limit; *UL* = upper limit.

[a] 0 = conventional checkout, 1 = checkout-free.

Recall Error for Purchased and Unpurchased Products

We fitted a linear fixed-effects model (lmerTest, Kuznetsova et al., 2017) to test the effects of checkout type, whether a product was purchased, and their interaction on the recall error for individual product prices. We specified random intercepts for different participants and different products. Because the data violate the assumption of normality of residuals, we

used bootstrapping (bootMer; Bates et al., 2015, 10,000 bootstraps) to obtain estimates and confidence intervals. The results are presented in Table 5.

Table 5

Linear Fixed Effect Model on the Effect of Checkout-Type, Purchase Status, and their Interaction on the Recall Error of Individual Products

	b	*SE*	*df*	*t*	95% CI		*p*
					LL	*UL*	
Constant	0.15	0.05	21.47	3.28	0.06	0.24	.004
Predictors							
Checkout type [a]	0.07	0.03	1057.62	2.37	0.01	0.12	.018
Purchase status [b]	0.18	0.02	2061.53	8.44	0.14	0.22	<.001
Checkout-type x Purchase status	-0.08	0.03	2058.84	-2.76	-0.14	-0.02	.006

Note. N = 150 participants, 15 price reports per participant. CI = confidence interval; *LL* = lower limit; *UL* = upper limit.

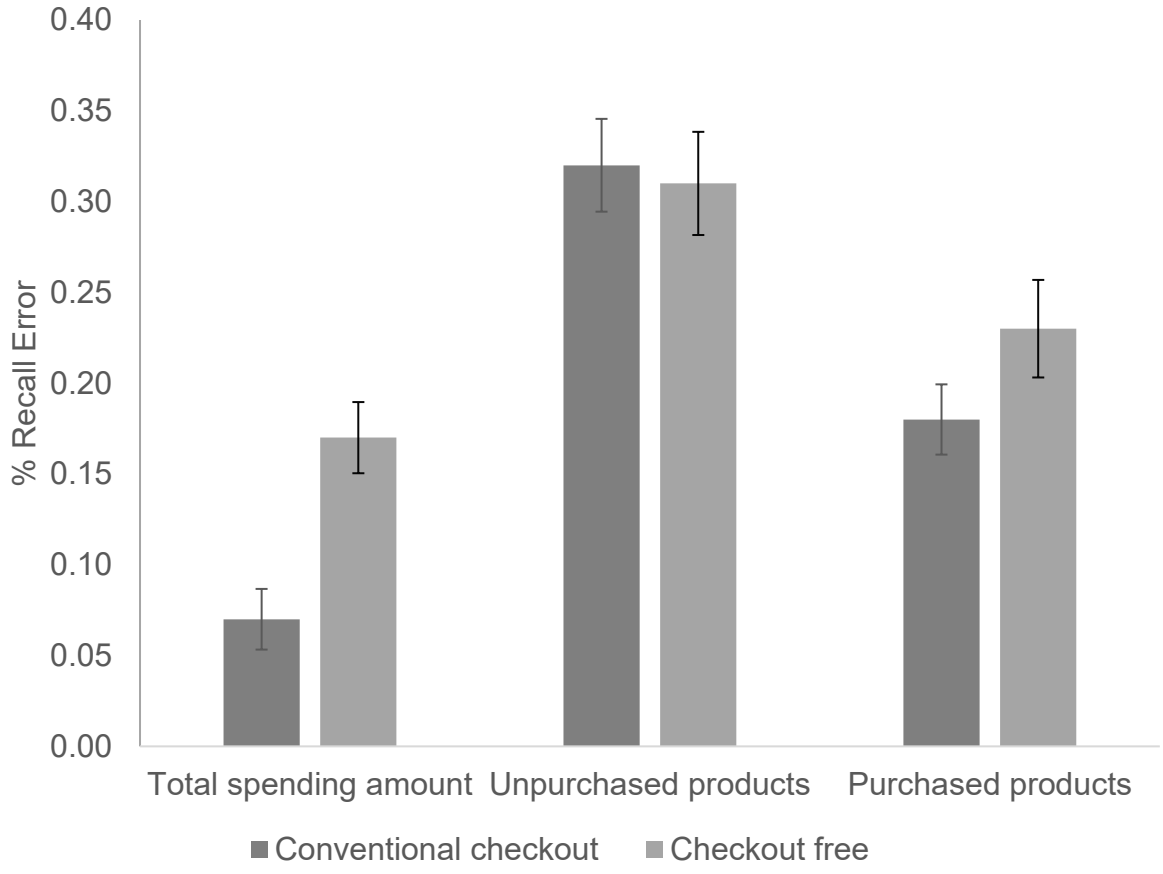
[a] 0 = conventional checkout, 1 = checkout-free. [b] 1 = unpurchased products, 0 = purchased products.

The results reveal a conditional effect of checkout type (b = 0.07, p = .018) and purchase status (b = 0.18, p <.001) and show a significant interaction between them (b = -0.08, p = .006). While the checkout type did not affect the recall error for prices of unpurchased products, checkout-free shopping increased the recall error for prices of

purchased products. Participants in the checkout-free condition had larger recall error (about 7 percentage points) for purchased products than participants in the conventional checkout condition (See Figure 4).

Figure 4

Recall Error for Prices of the Total Spending Amount, Unpurchased and Purchased Products by Checkout Condition



Note. $n_{\text{total spending amount}} = 147$, $n_{\text{individual prices}} = 150$. Error bars indicate standard errors.

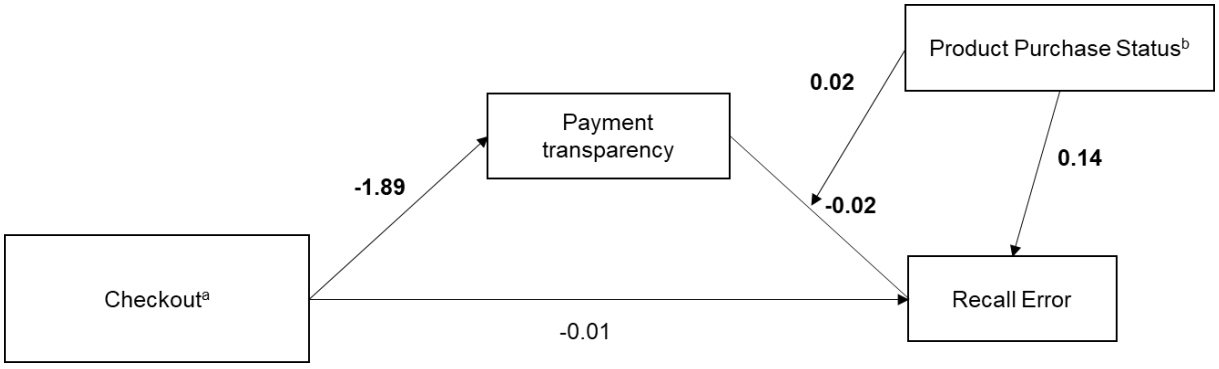
Additionally, to test the hypothesized moderated mediation, we first specified the mediator model by fitting a linear model of the effect of checkout type on payment transparency ($b = -1.89$, $SE = 0.20$, $t(148) = -9.46$, $p < .001$; a-path). We then specified the

outcome model by fitting a linear fixed-effects model (lmerTest, Kuznetsova et al., 2017) of the effects of checkout and payment transparency, whether a product was purchased, and the interaction between payment transparency and purchase status on the recall error for prices of individual products. We specified random intercepts for different participants and different products (See Appendix B, Table B.1). Within this model, the estimate for payment transparency referred to the b-path of the mediator model for purchased products. The sum of the payment transparency estimate and the interaction between transparency and purchase status referred to the b-path of the mediator model for unpurchased products. To determine the indirect effects, we used bootstrapping (10,000 bootstraps).

We found a significant moderated mediation: $b_{(a*int)}$ = -0.04, 95% *CI* [-0.075, -0.002]. More precisely, the conditional indirect path for purchased products was significant: $\text{Estimate}_{(a*b)}$ = 0.04, 95% *CI* [0.007, 0.080], while the conditional indirect path for unpurchased products was not significant: $\text{Estimate}_{(a*b+int)}$ = 0.01, 95% *CI* [-0.014, 0.026]. There was no remaining direct effect of checkout-type on the recall error of individual products: Estimate = 0.05, 95% *CI* [-0.05; 0.03]. That means that payment transparency fully mediates the positive effect of checkout-free shopping on recall error for purchased products (see Figure 5).

Figure 5

Results of the Multilevel Moderated Mediation Model



Note. $N = 150$, coefficients represent b, [a] 0 = conventional checkout, 1 = checkout-free; [b] 0 = purchased products, 1 = unpurchased products;

bold coefficients differ significantly from zero ($p < .05$)

Discussion

We are the first to test the effect of checkout-free shopping on payment transparency, downstream price perception, and price recall in a simulated store with real purchasing decisions. Our key finding is that checkout-free shopping reduces payment transparency, thereby increasing recall errors for the prices of purchased products and the total amount spent. This aligns with previous findings that show that less transparent payment methods can diminish price recall accuracy (Gafeeva et al., 2018; Soman, 2001). We extend these findings to checkout-free shopping and thereby demonstrate that not only the moment of payment but the entire checkout process matters for payment transparency and price recall.

Our findings further emphasize that the psychological consequences of checkout-free shopping must be taken into account. While it was initially shown that not all consumers see themselves as competent using such stores (Sohn, 2024; van Esch et al., 2021) or even perceive the required AI technology as threatening (van Esch et al., 2021b), our research adds to that by demonstrating that checkout-free shopping can adversely affect consumers' memory of prices. More precisely, we show that participants who shopped in the checkout-free store had a 7%-point higher recall error for the prices of purchased products and a 10%-point higher recall error for the total spending amount. Moreover, we demonstrated that while 24 participants in the conventional group correctly recalled their total spending amount, only 4 did so in the checkout-free group.

We could not confirm the effect of checkout-free shopping on the price recall for unpurchased products or the price perception. Thus, we cannot confirm that the reduced payment transparency in checkout-free shopping makes consumers less sensitive to prices during the product selection phase (conflicting with Chatterjee & Rose, 2012). Possibly,

participants need experience with checkout-free shopping to accurately anticipate the checkout and payment transparency, which may then affect their attention to prices during product selection.

This research has some limitations. First, our sample was demographically rather homogeneous, containing young university students. Second, we used a controlled laboratory environment, which may limit the generalizability of our findings to shopping in real stores. Lastly, participants in our store did not receive a paper receipt (conventional shopping) or a digital receipt (checkout-free shopping). This receipt could be used to restore lost price memories, but our study's findings do not allow us to conclude that. However, it has been proven that the temporal proximity between the purchase decision and the perceived payment is essential to consumers (Soman, 2001). The immediacy of wealth depletion increased recall accuracy for prices and decreased future purchase intentions for products of the same category (Soman, 2001). Thus, even though the digital receipt could restore missing memories, the timely separation between the purchase and the perceived depletion of wealth can lead to further biases. Future research should investigate the effect of delayed digital receipts.

Our findings make essential theoretical and normative contributions. First, our findings contribute to the payment transparency literature by considering the entire checkout process as relevant. Second, our findings inform marketers and policymakers. Accurate, unbiased recall of expenditures is necessary for consumers to monitor their budgets effectively (Hernandez et al., 2017; Soman, 2001). Moreover, it has been shown that consumers strategically use more transparent payment methods to control their expenditures (Hernandez et al., 2017). Recent developments in Amazon Fresh stores (Kumar, 2024) further emphasize this by showing that consumers hesitate to adopt the technology, especially for larger shopping trips, which are more challenging to keep track of. In this context, recent studies have shown that consumers value payment verification and that delayed information about the purchase and, in particular, the price can reduce their intention to buy (Benoit et al.,

2024). Further, in a qualitative study, participants stated a perceived loss of control over payments as a reason not to patronize checkout-free stores (Hazée et al., 2025). Following that, the checkout process may not only be a source of confusion and pain, but also a meaningful element that customers value for tracking their expenses. Checkout-free shopping is then not merely an improvement from the customers' perspective but also a risk. Retailers may want to counteract this perception by supporting customers with their budget overview. For example, they could use the checkout-free shopping app to provide live information that supports budgeting, including unusually high or low spending due to special offers.

In conclusion, we demonstrate that checkout-free shopping reduces payment transparency, thereby increasing price recall errors for purchased products and total spending. Given the growing adoption of checkout-free shopping, these findings raise concerns about consumer welfare and provide retailers with insights into the potential pitfalls of this new technology.

MINDSETS AT WORK: UNDERSTANDING THE POSITIVE IMPACT OF GROWTH MINDSETS ON WORKPLACE COACHING

Short title: *A COACHING MINDSET*

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Sophia Wingen*[1], https://orcid.org/0000-0001-8734-9026, Thomas Graczyk[1], https://orcid.org/0000-0002-8339-3350, Tobias Wingen[2,3], https://orcid.org/0000-0002-1559-859X, Simone Dohle[3], https://orcid.org/0000-0003-1535-5464

[1] *Institute of Sociology and Social Psychology, Faculty of Management, Economics and Social Sciences, University of Cologne, Germany*

[2] *Department of Behavioral Economics and Intercultural Psychology, Faculty of Psychology, FernUniversität in Hagen, Hagen, Germany*

[3] *Institute of General Practice and Family Medicine, University Hospital Bonn, University of Bonn, Germany*

*Correspondence concerning this article should be addressed to Sophia Wingen, Institute of Sociology and Social Psychology (ISS), University of Cologne, Albertus-Magnus-Platz, 50931 Köln, Germany. E-Mail: sophia.wingen@wiso.uni-koeln.de

Abstract

Understanding mindsets is crucial to understanding coaching behavior. Recent research has demonstrated that growth mindsets have both positive (i.e., increased outcome expectancy) and negative (i.e., increased blame attributions) effects on prosocial behavior. However, little is known about the role of these two opposing effects in work settings, especially when considering the history of competence-related and moral failure of people receiving coaching. To address this gap, we conducted four experiments (N = 2,295), which consistently revealed that a growth mindset (mediated through outcome expectancy) was positively related to willingness to coach. However, there was no evidence of detrimental effects via blame attributions, regardless of the recipient's failure history. Therefore, promoting growth mindsets in organizations likely contributes to a supportive workplace culture.

Keywords: Mindsets, Failure, Blame, Outcome Expectancy, Willingness to Coach

Mindsets at Work: Understanding the Positive Impact of Growth Mindsets on Workplace Coaching

Imagine there is a person who is struggling at work—John. Over an extended period at his company, he has routinely made avoidable mistakes, missed deadlines, and thus failed to meet performance goals on several projects. What do you think about John? Do you think coaching John would improve his performance?

Workplace coaching—a human resources development tool aimed at the achievement of professional outcomes in a one-to-one setting (Smither, 2011) and workplace helping in general (Brief & Motowildo, 1986)—enjoys great attention and popularity in organizations as well as in research (Bozer & Jones, 2018; Grant & Patil, 2012; Grover & Furnham, 2016; Jones et al., 2016; N. P. Podsakoff et al., 2009; P. M. Podsakoff et al., 2000; P. M. Podsakoff & MacKenzie, 1997; Theeboom et al., 2014; Q. Wang et al., 2022; Whiting et al., 2008). Not surprisingly, given that coaching can be a simple and readily available tool to improve performance, organizations and researchers alike are interested in the factors that motivate coaching to promote it in the work context.

One factor that contributes to the willingness to help and coach others is the mindset (Heslin et al., 2006; Rogers et al., 2023), that is, the degree to which people believe that people can (growth mindset) or cannot (fixed mindset) change (Dweck, 1999; Dweck & Yeager, 2019). While, in managers and employees, initial research showed that a growth mindset leads to an increased willingness and effectiveness of helping and coaching (Heslin et al., 2006; Kouzes & Posner, 2019; Rogers et al., 2023), recent mindset research suggests that a growth mindset may also have detrimental effects on helping and coaching through increased blame attributions (Burnette et al., 2017; Dohle et al., 2022; Erb & Busseri, 2022; Graczyk et al., 2024), especially, if a person fails continuously to exhibit the desired behavior (Ryazanov & Christenfeld, 2018).

Thus, the present research project aims to 1) shed further light on the relationship of mindsets with individuals' willingness to coach a person regarding their competence- and morality-related failures in a workplace setting by elaborating on the underlying mechanisms behind this relationship drawing on a recently developed dual pathway model (DUPA model, Dohle et al., 2022; Graczyk et al., 2024) and 2) expand the scope of the research and this model by distinguishing between instances of single and continuous failures.

Workplace coaching

Prosocial organizational behavior is characterized by an intent to benefit the individual, group or organization as a whole to which it is directed (Brief & Motowildo, 1986; Motowildo et al., 1997). Next to the recipient, one can also distinguish whether this behavior is organizationally (dys-)functional and whether it falls within one's formal role in the organization (Brief & Motowildo, 1986). For instance, helping a co-worker by covering up a mistake of theirs—which, typically, would be considered as extra-role behavior—is a kind of prosocial behavior which, however, (in the long run) may hinder an organization from being effective, leading to organizational dysfunction.

Conversely, helping behavior that aligns with one's prescribed role or tasks in the organization, such as training new employees, is usually considered functional as it, in theory, operates within organizationally established boundaries (Brief & Motowildo, 1986). Coaching—which is characterized by a collaborative and goal-oriented helping relationship between a coachee and a coach (Bozer & Jones, 2018; Smither, 2011)—is a prime example of role-prescribed prosocial behavior in a workplace context. Once the learning objectives are formally agreed upon, the goal of one-on-one coaching is to achieve them by providing the coachee with the necessary tools and skills, thus, helping the coachee to develop personally and/or professionally over a certain period (Bono et al., 2009; Smither, 2011). As an on-the-job learning intervention, coaching has the advantage of providing increased opportunities to

apply learned behavior directly to the work environment to maximize training transfer (Burke & Hutchins, 2007; van der Loch et al., 2013).

In this regard, the type of coach—internal versus external—represents a common distinction in the literature (Jones et al., 2016, 2018). Coaching within an organizational context may be facilitated by professional external coaches acting as consultants (referred to as external coaching), or by peers (peer coaching) and managers (managerial coaching), all of whom can provide valuable guidance and help from both internal and external perspectives. For example, internal coaches may provide advantageous task support because they are familiar with the organization's processes. On the other hand, coachees may feel more comfortable sharing sensitive information that is important for coaching success with a more confidential external coach (M. B. Arthur et al., 2005; Haggard et al., 2011; Jones et al., 2016, 2018). Irrespectively, research demonstrated that coaching is associated with a series of desired outcomes, such as increased work performance or well-being (Bozer & Jones, 2018; Grover & Furnham, 2016; Jones et al., 2016, 2018; Theeboom et al., 2014). Managerial coaching, for example, was identified as an important factor in organizational success as well as employee performance and thus an indicator of good leadership (Dahling et al., 2016; X. Liu & Batt, 2010; Olivero et al., 1997; Zuñiga-Collazos et al., 2020).

Arguably, internal coaching is likely to play a special role in the future, as it usually requires less financial resources and is thus made available to more employees, other than professional external coaching, which is mostly reserved for executives (Frisch, 2001; Theeboom et al., 2014). The demand for internal coaching may therefore continue to rise, thus, promoting the willingness to coach among employees or managers is a crucial task. On the other hand, while role-prescribed helping, such as coaching, is typically viewed as organizationally functional and beneficial (Bozer & Jones, 2018; Brief & Motowildo, 1986), an unrestricted positive perspective on this may not be justified as it can be associated with

high (psychological and resource) costs on the helpers' side and downstream the organization, for example, if they help beyond their formal obligations and capacities (Bolino et al., 2015; Bolino & Turnley, 2005; Edú-Valsania et al., 2022; Koopman et al., 2016; Spitzmuller & Van Dyne, 2012). Given that a change toward a desired behavior often requires multiple attempts (Baumeister & Heatherton, 1996; Heatherton & Nichols, 1994; Prochaska et al., 1993), repeated or continuous (costly) coaching is necessary to reach the goal. Likewise, it is necessary to assess whether any further coaching will contribute to its success—for example, if the person already failed multiple times to exhibit the desired behavior. Therefore, gaining insights into what influences those considerations, thus encouraging or discouraging individuals from coaching others within an organization, is essential.

Thereby, a factor relating to the perception of someone's repeated failures to exhibit the desired behavior, the expectation of outcomes and thus downstream the willingness to coach is the potential coaches' mindset (Dohle et al., 2022; Dweck & Yeager, 2019; Graczyk et al., 2024; Ryazanov & Christenfeld, 2018).

Mindsets, history of failure and coaching

Mindset research originates in the investigation of how children perceive their cognitive abilities, how they organize their world, how motivated they are, and how successful they will be academically (Burnette et al., 2013; Dweck, 1999; Dweck & Yeager, 2019). For example, a growth mindset is associated with external attributions for failure (Robins & Pals, 2002) and mastery strategies such as increasing effort (Burnette et al., 2013), while a fixed mindset is associated with internal and stable attributions for failure (Robins & Pals, 2002) and helplessness strategies such as reducing attention to a task (Burnette et al., 2013). Therefore, mindsets predict differences in performance, especially after obstacles and

failures, and those with a growth mindset are more successful than those with a fixed mindset (Blackwell et al., 2007; Burnette et al., 2013; Dweck, 1999).

Soon after, the concept of mindsets was adapted to interpersonal encounters. Mindsets shape the way people make judgments about others, the stability of those judgments, and subsequent behavior (Chiu et al., 1997; Erdley & Dweck, 1993; Gervey et al., 1999; Heslin et al., 2006). People with a fixed mindset tend to make judgments about others that are based on single observed behaviors (Erdley & Dweck, 1993; Gervey et al., 1999) and then deviate less from these judgments (Chiu et al., 1997; Erdley & Dweck, 1993) than people with a growth mindset. Consequentially, those with a fixed mindset are less forgiving of interpersonal transgressions than those with a growth mindset (Iwai & Carvalho, 2020). Ultimately, people with a fixed mindset seek to punish others who behaved contrary to social norms (Erdley & Dweck, 1993), whereas people with a growth mindset prefer to educate and help them (Gervey et al., 1999).

Fixed mindsets have similar detrimental consequences in the organizational domain. Managers with fixed mindsets are more likely to stick to their first impressions of a subordinate and are less sensitive to changes in their subordinates' performance (Heslin et al., 2005), while managers with a growth mindset are more likely to revise their first impression when the subordinate's performance changed (Heslin et al., 2005). Consequentially, managers are more willing to coach their subordinates and exhibit higher coaching quantity and quality when they hold a growth mindset toward their employee's characteristics than when they hold a fixed mindset (Heslin et al., 2006; Kouzes & Posner, 2019). Relatedly, recent research found that a work growth mindset contributes to more helping, for example mentoring, among employees if it contributes to both the own and the help recipient's development (Rogers et al., 2023).

The consistent findings of desirable cognitions and behaviors among those with a growth mindset have resulted in strong recommendations to promote a growth mindset among managers to increase their unbiased perception of employee performance as well as the quantity and quality of their coaching (Heslin et al., 2006; Heslin & VandeWalle, 2008).

A dark side of growth mindsets?

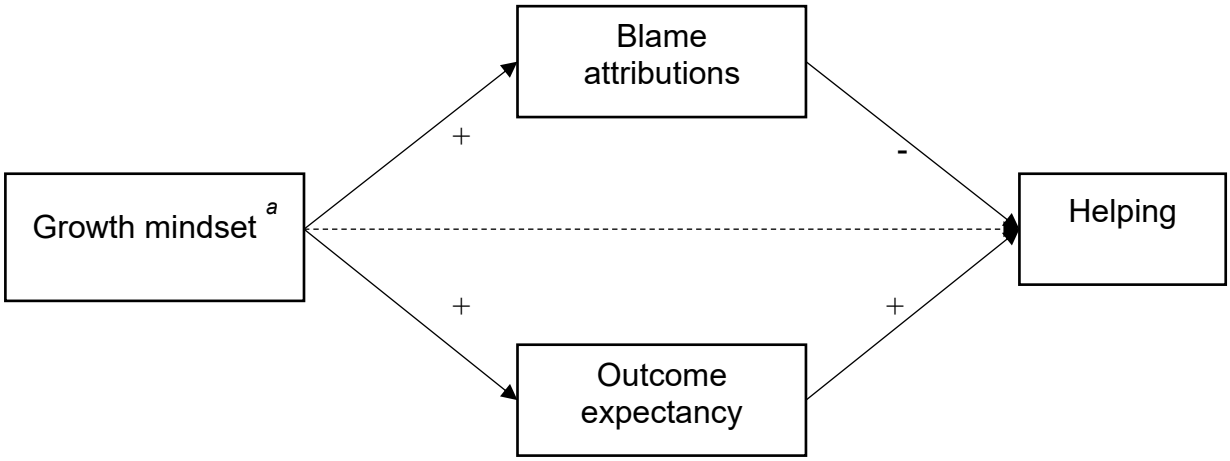
Recent research, rooted in classic theories from the field of moral psychology (Erb & Busseri, 2022; Ryazanov & Christenfeld, 2018), indicates that such promotion of growth mindsets might not be without risks. These theories postulate that the extent to which a person is blamed for a particular undesired behavior or trait depends on the belief about the amount of control a person has over that behavior or trait (Alicke, 2000; Ryazanov & Christenfeld, 2018). Thereby, a growth mindset is related to assumptions about greater control others have over undesirable behaviors, which, in turn, is related to greater attributions of blame toward a person who exhibits these behaviors (Ryazanov & Christenfeld, 2018). These theoretical assumptions were supported by research showing that a growth mindset led to larger blame attributions toward others who were physically or mentally ill (Dohle et al., 2022), overweight (Burnette et al., 2017; Hooper et al., 2018) or needed help from charities to escape poverty (Graczyk et al., 2024).

These negative effects conflict with the commonly held assumption about the superiority of a growth mindset in interpersonal encounters. To resolve this conflict, recent work developed and tested a dual-pathway model (DUPA model) on the interpersonal effects of mindsets (Dohle et al., 2022; Graczyk et al., 2024, for a related dual pathway model focusing on stigma, see Burnette et al., 2017). This model postulates that a growth mindset triggers two different interpersonal effects counteracting each other. On the one hand, it posits that a growth mindset increases blame attributions toward a person, which then reduces helping. On the other hand, a growth mindset is increasing expectations that helping the

person will be successful (i.e., outcome expectancy), as a result of more controllable attributions of the behavior (Dweck & Yeager, 2019; Hong et al., 1999), which then increases helping intentions (e.g., Dohle et al., 2022; Graczyk et al., 2024). Thereby, contextual factors would determine the relative magnitudes of these two effects, which subsequently determine the direction of the overall effect of mindsets on subsequent intentions or behavior (see Dohle et al., 2022; Graczyk et al., 2024, Figure 6).

Figure 6

Conceptual Dual-Pathway Model of the Influence of Mindsets on Helping (e.g., Coaching).



Note. [a] Higher values indicate a growth mindset. The model involves two mediating paths: outcome expectancy (when helping another person) and blaming a failing person. The dashed line indicates an expected direct null effect when the opposing effects are equal in magnitude.

One possible contextual factor shaping the effect of a growth mindset stems from work in the area of empathy mindsets, which emphasizes the dependence on how often the person fails to exhibit the desired behavior (Ryazanov & Christenfeld, 2018). Those with a growth mindset blamed a person more for not being empathetic than those with a fixed mindset when the person failed continuously (Ryazanov & Christenfeld, 2018). The authors

argued that single failures may not be judged as harshly by people with a growth mindset—compared to people with a fixed mindset—because they do not infer a bad character from single behaviors as readily. However, continuous failure may be judged more harshly by people with a growth mindset because it disproves their initial assumption about another person being generally willing and able to improve. In contrast, people with a fixed mindset may be more accepting of another's limitations to improve, thus attributing less responsibility to that person for their shortcomings (Ryazanov & Christenfeld, 2018). In this case, and especially because employees who failed repeatedly—which likely occurs in the work context—might be in an even higher need for coaching than others, the superiority of a growth mindset to promote workplace coaching can be questioned.

In summary, we believe that in the coaching context, blame and outcome expectancy may work in opposite directions, with a growth mindset being related to increased blame (H1a), which negatively mediates the effect of a growth mindset on willingness to coach (H1b) and a growth mindset being related to increased outcome expectancy of coaching (H2a), which positively mediates the effect of a growth mindset on willingness to coach (H2b). Furthermore, we assume that the positive indirect pathway of a growth mindset via outcome expectancy is stronger if a coachee has previously failed once (H3a), resulting in an overall positive effect of a growth mindset (H3b) and that the negative indirect pathway of a growth mindset via blame is stronger if a coachee has previously failed continuously (H4a), resulting in an overall negative relationship of a growth mindset with the willingness to coach (H4b).

In the workplace context, the applicability of the DUPA model (Dohle et al., 2022; Graczyk et al., 2024) is unclear as existing research did not test the mediating effects of the two proposed opposing paths. Likewise, as far as we know, there has not been any research

exploring whether having a growth mindset relates to increased blame and reduced willingness to coach when a person continuously fails to improve.

Overview of studies

We conducted four experimental studies to test the DUPA model (Dohle et al., 2022; Graczyk et al., 2024), which predicts that blame and outcome expectancy mediate—in opposite directions—the relationship between mindsets and willingness to help. We further extended this model by testing whether these relationships are moderated by the recipient's history of failure. In Study 1, we measured the mindsets of German students and their willingness to coach in a peer-coaching scenario and experimentally manipulated the target's history of competence-related failure (single versus continuous failure). In Study 2, we replicated the findings from Study 1 in a scenario more closely related to the workplace coaching with participants from the United States workforce. In Study 3, in addition to the history of failure, we experimentally manipulated participants' mindsets to investigate the causal effect of a growth mindset on the willingness to coach. Finally, in Study 4, we investigated the hypothesized relations in light of morality-related failures. All studies were preregistered. Materials, data, code, and supplemental materials are shared on OSF (https://osf.io/27vn4/).

Study 1

In Study 1, we used a vignette-based experimental study, to investigate whether the relation between participants' mindsets and their willingness to coach in a peer-coaching program is moderated by the recipient's history of failure. In addition, we tested whether blame attributions and outcome expectancies mediated the relationship between mindset and willingness to help in parallel and opposite ways.

Method

Participants.

Since our objective was to test the interaction between participants' mindset and history of failure, similar to the study conducted by Ryazanov & Christenfeld (2018), we utilized the effect size they identified for this interaction to perform an a priori power analysis (f^2 = .067, 1-β = .95 and α = .05) using G*Power (Faul et al., 2009). The power analysis indicated that a sample size of N = 262 is necessary to detect the interaction effect. While this was not our only objective, we decided to use this effect as the relevant criterion, assuming that this interaction effect would be smaller or equally large as potential main effects.

To compensate for potential data exclusions, we aimed to recruit N = 300 students who were at least in their third bachelor's semester to ensure they could picture the university-related situation described in our vignettes. We recruited a total of N = 310 participants between June and July 2021. We excluded 19 participants because they did not agree to use their data for analysis, leaving a final sample of N = 291 aged between 18 and 49 years (age: M = 25.39, SD = 5.11; 73.2% female). 51.1% of participants were Bachelor students, 38.5% were Master students, and the remaining participants were students enrolled in another system (e.g., state examination). The participants stated to have little experience in peer coaching (M = 2.22, SD = 1.31) and some experience in writing term and final theses (M = 3.67, SD = 1.15). About half of the participants (48.8%) had already written a final thesis (e.g., a Bachelor's thesis).

The study took about 15 minutes to complete. Participants were compensated with course credit if they were psychology students, additionally, all participants had a chance to win one of five 20€ Amazon gift cards.

Procedure.

First, we assessed the participants' mindsets using a German translation of the eight-item domain-general "*Kind of Person – Implicit Theory – 'Others' Form for Adults"* mindset scale (Dweck, 1999), for example, "The kind of person someone is, is something very basic

about them and it can't be changed very much." (*M* = 3.46, *SD* = 0.90, α = .90), on a scale ranging from 1 (*strongly disagree*) to 6 (*strongly agree*).

The participants were asked to imagine participating in a peer coaching program for students who had problems with their studies as a result of the COVID-19 pandemic and the online teaching associated with it. We then randomly assigned participants to either read a short text about a student ("Caroline") seeking help in that program who failed in one *or* all of her term papers because she did not properly assess her time management, style of references, and structure of the paper (see OSF for the full material).

Afterward, in randomized order, we measured blame attributions using three items, for example, "I think it's Caroline's fault that she's having trouble with her assignments." (*M* = 4.55, *SD* = 1.80, α = .92, adapted from Dohle et al., 2022) and outcome expectancy using three items, for example, "With the right support, Caroline could improve her performance in her assignments" (*M* = 7.65, *SD* = 1.25, α = .86, adapted from Dohle et al., 2022) both on a scale ranging from 1 (*strongly disagree*) to 9 (*strongly agree*), as well as the willingness to coach by adapting the three-item scale developed by Heslin et al., (2006), for example, "To what extent are you likely to help Caroline?" (*M* = 6.72, *SD* = 1.48, α = .77), on a scale ranging from 1 (*to a minimum extent*) to 9 (*to a large extent*). Furthermore, participants had the opportunity to give some advice to the student in an open answer text box.

For explorative purposes, we measured participants' personalities using the 21-item BFI-K Scale (Rammstedt & John, 2005). As for basic demographics and controls, participants indicated their age, and gender (diverse/female/male/other), how much experience they had with writing term papers, if they had written a thesis before (*Yes/No*) and how much experience they had with tutoring other students (1 = *very little experience*, 5 = *very much experience*). Additionally, they answered the careless response items developed by Meade & Craig (2012), that is, how much effort they put into the study (*M* = 4.24, *SD* = 0.66;

1 = *almost no*, 5 = *a lot of*), how much attention they paid to this study ($M = 4.62$, $SD = 0.60$; 1 = *almost no*, 5 = *my full*), and whether we should use their data for our analyses (*Yes/No*). Finally, to check whether the history-of-failure manipulation was successful, participants indicated which of the following two statements applied to the text they read before: "Caroline had difficulties in one *[all]* of her assignments".

Results

To assess whether the manipulation was successful, we conducted a χ^2 independence test showing that participants correctly remembered the history of failure, depending on their condition, $\varphi = .869$.

To test the relationship between mindset and blame attributions, outcome expectancy, and willingness to coach, as well as the interaction between the mindset and the history of failure (single/continuous failure), we conducted three moderated linear regression models for each dependent variable (see Table 6). We use the conventions established by Cohen (1988) for interpreting the magnitude of effect sizes. Other than hypothesized, we could only show an irrelevant relationship between the mindset and the three dependent variables (blame, outcome expectancy, and willingness to coach). Further, the main effect of the history of failure and their interaction on our dependent variables were irrelevantly small. The results of the analyses remained structurally the same after controlling for gender (including only participants who reported their gender as male or female; see OSF for details).

Table 6

Moderated Linear Regression Analysis of Associations Between Mindset, History of Failure*, and their Interaction with the Criterion Variables: Blame Attributions, Outcome Expectancy, and Willingness to Help (Study 1).*

Variables	*b*	*SE*
Blame attributions		

Growth Mindset [a]	.00	.16
History of failure [b]	.06	.21
Growth Mindset x History of failure	-.34	.23
Adjusted-R² = .00		
Outcome expectancy		
Growth Mindset [a]	.12	.11
History of failure [b]	.08	.15
Growth Mindset x History of failure	.24	.16
Adjusted-R² = .03		
Willingness to coach		
Growth Mindset [a]	.17	.13
History of failure [b]	.12	.17
Growth Mindset x History of failure	.09	.19
Adjusted-R² = .02		

Note. $N = 291$, results of the multiple regression models without control variables are shown.

[a] The mindset was mean-centered. High values indicate a growth mindset

[b] The history of failure was dummy coded (continuous failure = 1, single failure = 0)

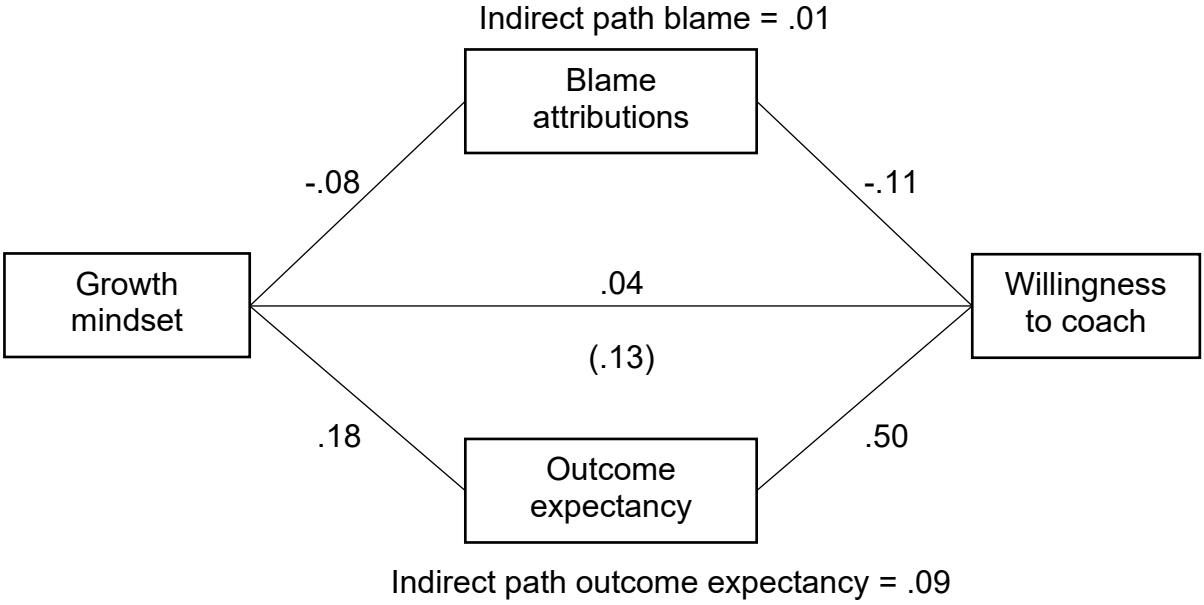
As a next step, we directly tested the DUPA model, which does not include history of failure as a relevant factor. We thus only included the mindset, blame attributions, and outcome expectancy in the model.

A parallel mediation model using the R package *lavaan* (Rosseel, 2012) with 10,000 bootstrap samples confirmed an indirect path of the mindset to the willingness to help via outcome expectancy (see Figure 7). Participants with a growth mindset reported a higher outcome expectancy for helping, which was related to a higher willingness to help, $b = .14$, $\beta = .09$, $SE = .05$. The indirect path via blame attributions was irrelevantly small, $b = .02$, β

=.01, *SE* = .02. Overall, without controlling for the other variables, we found a small relationship between a growth mindset and a higher willingness to help, $r(N = 291) = .13$.

Figure 7

Parallel Mediation Analyses Involving Blame and Outcome Expectancy as Dual, Simultaneous Mediators for the Link Between Mindset and Willingness to Help in Study 1.



Note. N = 291. Values represent standardized path coefficients. The total effect is presented in parentheses.

The pattern of results remained the same after adding control variables (Big-5 personality traits, management experience, coaching experience, gender) into the model. Only the negative relationship between blame attributions and the willingness to help increased to a relevant size, *b* = -.10, *SE* = .05, (see OSF for details).

Study 2

In Study 1, we found a positive relationship between a growth mindset and the willingness to coach via an increased outcome expectancy of coaching, at least when

following the original DUPA model without considering history of failure. Other than expected, even in this model we found no support for the positive relationship of a growth mindset with blame attributions subsequently lowering the willingness to help, irrespective of the history of failure. In Study 2, we sought to replicate these findings, however, with three important changes. First, we used a managerial coaching scenario more closely related to workplace helping. Second, to ensure participants could put themselves in the situation described, we only collected data from participants who had at least one year of working experience. Third, we conducted this study in a US context to explore intercultural generalizability.

Method

Participants

For this study, our sample size was not based on formal power analyses, which is not available for parallel mediation models in commonly used software, but instead relied on prior research utilizing similar sample sizes for parallel mediation models (T. Wingen et al., 2022).We recruited $N = 502$ participants from Amazon's Mechanical Turk (AMT)—no participant had to be excluded—aged between 19 and 76 years (age: $M = 40.76$, $SD = 12.89$; 46.0% female). Participants indicated to have some coaching experience ($M = 3.68$, $SD = 2.00$), and on average 4.66 ($SD = 7.33$) years of management experience. Study participation took about 5 minutes and participants received $0.75 for participation.

Procedure

The procedure and variables measured have been the same as in Study 1, however, with adjustments to fit the managerial coaching situation. The survey started with the assessment of the participants' mindsets using the same scale as before ($M = 3.94$, $SD = 1.31$, $\alpha = .97$).

Subsequently, participants imagined that they were middle-level managers in a company and asked to read a short performance review. Participants read either of two different performance reviews, stating that the person failed due to avoidable errors leading them to miss deadlines continuously or only a single time (see OSF for the full material; for a related vignette, see Iwai & Carvalho (2020).

Based on this review and in random order, participants were again asked about blame attributions using three items, for example, "I think it is John's own fault that he is missing his performance goals." (M = 7.00, SD = 1.75, α = .96, adapted from Dohle et al (2022) and outcome expectancy using three items, for example, "I think coaching John would have an impact on him meeting his performance goals." (M = 5.55, SD = 2.54, α = .98, adapted from Dohle et al., 2022) both on a scale ranging from 1 (*fully disagree*) to 9 (*fully agree*), and willingness to coach by adapting the three-item scale developed by Heslin et al. (2006), for example, "To what extent would you be willing to coach John to help him to improve his performance?" (M = 5.85, SD = 2.59, α = .97) on a scale ranging from 1 (*to a minimum extent*) to 9 (*to a large extent*).

For explorative purposes, we measured participants' personalities using the 30-item BFI-2-S Scale (Soto & John, 2017). Furthermore, we included the same basic demographic and (adjusted) control questions as in Study 1. Now, participants were asked how much experience they had with coaching (1 = *very little experience*, 7 = *very much experience*) and how many years of experience they had working as a manager. To check whether the history-of-failure-manipulation was successful, participants were asked to indicate how frequently "John" failed on a scale ranging from 1 (*John failed rarely*) to 7 (*John failed continuously*).

Results

As intended, participants in the continuous failure condition reported higher agreement with the statement that John failed continuously (M = 6.71, SD = 0.82) than

participants in the single failure condition (M = 2.07, SD = 1.48), thus we could confirm a large effect of our experimental manipulation, d = 3.91.

Again, we first conducted three moderated linear regressions for each dependent variable, using mindset, history of failure, and their interaction as predictors (see Table 7). Analyses showed that the mindset and the history of failure this time showed large relationships with the outcome expectancy and the willingness to coach. Those with a growth mindset reported a higher outcome expectancy as well as a higher willingness to coach. Additionally, people had a lower outcome expectancy and a lower willingness to coach an employee who failed continuously. Furthermore, participants blamed a person who failed continuously more than a person who failed a single time. Mindset, however, showed no relevant relationship with blame attributions. There was no evidence for our hypothesized interaction effect between the mindset and the history of failure. The results of the analyses remained structurally the same after controlling for gender (including only participants who reported their gender as male or female; see OSF for details).

Table 7

Moderated Linear Regression Analysis of Associations Between the Mindset, the History Of Failure, and their Interaction with the Criterion Variables: Blame Attributions, Outcome Expectancy, and Willingness to Coach (Study 2).

Variables	*b*	*SE*
Blame attributions		
Growth Mindset [a]	-0.02	0.08
History of failure [b]	0.75	0.15
Growth Mindset x History of failure	0.04	0.12
Adjusted-R^2 = .04		

Outcome expectancy		
Growth Mindset [a]	0.47	0.10
History of failure [b]	-2.71	0.19
Growth Mindset x History of failure	-0.17	0.14
Adjusted-R² = .32		
Willingness to coach		
Growth Mindset [a]	0.48	0.11
History of failure [b]	-2.39	0.20
Growth Mindset x History of failure	-0.11	0.15
Adjusted R² = .26		

Note. $N = 502$, results of the multiple regression models without control variables are shown.

[a] The mindset was mean-centered. High values indicate a growth mindset

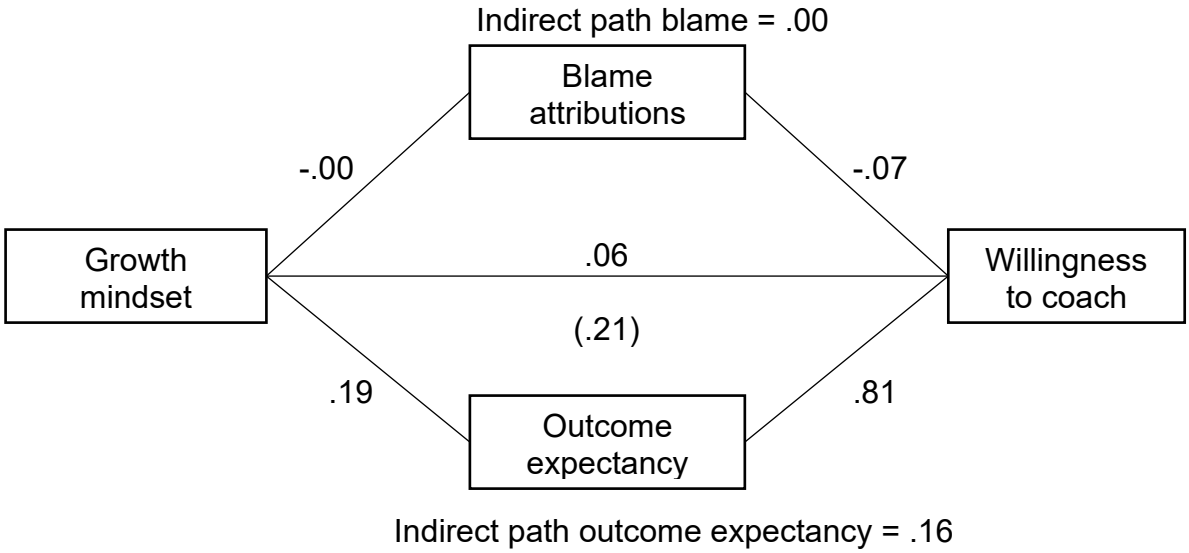
[b] The history of failure was dummy coded (continuous failure = 1, single failure = 0)

To test the DUPA model, we again conducted a parallel mediation model using the R package *lavaan* (Rosseel, 2012). We used the participant's mindset as the independent variable, blame attributions and outcome expectancy as parallel and opposing mediators, and willingness to coach as the dependent variable. As in Study 1, we did not include the history of failure in the model, as we found no interaction effect in the regression models and because history of failure is no original component of the DUPA model. The results confirmed an indirect path from mindset to willingness to coach via outcome expectancy, $b = .31$, $\beta = .16$, $SE = .08$, but no relevant indirect path via blame attributions, $b = .00$, $\beta = .00$, $SE = .01$. After including the mediators into the model, the direct relationship between mindset and willingness to coach persisted, $b = .11$, $\beta = .06$, $SE = .05$, indicating a partial mediation (see Figure 8). Overall, a growth mindset showed a medium sized relationship with the willingness to coach, $r(N = 502) = .21$.

Adding control variables (Big-5 personality traits, management experience, coaching experience, gender) into the model did not change the pattern of results (see OSF for details).

Figure 8

Parallel Mediation Analyses Involving Blame and Outcome Expectancy as Dual, Simultaneous Mediators for the Link Between Mindset and Willingness to Coach in Study 2.



Note. N = 502. Values represent standardized path coefficients. The total effect is presented in parentheses.

Study 3

In Study 2, consistent with Study 1, a growth mindset was associated with the willingness to coach, (partially) mediated via increased outcome expectancy but not via blame. However, in the scenario more closely related to an organizational setting, the history of failure proved impactful for blame attributions, outcome expectancy and willingness to coach alike. Still, the proposed moderating effect of the history of failure was absent. As the cross-sectional data from Studies 1 and 2 do not allow any causal inferences on the effect of the mindset, in Study 3, we used a 2x2 experimental design, to test the causal effect of mindsets on blame attributions, outcome expectancy, and willingness to coach. Building on

the results from Studies 1 and 2, we now preregistered that we expected no interaction effect of the mindset with the history of failure and only a significant path via outcome expectancy.

Methods

Participants

To ensure a similar power as in Study 2, we aimed to collect data from $N = 1000$ participants (250 per cell). Via our recruitment platform (AMT), we only included participants who had at least one year of working experience and who did not participate in Study 2. We recruited $N = 1{,}000$ participants aged between 18 and 93 years (age: $M = 43.33$, $SD = 13.40$; 51.4% female), and no participant had to be excluded. Participants had some coaching experience ($M = 4.00$, $SD = 1.98$), and on average 5.57 ($SD = 7.98$) years of management experience. Participation took about 5 minutes to complete the study and participants received $0.75 as compensation for their participation.

Procedure

The survey started with a manipulation of the participants' mindsets of personality using a well-established (Chiu et al., 1997) and pretested mindset manipulation. In this mindset manipulation, participants read about a recent talk at a scientific conference in which a scientist described research on whether personality can be changed. Depending on the condition, this scientist argued that personality can be changed (growth mindset condition) or not (fixed mindset condition). Subsequently, as a manipulation check, we measured participants' mindsets using the same questions as in the previous study ($M = 3.95$, $SD = 1.26$, $\alpha = .96$).

Afterward, we manipulated the history of failure corresponding to the manipulation used in Study 2. We also used the same variables to measure participants' blame attributions ($M = 6.96$, $SD = 1.67$, $\alpha = .94$), outcome expectancy ($M = 5.74$, $SD = 2.38$, $\alpha = .98$), and willingness to coach ($M = 5.85$, SD = 2.48, $\alpha = .97$) as in Study 2.

We also included the same basic demographic and control questions as in Study 2, but participants' personalities were not measured. At the end, participants were debriefed and informed that the discourse on the stability versus malleability of personality had not been concluded. They were provided further information on the discourse and an explanation for the one-sided message they read during the study.

Results

To assess whether the history-of-failure manipulation was successful, we conducted a *t*-test revealing that participants in the continuous failure condition reported higher agreement with the statement that John failed continuously ($M = 6.67$, $SD = 0.74$) than participants in the single failure condition ($M = 2.06$, $SD = 1.42$) as intended, $d = 4.08$. The mindset manipulation was also successful. Participants in the growth mindset condition reported higher growth mindsets ($M = 4.19$, $SD = 1.19$) than participants in the fixed mindset condition ($M = 3.71$, $SD = 1.28$), $d = 0.38$, however, this effect was rather small.

Again, we conducted three moderated linear regression models for each dependent variable to test our hypotheses. We used the mindset condition (fixed vs. growth), the history of failure condition (single vs. continuous failure) and their interaction as predictors, and our three dependent variables as the criterion (see Table 8). While the analyses showed a main effect of the history of failure on all of our dependent variables, we did not find a relevant effect of the mindset manipulation or its interaction with the history of failure. Thus, we could not support our hypothesis that a growth mindset leads to higher blame attributions, higher outcome expectancy, or a higher willingness to coach. The results of the analyses remained structurally the same after controlling for gender (including only participants who reported their gender as male or female; see OSF for details).

Table 8

Moderated Linear Regression Analysis of Associations Between the Mindset, the History of Failure, and their Interaction with the Criterion Variables: Blame Attributions, Outcome Expectancy, and Willingness to Coach (Study 3).

Variables	*b*	*SE*
Blame attributions		
Growth Mindset [a]	0.02	0.15
History of failure [b]	0.60	0.15
Growth Mindset x History of failure	0.07	0.21
Adjusted-R² = .03		
Outcome expectancy		
Growth Mindset [a]	0.06	0.18
History of failure [b]	-2.49	0.18
Growth Mindset x History of failure	0.18	0.26
Adjusted-R² = .25		
Willingness to coach		
Growth Mindset [a]	-0.03	0.20
History of failure [b]	-2.12	0.20
Growth Mindset x History of failure	0.19	0.29
Adjusted-R² = .16		

Note. $N = 291$, results of the multiple regression models without control variables are shown.

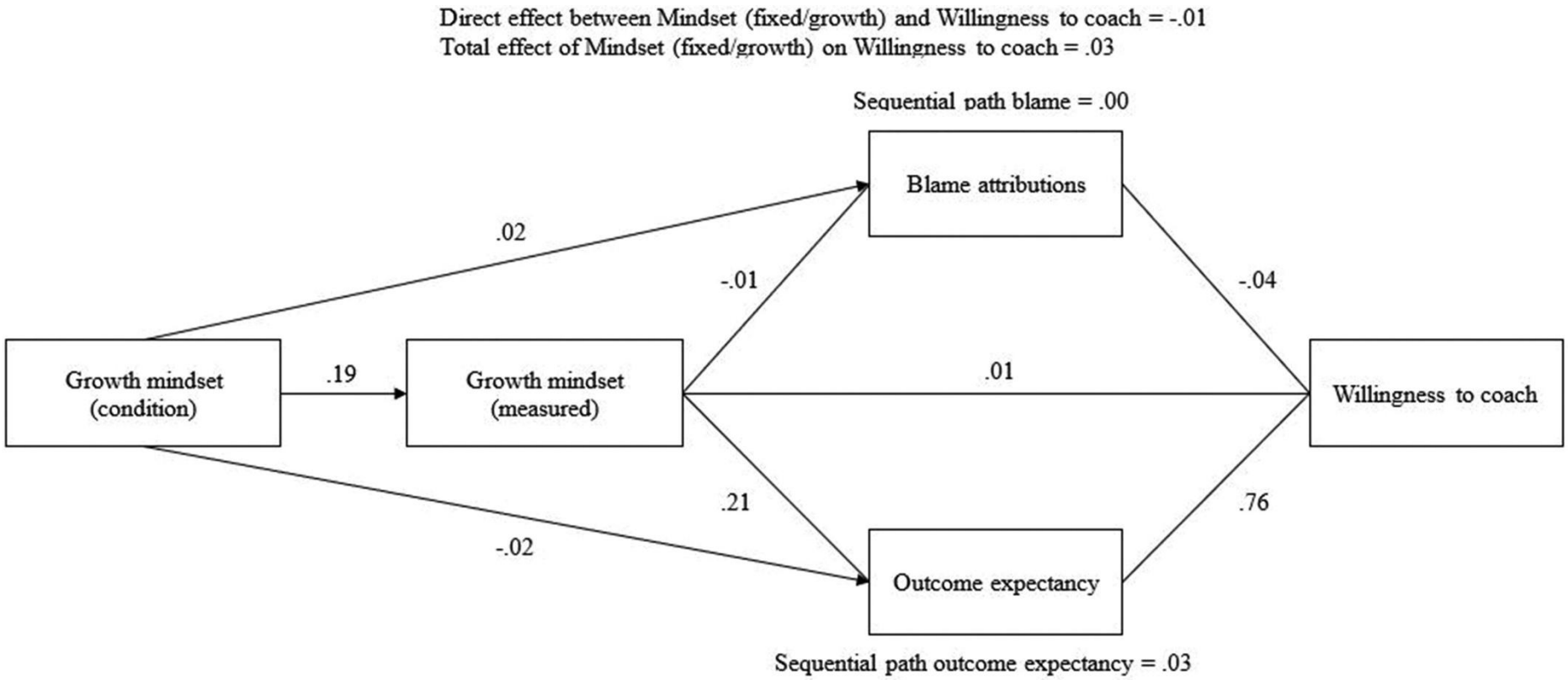
[a] The mindset was dummy coded (growth mindset = 1, fixed mindset = 0)

[b] The history of failure was dummy coded (continuous failure = 1, single failure = 0)

Whereas Study 3 thus found no effects of a manipulated mindset, Studies 1 and 2 found relevant associations with participants' measured mindset. We thus decided to explore both manipulated and measured mindset within the same model and conducted a sequential-parallel mediation model using the R package *lavaan* (Rosseel, 2012) to further explore the effect of the manipulation. In this model, we included the measured mindset of the

participants as a sequential mediator preceding blame attributions and outcome expectancy (see Figure 9). Results showed that there was an indirect sequential effect of the manipulation, mediated by the participant's measured mindset and the outcome expectancy on willingness to coach, $b = .15$, $\beta = .03$, $SE = .04$. The indirect sequential effect via blame attributions was found to be irrelevantly small, $b = .00$, $\beta = .00$, $SE = .00$.

Figure 9
Sequential Parallel Mediation Analyses Involving Measured Mindset (IPT) as a Sequential and Blame and Outcome Expectancy as Dual, Simultaneous Mediators for the Link Between Manipulated Mindset and Willingness to Coach in Study 3.



Note. $N = 1000$. Values represent standardized path coefficients.

Adding control variables (management experience, coaching experience, gender) into the model did not change the pattern of results, except for the negative relationship between blame attributions and the willingness to coach which substantially increased, b = -.08, SE = .03, (see OSF for details).

Study 4

In Studies 1 to 3, we consistently showed that a (measured) growth mindset was associated with increased willingness to coach via increased outcome expectancy, but not reduced willingness to coach via blame irrespective of history of failure. Interestingly, we observed only an indirect, but no overall effect of an experimentally manipulated mindset on willingness to coach. Until now, Studies 1 to 3 employed the relation of a growth mindset with the willingness to coach via blame attributions and outcome expectancy with competence-related failures. However, the evaluation of a person is not limited to competence-related behaviors but also refers to morality-related behaviors, such as honesty, helpfulness or friendliness (Fiske et al., 2007). Importantly, the diagnosticity of the observed behavior for the underlying traits is assessed asymmetrically for the morality- and competence-related behaviors, with incompetent behavior being attributed more situational—thus, overall less personally controllable and less diagnostic—and immoral behavior being attributed more dispositional—thus, overall more personally controllable and diagnostic (Fiske et al., 2007). This may alter the relationship between the mindset and downstream cognitions and reactions. For example, in their study, Ryazanov and Christenfeld (2018) described a person who continuously failed due to a “lack of empathy” (p. 38), suggesting a dispositional cause for the behavior. It may be that the suggested interactive effect of a growth mindset and history of failure on blame attributions (Ryazanov & Christenfeld, 2018) only emerges regarding behaviors that are rather diagnostic for dispositional traits, generally encompassing a more personal locus of control (Heider, 1958; Weiner, 2001). Thus, in Study

4, we aimed to investigate the hypothesized relations of a growth mindset, with blame attributions, outcome expectancy and willingness to coach in light of morality-related failures.

Methods

Participants

We aimed to recruit a similar number of participants as in Study 2. Via our recruitment platform (AMT), we only include participants who had at least one year of working experience. We recruited $N = 502$ participants aged between 19 and 79 years (age: $M = 41.15$, $SD = 11.29$; 43.8% female), and no participant had to be excluded. Participants had some coaching experience ($M = 3.98$, $SD = 1.97$), and in mean 5.12 ($SD = 7.11$) years of management experience. Participation took about 5 minutes and participants received $0.75 as compensation for their participation.

Procedure

We used the same procedure and the same variables as in Study 2. This time, however, after we measured participants' mindsets ($M = 3.86$, $SD = 1.28$, $\alpha = .95$), participants read one of two different reviews, stating that the person to be coached failed due to using working time for personal matters and lying about the project progress leading them to miss deadlines and project goals continuously *or* only a single time (see OSF for the full material).

After answering the same questions about blame attributions ($M = 7.67$, $SD = 1.45$, $\alpha = .94$), outcome expectancy ($M = 5.18$, $SD = 2.41$, $\alpha = .98$), and willingness to coach ($M = 5.19$, $SD = 2.62$, $\alpha = .97$) as in Studies 2 and 3, we measured participants' personalities using the 30-item BFI-2-S Scale (Soto & John, 2017). We included the same basic demographic and control questions as in Studies 2 and 3. Additionally, we asked participants whether they had previously participated in a study involving similar questions.

Results

To assess whether the manipulation was successful, we conducted a t-test showing that participants in the continuous failure condition agreed more with the statement about a continuous failure ($M = 6.72$, $SD = 0.81$) than participants in the single failure condition ($M = 3.04$, $SD = 1.95$), $d = 2.47$.

Again, we conducted three moderated linear regression models for each dependent variable to test our hypotheses. We used the mindset, the history of failure (single vs. continuous failure), and their interaction as predictors, and our three dependent variables as the criterion (see Table 9). The results show that the mindset and the history of failure were important predictors of outcome expectancy and willingness to coach. Those with a growth mindset reported a higher outcome expectancy as well as a higher willingness to coach. Additionally, people had a lower outcome expectancy and a lower willingness to coach a coachee that failed continuously than a coachee that failed a single time. Furthermore, participants blamed a person who failed continuously more than a person who failed a single time, but the mindset showed no relevant relationship with blame attributions. As in the previous studies before, there was also no evidence for our hypothesized interaction effect between the mindset of a coach and the history of failure. The results of the analyses remained structurally the same after controlling for gender (including only participants who reported their gender as male or female; see OSF for details).

Table 9

Moderated Linear Regression Analysis of Associations between the Mindset, the History of Failure, and their Interaction with the Criterion Variables: Blame Attributions, Outcome Expectancy, and Willingness to Coach (Study 4).

Variables	*b*	*SE*
Blame attributions		

Growth Mindset [a]	0.02	0.07
History of failure [b]	0.33	0.13
Growth Mindset x History of failure	-0.05	0.10
Adjusted-R² = .01		
Outcome expectancy		
Growth Mindset [a]	0.45	0.11
History of failure [b]	-2.14	0.19
Growth Mindset x History of failure	-0.15	0.15
Adjusted-R² = .25		
Willingness to coach		
Growth Mindset [a]	0.34	0.12
History of failure [b]	-2.08	0.21
Growth Mindset x History of failure	0.02	0.17
Adjusted-R² = .19		

Note. $N = 502$, results of the multiple regression models without control variables are shown.

[a] The mindset was mean-centered. High values indicate a growth mindset

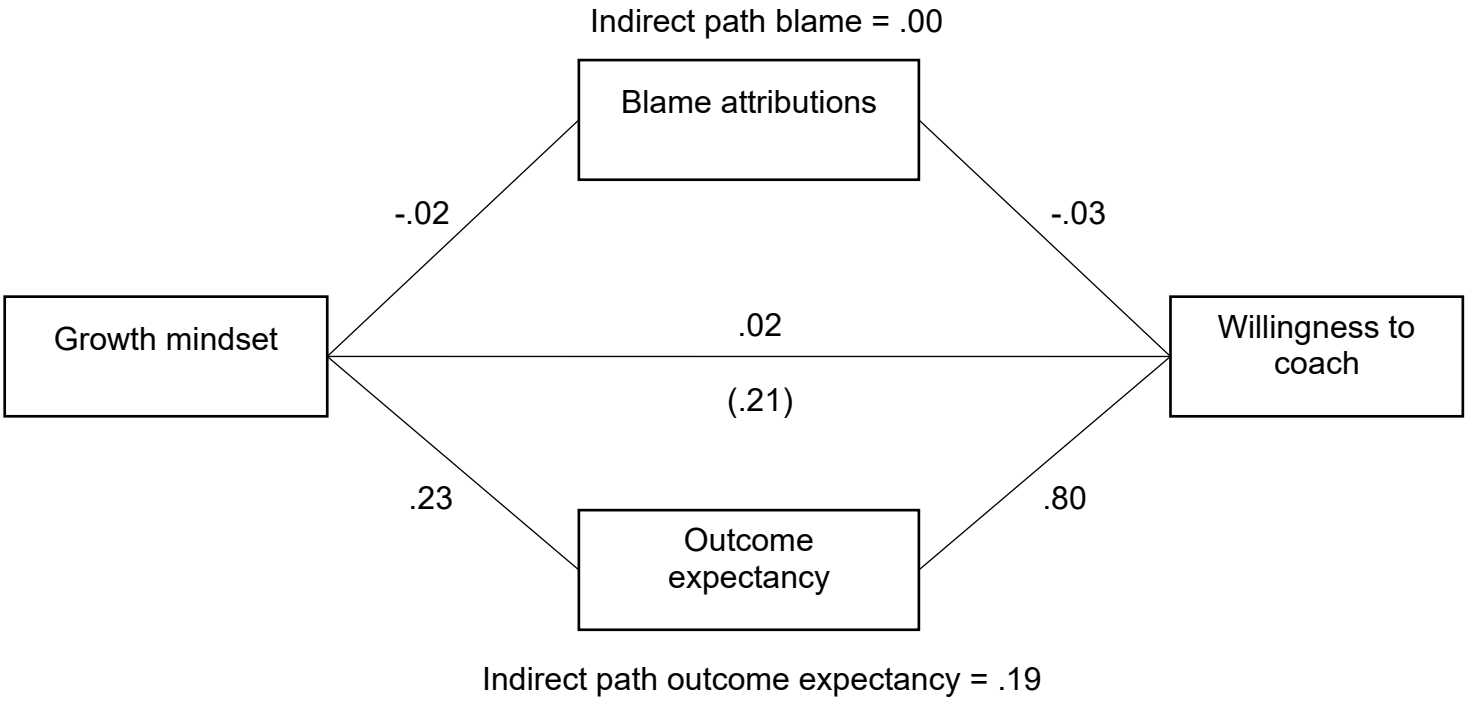
[b] The history of failure was dummy coded (continuous failure = 1, single failure = 0)

To test the DUPA model, we again conducted a parallel mediation model using the R package *lavaan* (Rosseel, 2012). We used the participant's mindset as the independent variable, blame attributions and outcome expectancy as parallel and opposing mediators, and willingness to coach as the dependent variable. Again, we did not include the history of failure of the coachee in the model, as we found no interaction effect in the regression models. The results showed an indirect path from the mindset to the willingness to coach via outcome expectancy, $b = .38$, $\beta = .19$, $SE = .08$, but no relevant path via blame attributions, $b = .00$, $\beta = .00$, $SE = .00$. The direct effect between the mindset and willingness to coach was negligibly small after including the mediators, indicating a full mediation of the mindset on

the willingness to coach via outcome expectancy (see Figure 10). Overall, a growth mindset showed a small to medium-sized positive relationship with the willingness to coach, $r(N = 502) = .21$.

Figure 10

Parallel Mediation Analyses Involving Blame and Outcome Expectancy as Dual, Simultaneous Mediators for the Link Between Mindset and Willingness to Coach in Study 4.



Note. $N = 502$. Values represent standardized path coefficients. The total effect is presented in parentheses.

The pattern of results remained the same after adding control variables (Big-5 personality traits, management experience, coaching experience, gender) into the model. In addition, including only the participants who reported not having participated in a study with similar tasks ($N = 480$) did not change the structure of the results of all analyses conducted (see OSF for details).

General Discussion

A better understanding of factors that motivate people to coach is crucial, since coaching increases employee and organizational performance (Bozer & Jones, 2018; Dahling et al., 2016; Jones et al., 2018; Olivero et al., 1997; Zuñiga-Collazos et al., 2020). In the present work, we aimed to deepen this understanding by exploring a social-cognitive model of the relationship between mindsets and coaching, focusing on theoretically highly relevant mediators (blame and outcome expectancy) and moderators (history of failure).

Our first aim was to investigate the effect of a growth mindset in relation to a history of failure. We showed that continuous failure indeed leads to higher blame attributions, a lower outcome expectancy, and a lower willingness to coach for competence-related failures (Studies 2 and 3) and morality-related failures (Study 4). However, across all studies, we found no evidence that the history of failure moderates the relation of a mindset on blame attributions, outcome expectancy, and ultimately, willingness to coach.

Secondly, we aimed to test two opposing paths of a growth mindset (DUPA model, Dohle et al., 2022; Graczyk et al., 2024) in the workplace context. We found consistent evidence for a positive relationship between participants' mindsets and their willingness to coach. This effect was statistically (partially in Study 2) explained by the outcome expectancy of coaching. Thus, we found support for one path of the assumed dual-pathway model (Dohle et al., 2022; Graczyk et al., 2024). However, we found no evidence for the hypothesized negative indirect effect of blame attributions. Relatedly, we found no

relationship between mindsets and blame attributions in a coaching context. In conclusion, rather than a dual pathway model, we found evidence for a single pathway model, showing that a growth mindset is related to a higher outcome expectancy which is related to a higher willingness to coach.

The positive indirect relationship of a growth mindset with the willingness to coach via outcome expectancy supports prior research in the domains of health (Dohle et al., 2022, Study 2) and poverty (Graczyk et al., 2024) and extends it to the workplace context. Moreover, we replicate previous findings showing that a growth mindset leads to more and better coaching (Heslin et al., 2006; Kouzes & Posner, 2019). Given existing concerns about the replicability of published research results (Open Science Collaboration, 2015), this finding provides additional assurance regarding the robustness of the effect. Further, we advance this field of research through an elaboration of the cognitive mechanism of this effect. In particular, we find that a growth mindset was positively related to willingness to coach through increased outcome expectancies. Furthermore, there were no detrimental effects of a growth mindset due to increased blame attributions. This was true regardless of how often the recipients failed in the past to exhibit the desired behavior. Thus, we contribute to the question of how to increase coaching in organizations by emphasizing the importance of mindsets for the motivation to coach.

The non-significant relationship between the mindset and blame attributions in our studies is contrary to earlier research in the domains of mindsets on health (Dohle et al., 2022), body weight (Burnette et al., 2017), personality (Ryazanov & Christenfeld, 2018) and poverty (Graczyk et al., 2024). Furthermore, in contrast to previous research, we found no evidence that a growth mindset is related to larger blame attributions toward consistently failing people (Ryazanov & Christenfeld, 2018). These findings highlight the need for further research on the relationship between growth mindsets and increased blame, to better

understand when and why this detrimental relationship occurs. This may also contribute to a better understanding of the non-significant interaction between mindsets and history of failure on blame attributions found in previous (but not our) research.

Additionally, future research could explore other possible downsides to growth mindsets in coaching contexts. As we did not find a reduced willingness to coach in participants with a strong growth mindset due to an increased attribution of blame to repeated failures—which might also serve as a protective mechanism, preventing potential coaches with a strong growth mindset from becoming overly invested in what seems like a hopeless situation—the question arises as to whether there might arise an escalation of commitment to coach (Heslin, 2009). Even though initial data about the differential perception of performance and performance change suggest that this might not be the case (Heslin, 2009; Heslin et al., 2005), our results draw further attention as to whether coaches with a growth mindset continue investing resources into coachees who simply cannot make the expected change. This ongoing investment could impact the coach (Schermuly & Graßmann, 2019) and the organization as a whole, especially regarding morality-related failures that constitute more intentional harm (Fiske et al., 2007).

Furthermore, prior research showed that participants with a growth mindset discriminated more in a personnel selection task (N. Huang et al., 2017). The authors argued that a growth mindset can lead to more unethical behavior because, under the framework of a growth mindset, unethical behavior is less diagnostic of a bad character, and thus unethical behavior is less threatening to a positive self-concept (N. Huang et al., 2017). Thus, the question arises whether coaches with a growth mindset are more likely to resort to unethical behavior to achieve their goals.

Lastly, future work may also increase the experimental reality, that is “if the situation is involving to the participants if they are forced to take it seriously if it has an impact on

them" (Wilson et al., 2010, p. 54). This could for example be achieved by building on the experimental scenario designed by Qin et al. (2018). In their experiment participants could influence the financial compensation for the experiment through their behavior. Such research could shed light on the question of whether the negative consequences of a growth mindset, such as increased blame attributions, become more pronounced if participants' behavior has real consequences for them.

Overall, our work suggests that growth mindsets are beneficial for managers' willingness to coach in applied workplace settings. Practitioners can thus utilize these findings to enhance managers' willingness to coach. To achieve this, they should first assess the mindsets of their managers. A cost-efficient way to implement this is the "Kind of Person – Implicit Theory – 'Others' Form for Adults" mindset scale (Dweck, 1999) used in this research, which can help to identify potentially problematic fixed mindsets. It is then important to foster a growth mindset among the relevant managers. While the results of our study strongly recommend promoting a growth mindset within organizations, we do not directly provide methods for fostering a growth mindset in the workplace. For this purpose, the work of Heslin et al. (2006) can be consulted, which broadly describes a growth mindset training program for managers. Despite their general malleability, mindsets often remain relatively stable over time (Heslin et al., 2006) and are associated with various positive cognitions and behaviors in the workplace (Dweck, 1999; Heslin et al., 2006; Kouzes & Posner, 2019; Rogers et al., 2023). Accordingly, such training is likely to have long-term benefits for the organization by fostering a growth mindset among managers which can lead to downstream positive cognitions and behaviors, including an increased willingness to coach.

Limitations

Certain limitations need to be considered when interpreting our findings and may be addressed by future research. Even though we manipulated the history of failure (Studies 1 to 4) and participants' mindsets (Study 3), the conclusions from our research are mainly based on cross-sectional data. Moreover, the mindset manipulation in Study 3 only had a rather weak effect on participants' actual measured mindset. Therefore, causal claims remain difficult (Shrout, 2011), as opposite causal influences are possible as well. For example, participants who are willing to coach might also report a high outcome expectancy to reduce cognitive dissonance (Festinger, 1964). Further, the relationships might be influenced by common third variables. For example, past observations of others changing successfully might influence participants' mindsets as well as their outcome expectancy and their willingness to coach. These alternative explanations cannot be ruled out based on our data. However, other research that either successfully showed the causal relationship between mindsets and the willingness to coach (Heslin et al., 2006) or tested the DUPA model thoroughly using experimental methods (Dohle et al., 2022; Graczyk et al., 2024) can provide some theoretical support for the assumed relationships.

Secondly, we did not investigate actual coaching behavior but participants imagined being a coach and then indicated their willingness to coach. While we designed our materials to ensure that participants could put themselves in the described situation and did control for management and coaching experience, we cannot be sure that the complex dyadic relationships in organizations are comparable to those in our study, especially in the context of internal coaching. For example, in reality, relationships between managers and their subordinates are formed over years of shared experience, characterized by co-dependencies and extensive behavioral options (e.g., letting continuously failing employees go). Thus,

caution should be exercised when attempting to generalize our findings to an organizational context (see Heslin et al., 2006) for a field study with a higher external validity).

Finally, the present work focused on internal (i.e., peer and managerial) coaching, as this type of coaching can, in theory, be provided in almost all organizations rather than external coaching. In this regard, however, we believe that our results may be informative as well. Arguably, internal and external coaches may be motivated by similar processes related to their experience with coachees' outcomes (Schermuly, 2014; Schermuly & Graßmann, 2019) as for both internal and external coaches, a core goal consists of fulfilling their agreement with a coachee to achieve their learning objectives (Bono et al., 2009; Smither, 2011). Accordingly, a coach's success or performance is measured by the success of their coachee. Thus, being able to reach or failing to reach the coachee's goals might affect the coach's sense of competence and self-efficacy as a coach (Bandura, 1977; Ryan & Deci, 2000), their status and reputation (Killburg, 2002; Schermuly, 2014; Schermuly & Graßmann, 2019) and downstream the coach's self-esteem and well-being (Abele & Wojciszke, 2007; Poggi & Errico, 2018; Wojciszke et al., 2011). Accordingly, both internal and external coaches likely experience the same cognitions based on their (expected) coachees' outcomes. However, it is certainly also important to explicitly consider the perspective of external coaching in future studies.

Conclusion

Taken together, our findings highlight the importance of growth mindsets in the workplace context, as they seem to be a crucial motivating factor for coaching others. Our results highlight that this is due to the increased outcome expectancy as suggested by the DUPA model (Dohle et al., 2022; Graczyk et al., 2024), while we found no evidence for any detrimental negative association via blame attributions. Exploring how coaches' mindsets can

be trained and gaining a better understanding of the potential downsides of a growth mindset are fruitful avenues for further research and practice.

Acknowledgments:

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Ethics approval statement:

All studies adhere to the ethical guidelines specified in the APA Code of Conduct as well as the guidelines of the German Psychological Society DGPS (Deutsche Gesellschaft für Psychologie). At the beginning of each study, all participants were informed that the data collection would be completely anonymous. Informed consent was obtained. Participants were able to withdraw from the studies at any time without giving a reason. The experimental manipulation of mindsets was approved by the Ethics Committee of the Faculty of Economic and Social Sciences (ERC-FMES) at the University of Cologne.

Conflict of interest:

The authors report there are no competing interests to declare.

Data availability statement:

Preregistration forms, materials, anonymized data, and analyses are available at: https://osf.io/27vn4/. All studies were conducted following APA guidelines and the rights of research participants have been protected.

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APPENDIX A: MANUSCRIPT 1

Appendix A.1. Study 1

A.1.1 Method

A.1.1.1 Vignette Used in Study 1

Please imagine the following scenario: A new grocery store has just opened in your area. This store uses a checkout-free system. That means that you can simply leave the store after shopping without having to stop at checkout to pay. Through sophisticated software, cameras, and sensors, the store's system can detect which products you pick up and put back. This way, the system automatically remembers all the products that you have selected, and when you leave the store, it completes the checkout process autonomously. The total price will automatically be charged to your account and you receive a digital receipt in the store's app or via e-mail. To use checkout-free shopping, you only need to install the store's app and to create a customer account. When entering the store, you scan a QR code provided by the app and start shopping. Again, you do not have to stop at checkout; you can simply leave the store after your shopping trip.

Appendix A.2: Study 2

A.2.1 Method

Table A.2.1.1

Vignettes to describe checkout-free shopping in the five experimental groups

Experimental group	Vignette
Baseline	Please imagine the following scenario: A new grocery store has just opened in your area. This store uses a checkout-free system. This means that you can simply leave the store after shopping without having to stop at checkout to pay. Through sophisticated software, cameras, and sensors, the store's system can detect which products you pick up and put back. This way, the system automatically remembers all the products that you have selected. When you leave the store, it completes the checkout process autonomously. The total price will automatically be charged to your account, and you will receive a digital receipt in the store's app or via email. To use checkout-free shopping, you only need to install the store's app and create a customer account. When entering the store, you scan a QR code provided by the app and start shopping.

	Again, you do not have to stop at a checkout; you can simply leave the store after your shopping trip.
High convenience	Please imagine the following scenario: A new grocery store has just opened in your area. This store uses a very convenient checkout-free system. This means that you can simply leave the store after shopping without having to wait at checkout, hurry to pack products, or handle the payment. Through sophisticated software, cameras, and sensors, the store's system can detect which products you pick up and put back, without you having to do anything. This way, the system automatically remembers all the products that you have selected. When you leave the store, it completes the checkout process autonomously, which makes it very convenient for you. The total price will automatically be charged to your account, and you will receive a digital receipt in the store's app or via email. To use checkout-free shopping, there is not much to do. You only need to install the store's app and create a customer account. When entering the store, you only scan a QR code provided by the app and start shopping.

	Again, you do not have to stop at a checkout; you can simply leave the store after your shopping trip. The system handles everything else. Checkout-free shopping is the most convenient and time-saving way to buy groceries and it will revolutionise retail.
High innovativeness	Please imagine the following scenario: A new grocery store has just opened in your area. This store uses an innovative AI technology that enables checkout-free shopping. This means that you can simply leave the store after shopping without having to stop at checkout to pay. Through sophisticated software, cameras, and sensors, the store's innovative system can autonomously detect which products you pick up and put back. This way, the advanced AI technology automatically remembers all the products that you have selected in real time. When you leave the store, it seamlessly completes the checkout process autonomously. The total price will automatically be charged to your account, and you will receive a digital receipt in the store's app or via email.

	To use checkout-free shopping, you only need to install the store's app and create a customer account. When entering the store, you scan a QR code provided by the app and start shopping. Again, you do not have to stop at a checkout; you can simply leave the store after your shopping trip, and the advanced network of AI-enabled cameras and smart sensors will handle everything else. Checkout-free shopping is the most innovative way to buy groceries and it will revolutionise retail.
Low payment transparency	Please imagine the following scenario: A new grocery store has just opened in your area. This store uses a checkout-free system. This means that you can simply leave the store after shopping without having to stop at checkout to learn the final price and pay. Through sophisticated software, cameras, and sensors, the store's system can detect which products you pick up and put back as well as their prices. This way, the system automatically remembers all the products that you have selected, and when you leave the store, it completes the checkout process autonomously. It will calculate the total price and automatically charge it to your account without your active confirmation, and you will receive a delayed digital receipt in the store's app or via email.

	To use checkout-free shopping, you only need to install the store's app and create a customer account where you connect your payment account. When entering the store, you scan a QR code provided by the app and start shopping. Again, you do not have to stop at a checkout; you can simply leave the store after your shopping trip, and your purchase amount will be automatically deducted from your account. Checkout-free shopping is the only retail technology that allows you to walk out with your groceries without feeling the pain of paying, and it will revolutionise retail.
Absence of human staff	A new grocery store has just opened in your area. This store uses a cashierless system, called checkout-free shopping. This means that you can simply leave the store after shopping without having to stop at checkout to pay. The store does not rely on employees completing the checkout process; instead, through sophisticated software, cameras. and sensors, the store's technology can detect which products you pick up and put back. This way, the system automatically remembers all the products that you have selected. When you leave the store, it completes the checkout process autonomously, without the involvement of any human statt. The total price will automatically be charged to your account, and you will receive a digital receipt in the store's app or via email.

To use checkout-free shopping, you only need to install the store's app and create a customer account. When entering the store. you scan a QR code provided by the app and start shopping.

Again. you do not have to stop at a checkout or interact with a cashier; you can simply leave the store after your shopping trip.

Checkout-free shopping is the only retail technology that makes it possible to operate grocery stores completely without human staff and it will revolutionise retail.

A.2.2 Results

Table A.2.2.1

Distribution of Responses to the Attention Check by Experimental Group

Response	Baseline	Convenience	Innovative AI technology	Low payment transparency	Absence of human staff
	n = 81	*n* = 80	*n* = 81	*n* = 80	*n* = 79
Neutral tone	50	16	20	14	20
Convenience	14	54	16	13	6
Innovative AI technology	9	9	43	4	15
Low payment transparency	3	0	1	49	0

Absence of human staff	5	1	1	0	38

Note. $N = 401$

Table A.2.2.2

Planned contrast of the ANOVAs for Patronage Intentions and Self-Efficacy

Patronage intentions				
Predictor	*b*	*SE*	*t*	*p*
Intercept	4.48	0.09	48.11	<.001
Convenience	0.12	0.19	0.64	0.525
Innovation	-0.25	0.19	-1.34	0.182
Payment transparency	-0.04	0.19	-0.19	0.849
Absence of human staff	0.21	0.19	1.14	0.257
Self-efficacy				
Predictor	*b*	*SE*	*t*	*p*
Intercept	5.40	0.07	78.09	<.001
Convenience	0.11	0.14	0.76	0.446
Innovation	-0.04	0.14	-0.28	0.777
Payment transparency	-0.13	0.14	-0.97	0.332
Absence of human staff	0.11	0.14	0.79	0.432

Note. $N = 401$

Table A.2.2.3

ANOVAs for the Prediction of Patronage Intentions from Self-Efficacy; Controlled for whether Participants Passed the Attention Check

Dependent variable		*df*	Sum sq.	Mean Sq	*F*	*p*
Patronage intentions	Experimental group	4	10.0	2.49	0.72	0.580
	Passed attention check [a]	1	4.8	4.83	1.39	0.239
	Residuals	395	1371.2	3.47		
Self-efficacy	Experimental group	4	3.5	0.89	0.46	0.764
	Passed attention check [a]	1	0.1	0.05	0.03	0.870
	Residuals	395	758.4	1.92		

Note. $N = 401$

[a] Dummy-coded; 1= passed, 0 = not passed

Table A.2.2.4

Indirect Effects of the Experimental Group on Patronage Intentions via Self-Efficacy.

				95% *CI*		
Predictor	*b*	*SE*	*p*	*LL*	*UL*	β
Convenience	0.09	0.13	.510	-0.17	0.35	0.02
Innovative AI technology	0.00	0.13	.991	-0.25	0.26	0.00
Low payment transparency	-0.06	0.13	.669	-0.31	0.20	-0.01

Absence of human staff	0.09	0.13	.504	-0.16	0.36	0.02

Note. $N = 401$; *LL = lower limit; UL = upper limit*, each indirect effect is defined as the difference from the baseline condition.

Appendix A.3: Study 3

A.3.1 Method

A.3.1.1. Detailed Description of the Conducted Pretest – Manipulation of Self-Efficacy

The manipulation was pretested with $N = 210$ participants. Participants were randomly assigned to one of three groups: (1) six simple questions, (2) a mix of three simple and three complex questions, (3) six complex questions. Following that, participants' self-efficacy and the difficulty of the questions were assessed. Because participants in the mixed group did not differ significantly from the other two groups, this group was omitted from the following analysis. Participants reported perceiving the simple questions as easier ($M = 1.76$, $SD = 0.99$) than the complex questions ($M = 3.98$, $SD = 1.44$), $t(118.26) = 10.56$, $p < .001$, $d = 1.81$, 95% CI [1.41, 2.20], $n = 139$). Additionally, participants from the complex-questions group had a lower self-efficacy ($M = 5.00$, $SD = 1.57$) than participants from the simple-questions group ($M = 5.49$, $SD = 1.30$, $t(137) = -2.02$, $p = .045$, $d = -.34$, 95% CI [-0.68, -0.00], $n = 139$).

Table A.3.1.1

Questions regarding checkout-free shopping that the participants had to answer, depending on their experimental group

Low self-efficacy (Complex questions)	High self-efficacy (Simple questions)
What happens if I'm charged for a product I didn't take?	Do I have to stop at a checkout and scan the items myself?
How do I buy age-restricted items like alcohol?	How is the total price charged to me?

Will I be accused of shoplifting if the system fails to detect an item?	What do I need to do to enter the store?
Does the autonomous checkout still work if my phone dies while shopping?	Do I need to install an app to use checkout-free shopping?
What if a child with me adds items to my cart?	How do I receive a receipt after shopping?
How do I purchase loose items, such as fruit that needs to be weighed?	Can the system detect if I put an item back?

APPENDIX B: MANUSCRIPT 2

B.1 Assessment of the Study Material

Participants reported that it was easy for them to understand their shopping task (M = 6.37, SD = 0.95), that they put effort into the study (M = 5.15, SD = 1.42), and that it was relatively easy for them to immerse into the study scenario (M = 5.55, SD = 1.30, all on 7-point scales). Two coders individually rated the answers to the manipulation check for a general understanding of the checkout system (Yes/No/Unclear). In the conventional checkout group, 73 participants correctly understood the checkout type, and for 2 participants, it was unclear from the provided response. In the checkout-free shopping group, 66 participants understood the checkout system correctly, and for 6, it was unclear from their responses. The raters disagreed in three cases. Additionally, participants in the conventional checkout group stated they had much experience with the checkout system (M = 5.56, SD = 1.56). In contrast, participants in the checkout-free group stated they had little experience with the checkout system (M = 1.71, SD = 1.39), $t(146.1) = 15.95$, $p < .001$)

Table B.1

Linear Fixed Effect Model on the Effect of Checkout-Type, Payment Transparency, Purchase Status, and their Interaction on the Recall Error of Individual Products

Effect	*b*	*SE*	*df*	*t*	95% CI		*p*
					LL	*UL*	
Constant	0.19	0.04	19.38	4.28	0.10	0.27	<.001
Predictors							
Checkout type [a]	-0.01	0.02	143.99	-0.49	-.051	0.03	.623
Transparency	-0.02	0.01	697.91	-2.21	- 0.04	-0.00	.028
Purchase status [b]	0.14	0.02	2063.50	9.14	0.11	0.17	<.001
Transparency x Purchase status	0.02	0.01	2058.91	2.01	0.00	0.04	.044

Note. N = 150 participants, 15 price reports per participant. CI = confidence interval; *LL* = lower limit; *UL* = upper limit.

[a] 0 = conventional checkout, 1 = checkout-free. [b] 1 = unpurchased products, 0 = purchased products

AI Tool Declaration

During the preparation of this work, I used ChatGPT 5.5 as an extended search engine to complement the literature search conducted with Google Scholar and PsycInfo and to troubleshoot analysis code. Finally, Grammarly was used to in order to edit the final text for grammar and clarity. After using these tools, I reviewed and edited the content as needed and take full responsibility for the content of this dissertation.