Pre-Moderation in Online Brand Communities*

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Abstract

Firms operating online brand communities (OBCs) face a key tension: aligning user contributions with brand values without slowing the flow of fresh, engaging content. This study offers one of the first empirical examinations of how pre-moderation—moderating content *before* publication—affects user behavior. Leveraging a natural experiment in a large OBC dedicated to recipes for a kitchen appliance, I find that the policy reduced published recipes by 4.4 percent. This decline is driven primarily by inexperienced users, who publish fewer recipes in oversaturated categories, and partly by experienced users, who are deterred by likely publication delays. The policy also lowered the novelty of published recipes, suggesting a narrowing of the acceptable solution space. However, user engagement with content, as measured by likes, remained largely unchanged. Together, these findings suggest that pre-moderation can inadvertently silence the periphery—deterring new or less-established contributors—and constrain creative freedom. For OBC operators, the results highlight a strategic trade-off: while pre-moderation can filter for quality and brand fit, it may also risk a drift toward homogeneity and disengagement of contributors.

Keywords: Online brand communities, content moderation, pre-moderation, natural experiment, difference-in-differences.

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1 INTRODUCTION

It was kind of the wild west, in terms of what people were submitting, the comments people were making. There wasn't pre-moderation at the time.

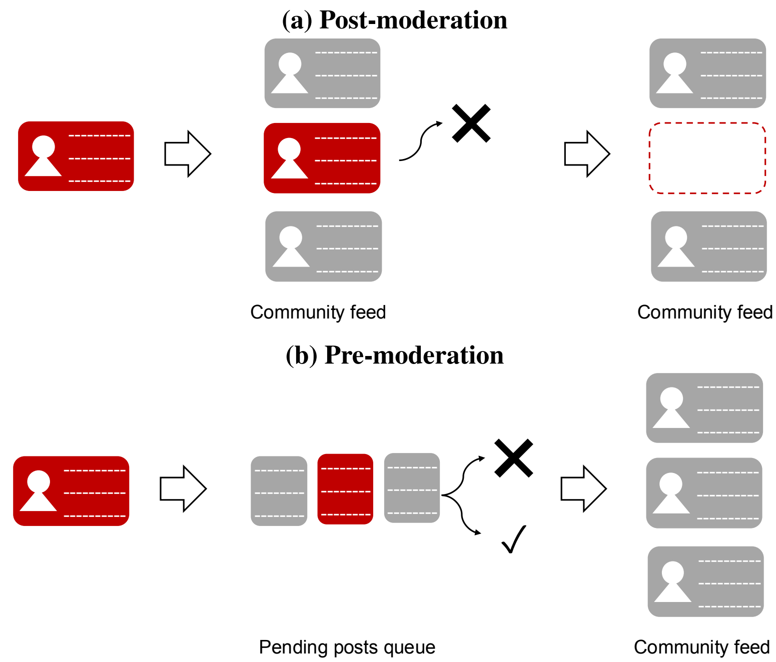
— Tim Courtney, Former Community and Experience Manager at LEGO Ideas
COMMUNITY SIGNAL PODCAST, FEBRUARY 24, 2020 (O'KEEFE, 2020)

Firms increasingly place the development of online brand communities (OBCs) at the core of their strategies to foster customer loyalty and transform passive consumers into active brand advocates (Bapna et al., 2019; Liang et al., 2024). In 2008, an influential article in *The Wall Street Journal* titled "Why Most Online Communities Fail" argued that many OBCs initially struggled to gain traction because firms failed to offer users a compelling and memorable experience (Worthen, 2008). Since then, many firms have successfully overcome this initial hurdle, cultivating vibrant and successful brand communities (Safadi et al., 2025). When managed well, these mature OBCs serve as important sources of innovation and brand value (Dahlander et al., 2023; Faraj et al., 2016). For example, Apple's community brings together passionate users, many of whom voluntarily contribute by answering questions and acting as informal ambassadors for the brand. Similarly, LEGO has created a thriving community where enthusiasts share ideas, connect with others, and participate in contests, with some selected designs even being turned into official products.

However, the very success and scale of these communities now present distinct managerial challenges: firms must sustain a steady flow of user-generated content (UGC) while also ensuring that these contributions align with brand values. The LEGO community provides a vivid example of this tension. Despite strong community support, LEGO ultimately decided not to produce the *Serenity* spaceship from the sci-fi series *Firefly*. This decision reflected concerns about the show's mature themes—which conflicted with LEGO's child-friendly image—prompting the firm to subsequently tighten its content moderation rules.

Balancing user participation with the enforcement of community standards is a central challenge in platform governance (He et al., 2024). To uphold these standards, OBCs must implement content moderation strategies that promote positive interactions and sustain high-quality contributions. The literature on content

Figure 1. Content Moderation Policies



Note: Figure 1a shows how post-moderation allows moderators to delete posts that violate community guidelines (in red) *after* they are published in the community. Figure 1b shows how pre-moderation allows moderators to filter these posts *before* they become visible to other members. This figure is adapted from Ribeiro et al. (2022, p. 335).

moderation has predominantly focused on post-moderation (see Figure 1a), wherein user submissions are reviewed only after publication (Gillespie, 2018; Ribeiro et al., 2022). Large communities such as Quora, Stack Overflow, and Yelp typically rely on user-driven reporting systems, in which community members flag content that may violate guidelines. These reports are subsequently reviewed by moderators—often volunteers or automated systems—who decide whether the content should be removed or retained (He et al., 2024). However, the main drawback of this approach is that harmful or inappropriate content remains publicly accessible until action is taken. In the context of OBCs, such delays can enable the spread of harassment or other undesirable content, risking negative brand experiences and reputational damage for the firm.

Against this backdrop, some OBCs have implemented pre-moderation policies (see Figure 1b), where content is reviewed before being made publicly visible. For instance, LEGO enforces full pre-moderation on its child-friendly pages; SAP pre-moderates posts from new members in its SAP Community Network;

and the calendar scheduling application Calendly enables pre-moderation in its community forum during off-hours to curb spam and abuse. *The Guardian* applies pre-moderation to articles and blog posts covering sensitive topics. Firm-sponsored recipe platforms such as AllRecipes and CookingHub require content approval for submitted recipes, with AllRecipes also reviewing user-submitted reviews and photographs.

On the one hand, pre-moderation policies grant moderators complete control over content published on the platform. This proactive approach ensures that all material aligns with safety standards, helping to keep "corporate brands untarnished" (Gillespie, 2018, p. 79). Furthermore, firms avoid the negative downstream interactions associated with post-publication modifications or deletions. Moderated communities also tend to be more attractive to users (Wise et al., 2006). Thus, pre-moderation has the potential to enhance the relevance of contributions, thereby fostering a stronger sense of attachment to the community. On the other hand, the review process inherent in pre-moderation introduces delays that disrupt the immediacy central to online interactions (Feng et al., 2024). This friction can significantly deter user participation, potentially reducing both the frequency and novelty of contributions. The added back-and-forth inherent in pre-moderation can be particularly discouraging for new or less engaged members. These users, who are still developing their relationship with the brand and its community, may be less likely to log in frequently, and thus, more likely to abandon their contributions when faced with additional hurdles.

Given these trade-offs, it is surprising that the empirical effects of pre-moderation on user engagement and content characteristics in OBCs remain largely unexplored. Addressing this critical gap, this study investigates how pre-moderation influences user contributions and the nature of UGC within OBCs. Specifically, I examine how pre-moderation affects (i) the volume of user contributions and participation across different user groups; (ii) the novelty of published content; and (iii) audience engagement with published content.

To address these questions, I examine the introduction of a pre-moderation policy on Rezeptwelt, a prominent German online brand community (OBC) operated by Vorwerk, which hosts over 80,000 recipes for its widely used multi-functional kitchen appliance, the Thermomix.[1] Given concerns that digital platforms may threaten domestic food preparation (Babar et al., 2025) and healthy eating (Li et al., 2024), the Thermomix has gained popularity as an efficient tool for time-constrained users to prepare fresh meals. Thermomix recipes provide step-by-step instructions and precise appliance settings, making Rezeptwelt a central hub for enthusiasts to discover, share, and discuss tailored recipes. This setting offers a compelling empirical context for studying pre-moderation. First, Thermomix users often form a tightly knit community, sometimes referred to as the “Thermomix cult” (Ribeiro, 2021). Such deep brand engagement means that changes to the platform are likely to face close scrutiny. Second, because food is a vital cultural artifact (Waldfogel, 2020), recipe sharing is deeply personal for many users. Consequently, moderating recipes may provoke strong emotional reactions rooted in the personal significance of cooking.

In July 2018, the platform unexpectedly announced a new pre-moderation policy: all newly submitted recipes would undergo review, either approved by moderators for publication or returned with feedback. Similar OBCs operated by Vorwerk in other Thermomix markets, such as France’s Espace Recettes, share Rezeptwelt’s features but did not adopt this policy. I collected a novel quantitative dataset from Rezeptwelt to study changes in contribution behavior six months before and after the OBC’s announcement to adopt a pre-moderation policy. To provide a robust control group, I paired this dataset with data from the French OBC Espace Recettes.[2] Using a difference-in-differences (DID) approach, I compared contribution behavior on Rezeptwelt with Espace Recettes.

Following the policy, Rezeptwelt users published 4.4% fewer recipes per month over the subsequent six months. This reduction is primarily observed among “inexperienced” users (those with below median recipes published before the observation period), who decreased their published recipes by 6.5%. In contrast, experienced users showed a smaller, statistically insignificant decrease. What explains these reactions? I

[1] The platform hosted 81,961 recipes as of 14 August 2025. https://www.rezeptwelt.de/suche?rec_all=0&search=.

[2] In 2020, Germany and France were the two largest markets for the Thermomix, making Rezeptwelt and Espace Recettes the two largest Thermomix OBCs.

consider two mechanisms that have been theorized in the context of pre-moderation policies (Gillespie, 2018): (i) the reduction in activity could be attributed to the OBC's decision to constrain the publication of certain types of recipes, or (ii) the reduction might stem from users' reluctance to submit recipes, possibly due to their unwillingness to accept the delay between posting content and its appearance on the platform.

First, the decline in published recipes for inexperienced users is concentrated in categories with a disproportionately high number of recipes compared to Espace Recettes. This pattern suggests a potential strategy by the firm to limit the publication of new recipes in oversaturated areas. This effect is not observed among experienced users, indicating that the filtering process allows for *some* new recipes in these categories, particularly those submitted by established members who are more likely to contribute valuable content.

Second, I examine whether publication delays *unintentionally* reduce user engagement. To investigate this, I leverage variations in publication speed due to human involvement in pre-moderation, finding that the proportion of recipes published on Rezeptwelt during weekends significantly decreases. This indicates that users posting on weekends experience longer publication delays and less immediate feedback. The effect of this friction is universal for inexperienced users, who decrease their recipe contributions regardless of whether they are weekend or non-weekend posters. However, for experienced users, the effect is heterogeneous: while weekend posters publish significantly fewer recipes, non-weekend posters slightly increase their recipes.

Finally, the recipes users *do* publish on Rezeptwelt tend to have less novel titles. Using a Sentence-BERT (SBERT) model (e.g., Quinn & Gutt, 2025), I find a significant decline in title novelty, measured as the cosine similarity to the existing corpus of recipes, and no significant change in community engagement, as measured by the number of likes (e.g., Wang & Greenwood, 2025). Together, these results hint at an unintended consequence of the pre-moderation policy: it suppresses creativity without increasing popularity.

This paper makes three contributions. First, by empirically examining pre-moderation in online brand communities (OBCs), it informs research on OBC governance (Bapna et al., 2019; Liang et al., 2024; Safadi et al., 2025). I highlight the tension firms face between fostering community engagement and ensuring content appropriateness, particularly under proactive content control. Second, this study advances the literature on content moderation in online communities (He et al., 2024; Jiang et al., 2023; Mudambi et al., 2024) by providing novel empirical evidence on the multifaceted effects of pre-moderation. The analysis shows that such policies can disproportionately discourage new users from participating, and are associated with a decline in content novelty—suggesting reduced creativity—while also failing to improve overall engagement. Third, the paper contributes to research on user motivation for producing UGC (Forderer & Burtch, 2024; He et al., 2023; Wang & Greenwood, 2025). The findings reveal that even experienced users can be significantly deterred by operational frictions, such as publication delays, underscoring that immediacy of content visibility is a key motivator for active participation.

2 RELATED LITERATURE

2.1 Governance in Online Brand Communities

OBCs are platforms designed to foster relationships among customers and between customers, the firm, and its products (Bapna et al., 2019; Liang et al., 2024). While some OBCs are organized independently (Thompson & Sinha, 2008), this study primarily focuses on firm-sponsored brand communities, which are directly hosted by the firm (Safadi et al., 2025). Research highlights OBCs' importance in facilitating knowledge exchange and strengthening brand ties, making them a significant source of competitive advantage (Dahlander et al., 2023). Active participation in OBCs has been linked to the adoption of new products (Thompson & Sinha, 2008), frequency of store visits (Rishika et al., 2013), and users' willingness to pay for premium services (Oestreicher-Singer & Zalmanson, 2013). In fact, a recent survey suggests that 66% of OBC members say they are loyal to the brand, and 27% of customers say belonging to an OBC influences their decision to do business with the brand (Hyken, 2023).

However, this reliance on OBCs raises the question of how firms are responding to the challenges of maintaining active contributions and ensuring content aligns with brand values. Recent scholarship has begun to unpack the platform mechanisms that drive (dis)engagement in OBCs, including features such as likes (Liang et al., 2024), crowd voting (Dahlander et al., 2023), and reputation systems (Hanson et al., 2019). Beyond these design elements, the nature and mix of user-generated and marketer-generated content play a pivotal role in shaping member engagement (Goh et al., 2013). Among the governance approaches that influence this content, content moderation has become especially important in online communities (He et al., 2024), particularly within OBCs (Safadi et al., 2025). Moderation broadly encompasses "governance mechanisms that structure participation in a community to facilitate cooperation and prevent abuse" (Grimmelmann, 2015, p. 45). Its primary function is to mitigate conflicts among users, ensure contributions remain relevant and on-topic, and curb disruptive behavior. A well-functioning moderation system is thus crucial for maintaining the core purposes of online communities, i.e., information sharing and social interactions (Kraut et al., 2012).

The implementation of content moderation frequently triggers debate over how much control firms should exert over their communities. While early views often considered community control to be an "illusion" (Fournier & Lee, 2009, p. 110), firms have nevertheless actively sought to assert it. As a former community manager at LEGO illustrated: "We would archive some stuff, if we knew we couldn't make it and say, 'Hey, we changed the rules. We're sorry, we've got to archive this. Thank you, but unfortunately, we're not able to do anything with it.' [...] We would systematically narrow that scope so that even though there's some ambiguity in the beginning, we're striving toward clarity" (O'Keefe, 2020). This drive toward formalization is a common theme, with other research observing a shift toward more firm-based governance mechanisms over time, such as the introduction of specific guidelines on how religious posts should be handled by moderators in a firm-sponsored patient community (Safadi et al., 2025).

Despite its crucial role, little is known about the effects of pre-moderation policies on user engagement in OBCs, as existing research has focused predominantly on post-moderation and on communities with user-led governance (Ribeiro et al., 2022). This policy creates a fundamental trade-off: it guarantees UGC aligns with brand values, but the associated delays and friction may discourage user contributions. This study addresses this gap by investigating a well-documented pre-moderation policy change in a major OBC.

2.2 Consequences of Content Moderation

Outside the context of OBCs, a growing stream of research explores the behavioral consequences of content moderation, particularly concerning user motivation and participation (e.g., Cao et al., 2024; Jiang et al., 2023; Mudambi et al., 2024). Sustaining user contributions is critical for online communities, with individuals often driven by motivations such as intrinsic enjoyment and a desire for acknowledgement from others (Jeppesen & Frederiksen, 2006; Wasko & Faraj, 2005).

A common thread in this literature is that moderation can undermine the value users derive from visibility and interaction with their contributions. For example, Mudambi et al. (2024) demonstrate that reducing the visibility of posts can lower participation, while Cao et al. (2024) show that punitive responses to low-quality content may discourage future contributions. Similarly, studies suggest that merely providing moderation transparency can induce users to invest less effort (Jiang et al., 2023). Beyond these direct effects, research also highlights more nuanced behavioral responses. For example, Jiménez-Durán (2023) finds that while content removal may not change the activity level of removed individuals, those targeted by attacks might increase their contributions.

This study investigates how restrictive *ex ante* moderation—mandatory review before content becomes public—affects contribution behavior. The most comparable work on this topic is Ribeiro et al. (2022), who examined post approvals as a form of participation control central to community-level moderation in Facebook Groups. However, this study differs in key aspects. First, it analyzes behavior over an extended period in a well-established OBC that serves not only as a platform for social interaction but also as a publicly accessible knowledge base for recipes. Second, it provides a more nuanced exploration of how different user groups respond to approval processes. Specifically, it investigates the differential effects on experienced and inexperienced contributors, shedding light on variations in posting behavior between these groups.

3 HYPOTHESES DEVELOPMENT

3.1 Number of Contributions

In what follows, I argue that the unexpected introduction of a pre-moderation policy constitutes a significant governance change, which, in turn, causes users to publish fewer contributions in the OBC. According to prior research (Gillespie, 2018), this adjustment can be explained through two distinct theoretical mechanisms: strategic filtering and participation friction.

The first mechanism, strategic filtering, positions the OBC as an active editorial gatekeeper, signaling a tighter control over content (Gillespie, 2018). This implies the OBC may proactively filter contributions that violate guidelines or are deemed redundant, thereby directly impacting what gets published (Ribeiro et al., 2022). As the scope of acceptable contributions becomes narrower, users may publish fewer contributions (see O'Keefe, 2020). Faced with uncertainty about content acceptance and reduced creative freedom, users may adapt their content to the new policy, stop producing content that falls outside the tightened scope, or disengage entirely. Similar patterns exist in academic publishing, where editorial gatekeeping selectively shapes the body of published work, which may lead authors to refrain from submitting certain paper types or reduce the number of papers to align with a journal's scope (Ellison, 2002; Gans & Shepherd, 1994).

The second mechanism, participation friction, introduces costs and disincentives that increase the burden on users to contribute (Gillespie, 2018). These frictions manifest as increased effort, extended time delays between submission and publication, or requirements for revisions. This disrupts the immediacy often valued in online interactions (Feng et al., 2024). Prior research on self-control suggests that people are impatient and seek immediate rewards (e.g., O'Donoghue & Rabin, 1999). Thus, the instant publication of a contribution likely serves as immediate gratification, whereas delaying posts could significantly reduce user engagement (Greis et al., 2014). This heightened friction induced by pre-moderation may therefore negatively impact perceived rewards for contributing, ultimately leading users to become frustrated and reduce their contributions (Kiene et al., 2016). Indeed, delays in the review process are a common complaint among content creators, as prompt publication is often crucial to their work's relevance (Spencer, 2016). Users in Western contexts, in particular, have developed an expectation of instant publications, making them unwilling to accept a delay between posting content and having it appear (Gillespie, 2018).

Taken together, this work suggests that the introduction of pre-moderation leads to fewer published contributions in the OBC by (i) establishing strategic filtering, which narrows the scope of acceptable content and prompts user adaptation or disengagement, and (ii) creating participation friction, which introduces costs and delays that deter user engagement. Therefore, I propose the following hypothesis.

H1. *Pre-moderation is negatively associated with the number of published contributions from users.*

Inexperienced users, by definition, have less familiarity with the OBC's established norms (Halfaker et al., 2013; Pethig et al., 2025). Thus, they are more likely to submit content that might be deemed undesirable by the newly tightened editorial stance, leading to a higher probability of rejection or demands for revision. This increased likelihood of direct filtering, or the anticipation of it, can disproportionately discourage inexperienced contributors who are still learning the ropes (Kraut et al., 2012). Thus, they are more sensitive to initial hurdles and rejections (Aaltonen & Wattal, 2025). Experienced users, in contrast, are more likely to already conform to acceptable content standards or can adapt more easily, thus facing less direct filtering and friction in their attempt to contribute to the community. Therefore, I propose the following hypothesis.

H2. *Pre-moderation is negatively associated with the number of published contributions to a greater extent for inexperienced users than experienced users.*

3.2 Novelty of Contributions

Pre-moderation could lead to more novel content. This expectation primarily results from strategic filtering. When moderators actively review submissions before they appear, they have an opportunity to prevent redundancy (Gillespie, 2018). By filtering out content that is too similar to existing contributions, moderators may curate a collection where each new addition offers more novelty. This process also incentivizes originality, as recognition by the firm may act as a positive reinforcement if users know their content will be reviewed

(Jeppesen & Frederiksen, 2006; Joyce & Kraut, 2006). Hence, under the pre-moderation policy, users might put more effort into creating truly original or innovative submissions (e.g., Jeppesen & Frederiksen, 2006). Furthermore, if unoriginal content is more readily rejected or requires revisions, contributors strive to avoid such negative experiences by producing more distinctive submissions to ensure smooth publication. Essentially, the pre-moderation process might act as a filter, raising the bar for content novelty.

Conversely, pre-moderation could also lead to a decrease in the novelty of published content. This prediction arises from a few potential mechanisms. Faced with the uncertainty of content acceptance under pre-moderation, users might become more risk-averse. As rules tighten, users may choose to submit safer, more conventional content types to minimize the chance of rejection or the need for revisions (Kiene et al., 2016). In turn, this might discourage experimentation and highly novel submissions. For example, a user from the Rezeptwelt community noted that “this annoying and unnecessary approval process ultimately prevents many creative recipes.” Furthermore, even without explicit rules, repeated filtering decisions can implicitly create a narrower “acceptable” range of content (O’Keefe, 2020). Users might learn to conform to these perceived preferences, leading to a homogenization of contributions at the cost of a reduction in diversity and novelty. The added friction—like increased effort and delayed gratification—might also disproportionately deter users who are intrinsically motivated and more inclined to create unique or experimental content (Jeppesen & Frederiksen, 2006), as such submissions could face higher uncertainty of acceptance.

Overall, the existing work does not point to an unequivocal impact of pre-moderation on content novelty. Therefore, I propose the following competing hypotheses.

H3a. *Pre-moderation is positively associated with the novelty of published contributions.*

H3b. *Pre-moderation is negatively associated with the novelty of published contributions.*

3.3 Number of Likes for Contributions

Pre-moderation could lead to an increase in the number of likes that contributions receive. This expectation is largely tied to the idea that pre-moderation, through strategic filtering, can enhance the overall relevance of the content that ultimately appears on the platform. If moderators effectively screen out redundant, low-quality, or off-topic submissions, the remaining content pool would theoretically be more valuable and appealing (Ribeiro et al., 2022). A more relevant content pool could, in turn, spur more engagement, particularly in terms of broad positive reception. Users might be more inclined to signal their approval by liking contributions that are genuinely interesting, useful, or well-curated, rather than sifting through less valuable content (Wang & Greenwood, 2025). This positive association suggests that pre-moderation successfully improves the signal-to-noise ratio in OBCs (Malhotra et al., 1997), leading to more broadly appreciated and popular content.

Conversely, pre-moderation could lead to a decrease in the number of likes that contributions receive. This prediction is driven by several potential factors related to both strategic filtering and overall audience engagement. First, if strategic filtering inadvertently leads to a reduction in content novelty (as per H3b), it could make the OBC less exciting (Kiene et al., 2016). Less novel or varied content might simply generate less interest and fewer broad signals of approval, leading to a decline in likes. Second, the reduction in the overall volume of contributions (as per H1) means there are simply fewer new pieces of content available for users to like, potentially leading to a natural decrease in liking activity. Third, while liking itself is a lightweight, low-effort action (Yang et al., 2019), a diminished overall "liveliness" of the platform might subtly dampen audience engagement as the excitement of users fades (Torkjazi et al., 2009). This could occur if participation friction affects the volume and continuous flow of new contributions, making the platform appear less active and reducing users' urge to actively participate or signal approval.

As with the novelty of contributions, the existing work does not point to an unequivocal impact of pre-moderation on the number of likes. Therefore, I propose the following competing hypotheses.

H4a. *Pre-moderation is positively associated with the number of likes that published contributions receive.*

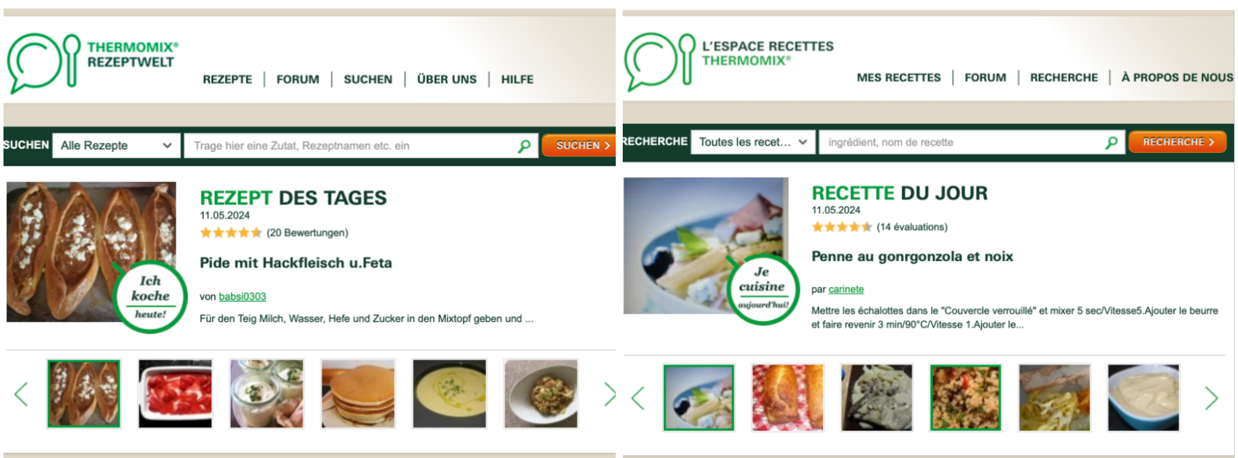
H4b. *Pre-moderation is negatively associated with the number of likes that published contributions receive.*

4 RESEARCH SETTING AND DATA

4.1 Study Context

The empirical setting comprises Rezeptwelt and Espace Recettes, the two largest OBCs dedicated for sharing Thermomix recipes. The Thermomix is a widely popular multi-functional kitchen appliance. Both platforms share identical functionality and layout (Figure 2). Recipes on these platforms offer step-by-step instructions, including specific Thermomix settings like speed levels. By the end of my observation period, Rezeptwelt hosted around 74,000 recipes, while Espace Recettes had approximately 33,000.

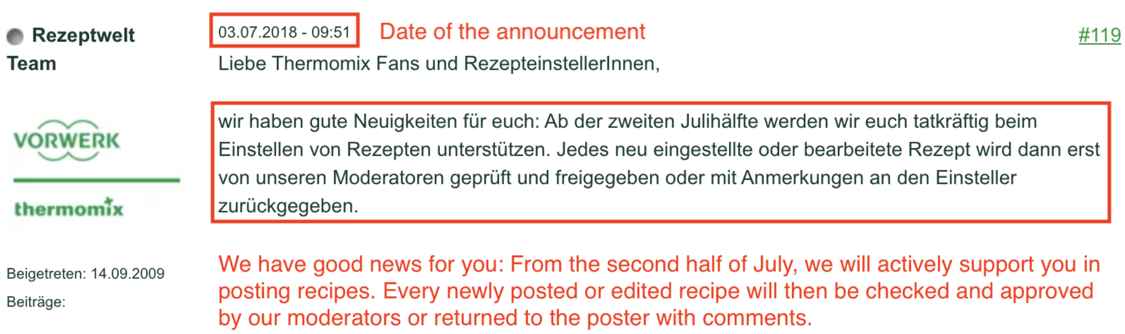
Figure 2. Rezeptwelt (left) and Espace Recettes (right)



On July 3, 2018, Rezeptwelt introduced a new pre-moderation policy, a change not adopted by its sister community, Espace Recettes. The announcement stated that all newly submitted or edited recipes would require moderator review and approval before publication (Figure 3). Submissions that were rejected would be returned to the poster with comments. The policy aimed to prevent the publication of incomplete, inedible, or potentially hazardous recipes. Moderators also specified clear approval criteria, such as complete ingredient lists and safety (e.g., no instructions that seal the device with a kitchen towel). This shift granted Rezeptwelt increased control over its content, intending to maintain a streamlined, high-quality repository of user-generated recipes. Although moderators cited similar practices on other platforms, the policy's introduction surprised many users because it occurred after the community was already well-established.

This sparked immediate criticism in the Rezeptwelt forum. For example, one user noted, "I find the control by TM [Thermomix] completely pointless. We are not Vorwerk's henchmen." Another user captured the sentiment that the old system worked just fine, as members had been resolving unclarities in user-generated recipes among themselves for years: "Honestly, you could spare yourselves this effort, since your members have been doing that job for you for years."

Figure 3. The Announcement



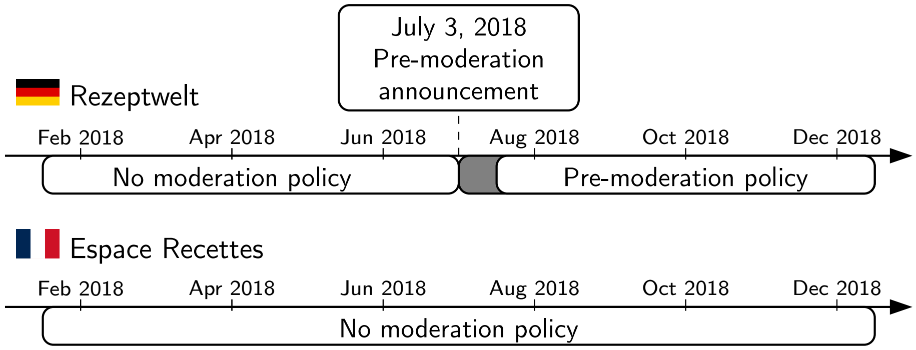
4.2 Identification Strategy

As illustrated in Figure 4, I leverage Rezeptwelt's pre-moderation policy announcement as a natural experiment to evaluate its effect. This allows for a comparison of contributor behavior on Rezeptwelt (the treated group) with Espace Recettes (the control group). The policy announcement serves as an exogenous shock, enabling the assessment of its consequences on contribution behavior. While the timing of the policy announcement could have been influenced by pre-existing platform issues, I address this potential for endogeneity through extensive analyses of pre-existing trends and matching. Furthermore, I perform falsification tests using placebo dates to ensure the robustness of my identification strategy.

4.3 Data

I collected all historical recipe data from Rezeptwelt and Espace Recettes, including title, content, ingredients, category, user ID, associated comments, and publication date. I then constructed a panel dataset with the user as the cross-sectional unit and the month relative to the policy announcement as the time unit. I applied three key restrictions to the data. First, I limited the sample to users who posted at least one recipe within the

Figure 4. Identification Strategy



Notes: This figure illustrates the identification strategy employed in this study. I exploit the announcement of the pre-moderation policy on 3 July 2018 (dashed line) as an exogenous shock to contributor behavior on Rezeptwelt. Starting on January 3, 2018, I track a sample of active contributors from Rezeptwelt and Espace Recettes over a monthly time-series, covering six months prior to the announcement (pretreatment period) until six months after the announcement (posttreatment period).

Table 1. Descriptive Statistics

Variable	Description	Observations	Mean	Standard deviation	Minimum	Maximum
$\log(\mathit{NumRecipes})_i$	The number of recipes published by user i in month t	13,800	0.11	0.31	0.00	3.18
Treat_i	A dummy for whether user i was active on Rezeptwelt	13,800	0.75	0.43	0.00	1.00
$\log(\mathit{Tenure})_i$	The number of months since user i's first recipe post until month t	13,800	3.34	0.69	0.00	4.72

Notes: I add the value of 1 to all continuous variables prior to performing the log transformation.

analysis window (six months before to six months after the announcement), ensuring they were active around the policy change. Second, I required that users be observed posting at least one recipe *before* this time window. This approach allowed me to construct a balanced panel for each user. Finally, I excluded users who had deleted their profiles by the data collection date and those affiliated with Vorwerk. With these restrictions, the final sample comprises 868 users on Rezeptwelt and 282 on Espace Recettes, totaling 1,150 users across both platforms. For the main analysis, these users were tracked from January 3, 2018, to January 2, 2019, resulting in 13,800 user-month observations. Table 1 presents the descriptive statistics for this sample.

4.4 Matching

To mitigate concerns over heterogeneity between users on Rezeptwelt and Espace Recettes as well as to address the fact that there are more Rezeptwelt users than Espace Recettes users, I employ a coarsened exact match (CEM). I use data up to the pre-announcement month to match users on (i) the cumulative number of recipes (to obtain users that are comparable in their contributions to the platform), (ii) on tenure (to account for differences in users' experience), and (iii) on the cumulative average number of likes and comments that recipes received (to obtain users whose recipes have comparable engagement). I coarsen the number of likes using eight equally spaced cut points and the other variables using six equally spaced cut points (Wang et al., 2022). Finally, I enforce k2k matching.

To assess the effectiveness of the matching, I compare Rezeptwelt and Espace Recettes users in Table 2. Column (1) reports a t-test for means to understand differences across treated and control users and shows no significant differences in cumulative number of recipes, tenure, and average number of comments before matching. Groups differed in likes. Column (2) shows that, after matching, these differences are insignificant.

Table 2. Matching Effectiveness

	(1) Full sample					(2) Matched sample				
	Rezeptwelt		Espace Recettes			Rezeptwelt		Espace Recettes		
Variables	Obs.	Mean	Obs.	Mean	Diff.	Obs.	Mean	Obs.	Mean	Diff.
log(*CumNumRecipes*)	868	1.95	282	1.93	−0.02	228	1.82	228	1.79	−0.03
log(*Tenure*)	868	3.32	282	3.39	0.07	228	3.33	228	3.32	−0.01
log(*CumAvgLikes*)	868	1.70	282	1.16	−0.54***	228	1.28	228	1.17	−0.11
log(*CumAvgComments*)	868	1.51	282	1.50	0.00	228	1.28	228	1.43	0.15

Notes: Obs. = observations, Diff. = difference in means. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

5 EMPIRICAL ANALYSIS AND RESULTS

5.1 Effect of Pre-Moderation on the Number of Published Recipes (H1 and H2)

5.1.1 Empirical Framework

The baseline specification aims to estimate the impact of the pre-moderation policy on the number of published recipes. In particular, I use a DID framework to estimate the following:

$$y_{it} = \beta_0 + \beta_1 Treat_i \times After_t + \gamma_i + \tau_t + \varepsilon_{it}. \tag{1}$$

The unit of observation of my analysis is the user-month. The main dependent variable y_{it} represents content contributions, measured by the log-transformed number of recipes posted by user i in month t. The variable *Treat* is a dummy that equals 1 if user i is active on Rezeptwelt and 0 if user i is active on Espace Recettes. *After* equals 0 before the announcement and 1 otherwise. The key independent variable is *Treat*×*After*, a time-variant, binary variable that equals 1 if user i is on Rezeptwelt and the platform had announced the pre-moderation policy in month t; 0 otherwise.[3] I track users over time, and thus γ_i captures user fixed effects, which helps to account for time-invariant user-specific characteristics such as gender, experience using the Thermomix, and cooking skills, among other factors. τ_t captures month fixed effects. Finally, ε_{it} captures any idiosyncratic random errors. I cluster standard errors at the user level. With this specification, the coefficient β_1 is the DID estimator, which measures the impact of the pre-moderation policy.

For the main analyses and robustness checks, I enhance the baseline specification by replacing the month fixed effects (τ_t) with a set of category-by-month fixed effects. The *Category* variable for user i is defined as the category to which they have contributed most frequently.[4] Since the pre-moderation policy might directly influence the categories a user contributes to, incorporating time-varying controls could bias the coefficient estimates. To address this, I use the user's most frequent category prior to the observation period, interacted with month fixed effects. This approach accounts for time-varying, category-specific shocks—such as fluctuations in activity during certain months among users who typically post in specific categories (e.g., users interested in baking might post more recipes during the Christmas season).

The previous regression captures the average effect of the pre-moderation policy. It is possible that unobservable factors, which are native to Rezeptwelt or Espace Recettes, are causing heterogeneity in the

[3]Note that the main effects of *Treat* and *After* are unidentified because they are subsumed by the user-specific and time-specific fixed effects, respectively.

[4]The main effect of *Category* is subsumed by the user-specifc fixed effects. If a user has contributed equally to multiple categories, the category of their most recent contribution among these is used to break the tie.

pretreatment contributions of users. To alleviate this concern, I provide evidence from relative models by explicitly showing leads and lags to ensure the absence of pretrends between users on Rezeptwelt and Espace Recettes and an impact only after the policy change. To execute the relative time model, I interact the treatment variable with the individual month dummies, $\tau_{t,m}$, indicating that the current month t is m months before (in the case of a negative m) or after (in the case of a positive m) the month when the pre-moderation was announced. The model specification is as follows:

$$y_{it} = \beta_0 + \sum_{m=-6}^{5} \lambda_m Treat_i \times \tau_{t,m} + \gamma_i + \tau_t + \varepsilon_{it}. \tag{2}$$

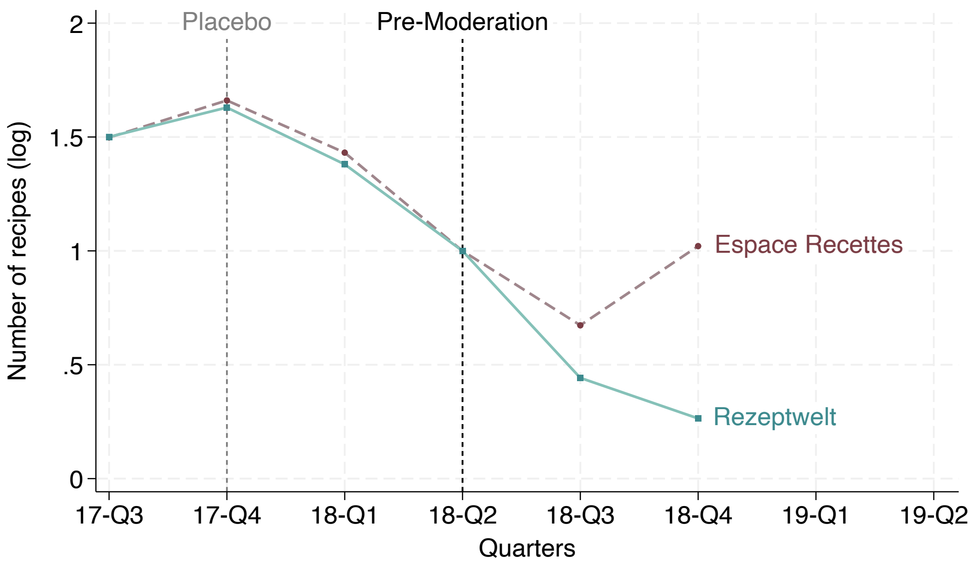
Hence, the coefficients λ_m are the estimates of interest, which allow me to estimate coefficients monthly to analyze the temporal dynamics associated with the pre-moderation policy. I estimate the coefficients relative to the omitted period, the one before the announcement of the pre-moderation policy. As with Equation (1), I look at the six months before and six months after the announcement.

5.1.2 Model-Free Evidence

I begin by presenting model-free evidence on recipe publication, aggregated at the quarter level to smooth out short-term fluctuations and highlight macro-level trends. Figure 5 illustrates these trends for all recipes published on Rezeptwelt and Espace Recettes, excluding those from deleted or Vorwerk-affiliated users. For the analysis, the number of log-transformed recipes published on each platform is normalized to the level observed one quarter before the announcement of the pre-moderation policy (indicated by the black dashed line). The data covers the four quarters preceding and the two quarters following the announcement (Q3 2017 to Q4 2018). As Figure 5 shows, both platforms exhibit parallel trends during the four quarters prior to the announcement. They follow a similar upward trajectory from Q3 to Q4 in 2017, which is then followed by a comparable downward trend. However, a noticeable divergence between the platforms becomes evident in the two quarters following the announcement. Espace Recettes shows a typical rebound from Q3 to Q4 after a steady decline, while Rezeptwelt exhibits a sharp downward trajectory followed by an atypical continued decrease during the same period.

Additionally, the grey dashed line in Figure 5 marks one of the placebo test dates, which is set to six months before the policy announcement. This test, described in more detail in Section 5.1.5, is a falsification exercise. A preliminary visual inspection of the figure confirms the absence of a relationship around the placebo date, which helps to support the validity of my identification strategy.

Figure 5. Recipes Published on Rezeptwelt and Espace Recettes

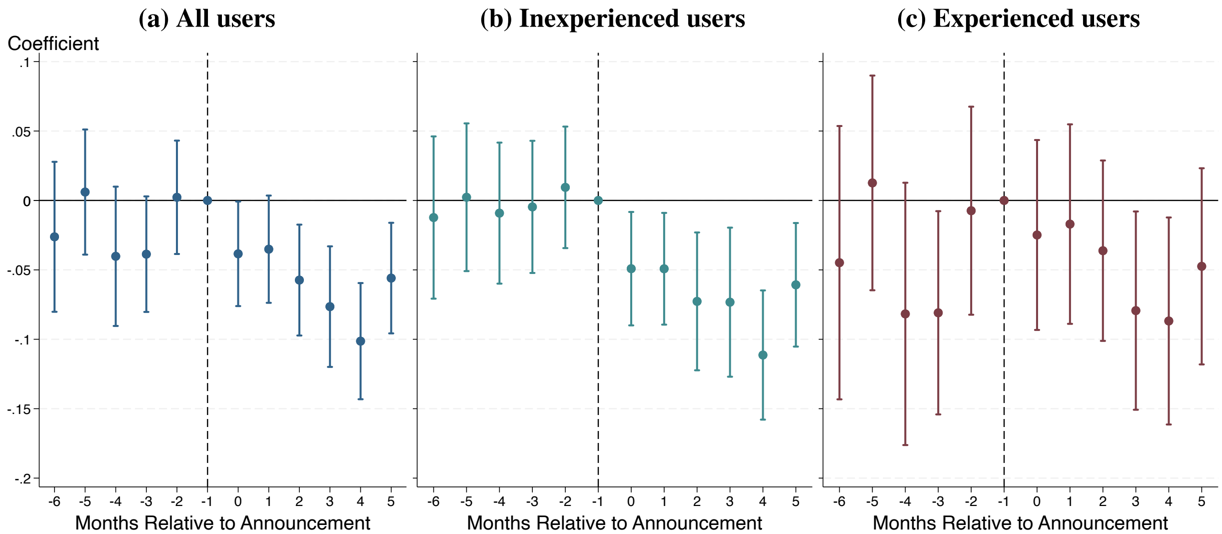


Notes: This figure displays the log-transformed number of recipes published on Rezeptwelt (treatment group) and Espace Recettes (control group). Both series are normalized to a value of 1 in the quarter preceding the introduction of the pre-moderation policy (denoted by the black dashed line). The grey dashed line marks a placebo date, which is set to six months before the policy announcement (Section 5.1.5). The unit of observation is a calendar quarter.

5.1.3 Regression Results

Figure 6a displays the regression coefficients from Equation (2), estimated using the full sample (see Column 1 of Table A1 in Appendix A). As can be seen from this analysis, users of Rezeptwelt and Espace Recettes are indistinguishable in terms of their recipe-posting activity before the pre-moderation policy. However, within the first month after the announcement the effect manifests. The number of published recipes drops and this drop remains permanent throughout the study period. Column 4 of Table A1 replicates the results using the matched sample and yields consistent findings.

Figure 6. Coefficients of the Relative Time Models



Notes: This figure reports the estimated coefficients (and respective 90 percent confidence intervals with standard errors clustered at the user level) from the regressions of *NumRecipes* on the interaction between treatment and months relative to the announcement (Equation (2)).

Table 3 presents the results of Equation (1). Column 1 presents the results of the DID estimation using the full sample. The coefficient of Treat×After is negative and statistically significant. Users exposed to the pre-moderation policy published 4.4% fewer recipes per month in the six months following the announcement, supporting H1.[5] Column 2 replaces the month fixed effects with category-by-month fixed effects, and the estimates are consistent. Finally, Columns 3 and 4 show the results from the CEM analysis, which are also consistently negative and statistically significant.

The next step in analyzing how the pre-moderation policy changed recipe publications is to understand who among the users decreased the number of recipes published (H2). To this end, I capture the number of recipes posted by each user prior to the observation period. I then divide the sample into experienced and inexperienced users along the pre-observation sample median. Figure 6b shows that inexperienced users publish fewer recipes as soon as the policy is announced. As in Figure 6a, the response is immediate and sharp, with no discernible pretrend prior to the policy change. Figure 6c shows that the pre-moderation policy had minimal impact on the number of recipes posted by experienced users. The individual coefficients for

[5] I calculated the effect size as $exp(-0.045) - 1 = 4.4\%$.

Table 3. Effect of Pre-Moderation on Recipes

	log(*NumRecipes*)			
	All users (1)	All users (2)	All users + CEM (3)	All users + CEM (4)
Treat×After	−0.045*** (0.013)	−0.048*** (0.014)	−0.050*** (0.015)	−0.059*** (0.018)
Fixed Effects				
User	✓	✓	✓	✓
Month	✓	—	✓	—
Category×Month	—	✓	—	✓
Observations	13,800	13,800	5,472	5,472
R-squared	0.239	0.248	0.151	0.174

Notes: Columns 1 and 3 show the baseline DID. Columns 2 and 4 replace the month fixed effects with category-by-month fixed effects. Robust standard errors are clustered at the user level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

each month are largely insignificant and smaller in magnitude than those obtained for inexperienced users. Thus, the pre-moderation policy disproportionally affects inexperienced users, potentially due to the extra effort involved in publishing a recipe. This suggests that experienced users, possessing a better understanding of community standards, likely faced fewer required changes, thereby increasing the probability of their contributions appearing on the platform.

Table 4 presents the results of the DID analysis. Column 1 shows a statistically significant decline in the number of recipes posted per month by inexperienced users, with an average decrease of about 6.5%. In contrast, Column 2 indicates that experienced users exhibit a much smaller, non-statistically significant decrease of only 1.5%. Importantly, the coefficients in Column 1 are significantly smaller than those in Column 2 ($p < 0.05$), indicating that user experience significantly moderates the pre-moderation policy's effect on published recipes. Finally, Columns 3 and 4 confirm that these differential effects are consistent when using the CEM sample. These findings provide support for H2.

5.1.4 Mechanism Exploration

Strategic Filtering. To explore the mechanisms underlying the changes in contribution behavior, I examine whether the platform likely restricts certain recipes from appearing in the community. Specifically, I focus

Table 4. Effect of Pre-Moderation on Recipes by User Experience

	log(*NumRecipes*)			
	Inexperienced (1)	Experienced (2)	Inexperienced + CEM (3)	Experienced + CEM (4)
Treat×*After*	−0.067*** (0.015)	−0.015 (0.020)	−0.078*** (0.019)	−0.006 (0.026)
Fixed Effects				
User	✓	✓	✓	✓
Month	✓	✓	✓	✓
Observations	7,620	6,180	3,288	2,184
R-squared	0.076	0.317	0.081	0.208

Notes: Users are defined as experienced if the number of posted recipes before the observation period is above the sample median. Robust standard errors are clustered at the user level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

on identifying categories that are more likely to be subject to such restrictions. The rationale is that if the platform strategically filters contributions, it is more likely to target categories with a historical "oversupply" of recipes, where new contributions would exhibit lower marginal value, risking redundancy or overwhelming users. This prompts the platform to moderate submissions more strictly to maintain content relevance and user engagement. By contrast, in undersupplied categories, the platform has little incentive to restrict contributions, as each additional recipe could fill content gaps.

To test this mechanism, I calculate the ratio of recipes in each category on Rezeptwelt relative to the corresponding category on Espace Recettes, using data from before the observation period. This ratio captures the relative content saturation at the category level and serves as a proxy for how well-covered a category is on Rezeptwelt compared to Espace Recettes. Based on these ratios, I classify categories into two groups by splitting them at the median: high-share categories (indicating potential "oversupply") and low-share categories (indicating "undersupply"). Table 5 presents the number of recipes per category on Rezeptwelt and Espace Recettes along with their respective ratios. The upper section shows high-share categories and the lower section shows low-share categories.

Figure 7 displays the estimated coefficients for the interaction term *Treat*×*After*, based on regressions

Table 5. Recipes by Category (Before the Observation Period)

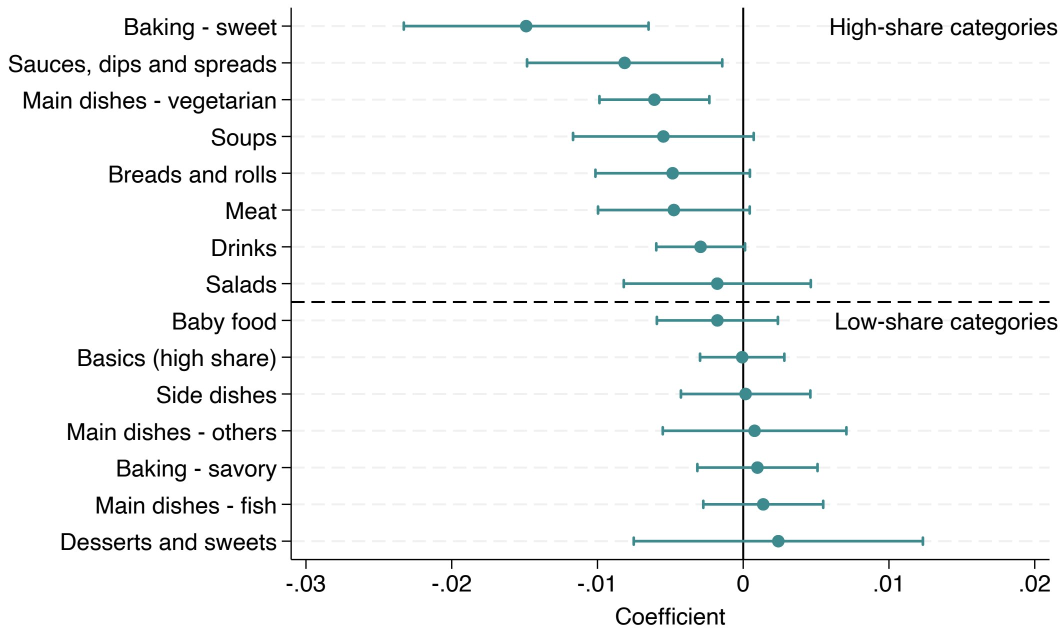
	Recipes *Rezeptwelt*	Recipes *Espace Recettes*	$\frac{Rezeptwelt}{EspaceRecettes}$
High-Share Categories			
Sauces, dips and spreads	10,722	1,320	8.12
Drinks	4,771	725	6.58
Basics	2,476	422	5.87
Main dishes - vegetarian	2,889	530	5.45
Main dishes - meat	4,982	923	5.40
Baking - sweet	16,523	4,230	3.91
Breads and rolls	5,903	1,660	3.56
Soups	4,696	2,174	2.16
Low-Share Categories			
Baking - savory	2,313	1,386	1.67
Main dishes - fish	1,154	802	1.44
Salads	3,075	2,499	1.23
Side dishes	1,560	1,463	1.07
Baby food	597	669	0.89
Main dishes - others	2,791	3,675	0.76
Desserts and sweets	5,195	8,359	0.62

that consider only a user's contributions to a single category at a time. The figure reveals that categories with historically high shares are more likely to experience a decline in published recipes. Specifically, when sorting the estimated coefficients from low to high, all high-share categories—except for basic recipes (e.g., pancake dough)—appear in the upper half of the figure, corresponding to more negative coefficients. In contrast, all low-share categories are found in the lower half, where the coefficients are more positive.

For example, the *Sauces* category has a ratio of 8.12, indicating that Rezeptwelt hosts approximately eight times more recipes in this category compared to Espace Recettes. Consistent with the oversupply hypothesis, *Sauces* exhibits the second-largest decrease in published contributions. Conversely, *Desserts*—the category with the lowest relative number of recipes on Rezeptwelt—shows the largest (albeit statistically insignificant) positive coefficient. Overall, this pattern suggests that the platform's content restrictions disproportionately affect categories that are already well-covered, lending support to the notion of strategic filtering.

To examine this intuition more formally, I constructed two new dependent variables, *NumRecipesHigh* and *NumRecipesLow*, to capture recipe publications in high-share and low-share categories. I then re-estimate

Figure 7. Coefficients by Category



Notes: This figure reports the estimated coefficients (and respective 90 percent confidence intervals with standard errors clustered at the user level) from the regressions of *NumRecipes* on the interaction between treatment and after for each category (Equation (1)) The coefficients are sorted from low to high. Categories above the dashed line represent high-share categories and those below the line represent low-share categories (as defined in Table 5).

Equation (1) using these outcome variables. Table 6 presents the results. Column 1 shows no significant changes in contributions to low-share categories—those with historically lower recipe shares than Espace Recettes. Consistent with this, Columns 2 and 3 indicate that neither inexperienced nor experienced users significantly change their low-share contributions. In contrast, Column 4 reveals a statistically significant decrease in high-share contributions, with this negative effect being driven by inexperienced users (Column 5). These findings suggest that the OBC imposes constraints on specific types of contributions—namely, recipes in already well-covered categories—and that these constraints particularly affect inexperienced users, who may face greater challenges in contributing valuable content to oversupplied categories.

Participation Friction. To examine the issue of participation friction, I test a key assumption about moderator behavior: that as paid employees, they are less likely to work on weekends. If this holds, the pre-moderation policy should disproportionately increase delays on weekends. Consistent with this, Figure 8a shows that the

Table 6. Effect of Pre-Moderation on Recipes in Low- and High-Share Categories

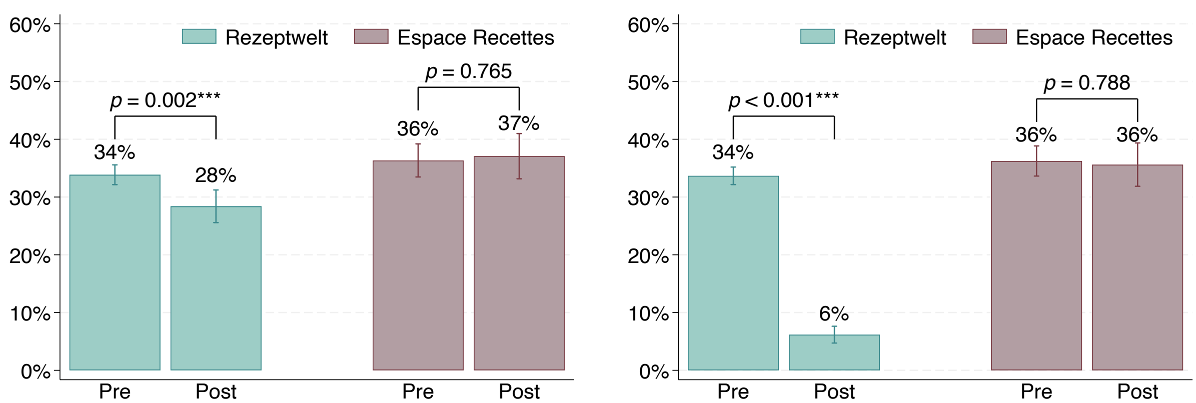
	log(*NumRecipesLow*)			log(*NumRecipesHigh*)		
	All users	Inexperienced	Experienced	All users	Inexperienced	Experienced
Treat×After	−0.001	−0.008	0.009	−0.046***	−0.063***	−0.023
	(0.009)	(0.011)	(0.015)	(0.009)	(0.010)	(0.017)
Fixed Effects						
User	✓	✓	✓	✓	✓	✓
Month	✓	✓	✓	✓	✓	✓
Observations	13,800	7,620	6,180	13,800	7,620	6,180
R-squared	0.277	0.110	0.355	0.205	0.078	0.266

Notes: NumRecipesLow denotes contributions to categories with a low supply of recipes (compared with Espace Recettes). *NumRecipesHigh* denotes contributions to categories with a high supply of recipes (compared with Espace Recettes). Results are consistent when using the CEM sample. Robust standard errors are clustered at the user level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

percentage of recipes published on weekends on Rezeptwelt significantly decreased from 34% to 28% after the policy change ($p = 0.002$), while it remained unchanged on Espace Recettes ($p = 0.765$).[6] This pattern is even more pronounced for recipe updates, as Figure 8b illustrates a sharp drop from 34% to just 6% on weekends on Rezeptwelt ($p < 0.001$), with no change on Espace Recettes ($p = 0.788$). This strong evidence of weekend delays suggests that the pre-moderation process likely introduces participation friction.

Figure 8. Percentage of Recipes Published on Weekends

(a) Percentage of recipes published on weekends **(b) Percentage of recipe updates published on weekends**



Next, I classify users based on the proportion of recipes they posted on weekends prior to the observation period. Users are split into two groups—high and low weekend posters—using the sample median of this

[6]In response to a question from a user on the forum, moderators confirmed that the publication date reflects the approval date rather than the submission date. https://www.rezeptwelt.de/comment/773878#comment-773878.

proportion. I then create a dummy variable, *Weekend*, which equals one if a user's share of weekend recipes in the pre-observation period is above the sample median, and zero otherwise. To test for heterogeneity, I interact *Treat*×*After* with this dummy. I also replace the month fixed effects with a set of weekend-by-month fixed effects to account for time-varying unobserved heterogeneity between weekend and non-weekend users. I then re-estimate Equation (1) as follows:

$$y_{it} = \beta_0 + \beta_1 Treat_i \times After_t + \beta_2 Treat_i \times After_t \times Weekend_i + \gamma_i + Weekend_i \times \tau_t + \varepsilon_{it}, \tag{3}$$

where *Treat*×*After*×*Weekend* represents the additional effect of the policy on high-weekend posters relative to low-weekend posters. In this specification, *After*×*Weekend* is not estimated as it is collinear with the interaction between the month fixed effects and *Weeekend*.

Table 7 presents the results of this analysis. Column 1 shows that the negative triple interaction is marginally significant, indicating that high-weekend users on Rezeptwelt published fewer recipes (relative to low-weekend users) following the policy change. Column 2 reveals that this effect is not driven by inexperienced users; while these users did reduce their output after the introduction of pre-moderation, the reduction was not significantly different for weekend users compared to non-weekend users. Column 3 reveals a significant and negative response to the policy from experienced weekend users, while experienced non-weekend users exhibited a slight, non-significant increase in contributions. These findings suggest that the overall negative baseline effect (see Table 3) is primarily driven by users who were more exposed to the policy—specifically, those who posted more frequently on weekends before the observation period. Notably, the results emphasize that experienced users, in particular, appear to be dissatisfied with the increased friction caused by the policy, such as the longer wait times, and were substantially more likely to decrease their contributions as a result.

5.1.5 Robustness Checks

Placebo Test. Following Frydman and Wang (2020), I conducted a multiple placebo test analysis. The premise is that if the observed changes in the number of published recipes around the pre-moderation policy's

Table 7. Heterogeneous Effects by Weekend and Non-Weekend Posters

	log(*NumRecipes*)		
	All users (1)	Inexperienced (2)	Experienced (3)
Treat×*After*	−0.021	−0.058***	0.045
	(0.019)	(0.021)	(0.032)
Treat×*After*×*Weekend*	−0.045*	−0.019	−0.099***
	(0.025)	(0.031)	(0.029)
Fixed Effects			
User	✓	✓	✓
Weekend×Month	✓	✓	✓
Observations	13,800	7,620	6,180
R-squared	0.240	0.077	0.319

Notes: This table shows the effect of the pre-moderation policy on recipes. *Weekend* is a dummy variable that equals one if the average percentage of recipes posted on weekends (measured as recipes posted on weekends over all recipes) before the observation period is above the sample median. Results are consistent when using the CEM sample. Robust standard errors are clustered at the user level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

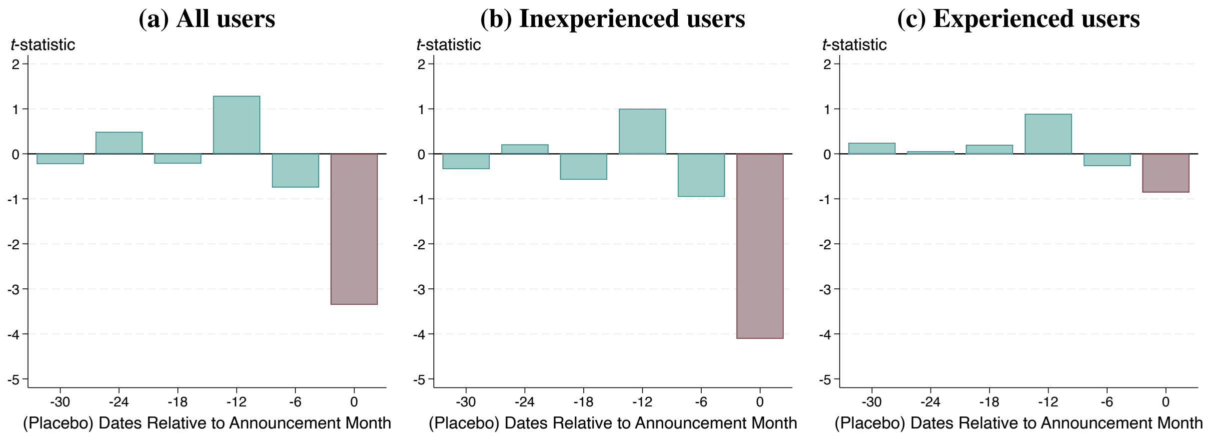
implementation were due to other structural factors, similar changes should appear at earlier dates. For this analysis, I re-estimated Equation 1 after recoding the announcement date. I defined multiple placebo dates by rolling backward in six-month increments from the actual announcement, extending up to 30 months prior. For each of these placebo dates, I repeated the estimations using a six-month window before and after.[7] I then recorded the t-statistic for the *Treat*×*After* interaction from each regression, which are plotted in Figure 9.

As anticipated, Figure 9a shows a distinct negative spike in the number of published recipes only during the actual policy announcement period. This was the sole coefficient to achieve statistical significance at the 5% level, strongly supporting the causal interpretation of the pre-moderation policy. I then repeated this analysis separately for inexperienced and experienced users (splitting them at the sample median). The results remained consistent: Inexperienced users exhibited a pronounced drop solely at the actual announcement date (Figure 9b), while none of the placebo tests—including the actual announcement—yielded significant results for experienced users (Figure 9c). Overall, the findings from these placebo tests indicate that the false

[7]To maintain consistency with the main analysis, the same sample selection criteria were applied, meaning I used the same type of user cohort as in the original timeframe, not necessarily the exact same users.

intervention dates lead to statistically and economically insignificant effects on the number of published recipes, reinforcing the reliability of the conclusions regarding the effect of the pre-moderation policy. This also rules out that the results are driven by seasonality, which would imply different trajectories between the German and French OBCs in the second half of the year.

Figure 9. Multiple Placebo Test Analysis



Notes: This figure presents results of the multiple placebo test analysis using the regression specification in Equation (1) (including category-by-month fixed effects). I define five placebo dates by rolling backwards in six-month increments, then conduct the DID test with the six months before and after each date, and compute the t-statistic on the $Treat \times After$ interaction. The red bar denotes the date of the actual announcement.

Newly Recruited Users. I extend the main analysis to examine the most extreme case of inexperience: newly recruited users with no prior experience. This allows us to test whether the policy's discouraging effect on inexperienced contributors extends to users who face the highest barriers to initial participation. Following Nagaraj and Piezunka (2024), I identify users who make their first-ever contribution in a given month. While the core DID framework remains consistent with the main analysis, the unit of observation now shifts to a group-level outcome: the total number of contributions made by all newly recruited users in each month. To formally estimate this effect, I use the following category-platform-month specification:

$$y_{cpt} = \beta_0 + \beta_1 Treat_p \times After_t + \gamma_c + \delta_p + \tau_t + \varepsilon_{cpt}. \tag{4}$$

Here, the subscript c denotes a generic category (e.g., "Baking - sweet") that exists across both platforms, p denotes the platform (Rezeptwelt or Espace Recettes), and t denotes the month. The dependent variable y_{cpt} is the total number of recipes from new recruits in category c on platform p in month t. $Treat_p$ is a binary

Table 8. Effect of Pre-Moderation on Recipes From Existing and Newly Recruited Users

	log(*NumRecipes*)		
	All users (1)	Existing users (2)	Newly recruited users (3)
Treat×After	−0.573*** (0.050)	−0.433*** (0.087)	−0.625*** (0.084)
Fixed Effects			
Platform	✓	✓	✓
Category	✓	✓	✓
Month	✓	✓	✓
Observations	360	360	360
R-squared	0.658	0.631	0.578

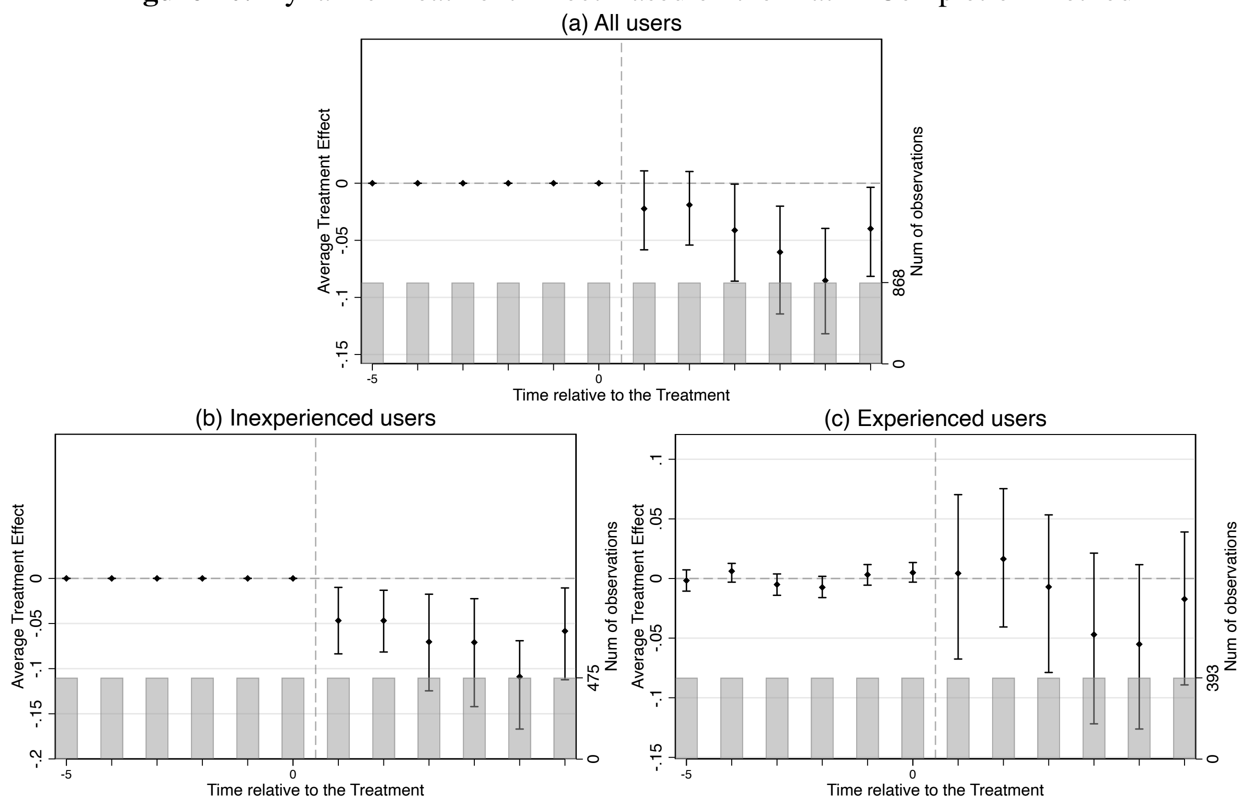
Notes: Robust standard errors are clustered at the category level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

variable equal to 1 if platform p is Rezeptwelt (the treated platform) and 0 if platform p is Espace Recettes (the control platform). $After_t$ is a binary variable equal to 1 for months after the policy announcement and 0 otherwise. γ_c are generic category fixed effects, capturing time-invariant unobservables common to each category across platforms. δ_p are platform fixed effects and τ_t are time fixed effects. The coefficient β_1 estimates the impact of the pre-moderation policy on newly recruited contributions.

Table 8 presents the results. Column 1 reports the total number of recipes published by all users and confirms the earlier finding of a substantial 44% decline following the introduction of pre-moderation. Column 2 restricts the sample to existing contributors, revealing a 36% decrease. Column 3 focuses on recipes from newly recruited users—defined as those who posted a recipe for the first time in a given month—and shows an even sharper decline of 46%. Table A2 in Appendix A replicates these results using relative time models, confirming that all pretreatment coefficients are statistically insignificant. Overall, the findings support a causal effect of pre-moderation, with the impact being most pronounced for newly recruited users, suggesting that the policy creates a particularly strong barrier to initial participation.

Alternative Estimators. I demonstrate that the results are robust to alternative estimation strategies. First, re-estimating the model with a Poisson regression, which is appropriate for count data, yields results consistent with the main analysis (Table A3 in Appendix A). Second, I apply the matrix completion (MC) method of Athey et al. (2021) to estimate the average treatment effect on the treated (ATT). The resulting dynamic treatment effects (Figure 10) support the parallel trends assumption, as pre-treatment estimates are close to zero across all groups. The post-treatment effects are negative and significant for all users, with the impact primarily concentrated on inexperienced users. In contrast, post-treatment estimates for experienced users are small and not statistically significant. Overall, these findings closely align with the main results and reinforce the conclusion that pre-moderation primarily deters contributions from inexperienced users.

Figure 10. Dynamic Treatment Effect Based on the Matrix Completion Method



Notes: This figure was produced using the "fect" package in Stata. Error bars denote bootstrapped 95% confidence intervals. Results are consistent when using the CEM sample.

Alternative Measures and Thresholds of User Experience. I also tested the robustness of the results using alternative measures and thresholds for user experience. Columns (1) and (2) of Table A4 in Appendix A show that the results remain consistent when the sample is split by tenure—measured as the number of months since a user's first recipe post—rather than by total recipe volume. Similarly, Columns (3) and

(4) confirm the findings when focusing exclusively on users in the lower and upper terciles of experience, classified as inexperienced and experienced users, respectively.

Alternative Time Window. One potential concern is that the announcement date may not align precisely with the actual onset of the pre-moderation policy (recall that the announcement was posted on July 3, while the policy was scheduled to begin in the "second half of July"). To address this, I replicated the analysis while omitting the first month before and after the announcement, ensuring a balanced pre- and post-period. The results, shown in Table A5 in Appendix A, are highly consistent with the main findings.

5.2 Effects of Pre-Moderation on Recipe Novelty (H3) and Number of Likes (H4)

My next focus is the effect of the pre-moderation policy on the novelty of contributions and the number of likes they receive. The unit of analysis in this section is the individual recipe (Wang & Greenwood, 2025). The dependent variables are: (i) *TitleNovelty*, measured as $1 - MaxSim$, where *MaxSim* is the maximum cosine similarity between a recipe's title and all titles from recipes previously posted in the same category up to the previous day; and (ii) log(*NumLikes*), the log-transformed number of likes received. Both dependent variables are measured for a given recipe j, posted by user i in month t.

I measure recipe title novelty using the SBERT model all-MiniLM-L6-v2, which efficiently generates semantically meaningful embeddings for short texts (e.g., Quinn & Gutt, 2025; Teutloff et al., 2025). SBERT produces high-quality sentence embeddings that support cosine similarity comparisons to assess novelty, leveraging transfer learning to deliver contextually rich vectors without requiring extensive in-domain retraining. Although all-MiniLM-L6-v2 is not fine-tuned for German or French, model architecture often matters more than training language for multilingual performance, and large, high-quality English corpora can even outperform smaller, machine-translated datasets (Ciancone et al., 2024). In my DID design, any remaining language-specific bias is also constant across groups and thus accounted for. As a robustness check, I use the multilingual paraphrase-multilingual-mpnet-base-v2, which outperforms other multilingual SBERT models (Blombach et al., 2025).

For each recipe, I create a rolling, category-specific reference set containing all prior recipes in the same category. I embed the recipe title—"the title usually encapsulates the essence of the recipe" (Hu et al., 2024, p. 1070)—to capture conceptual novelty without noise from common ingredients or standard steps. The novelty score, *TitleNovelty*, is defined as $1 - MaxSim$, where *MaxSim* is the highest cosine similarity with any recipe in the reference set. Using the maximum rather than the average similarity reflects the fact that novelty, from a user's perspective, falls sharply if even one near-duplicate already exists. This approach isolates the presence of close substitutes that most directly reduce perceived originality.

5.2.1 Empirical Framework

Similar to my analysis for the effect of pre-moderation on the number of recipes, I use a DID. The independent variable of interest is the interaction of *Treat*×*After*. Formally,

$$y_{ijt} = \beta_0 + \beta_1 Treat_i \times After_t + \gamma_i + \tau_t + X_{it} + \varepsilon_{ijt}. \tag{5}$$

where γ_i denotes individual fixed effects and τ_t captures time fixed effects, including month and day-of-week indicators.[8] *Treat* and *After* are omitted from the specification, as they are fully absorbed by the fixed effects. X_{it} represents a vector of time-varying controls, including log(*PreCumRecipes*), which measures the cumulative number of recipes from user i before month t to account for the influence of past activities, and log(*Tenure*), the number of months since user i's first recipe post as of month t.

5.3 Regression Results

Table 9 presents the results of the novelty analysis. Column 1 reports the baseline findings, showing a statistically significant decrease in recipe title novelty following the pre-moderation policy. Column 2 adds a control for the log-transformed recipe title length, log(*TitleLength*), which is positively associated with novelty. Including this control slightly reduces the DID coefficient, but it remains highly significant, ruling out the possibility that changes in novelty are driven by editorial adjustments related to title length enforced by the policy. Columns 3 and 4 replicate the baseline results using the CEM sample, with coefficients slightly

[8] I do not include day fixed effects, as the data is too sparse at the daily level—many users contribute infrequently, leading to limited within-user variation on a day-to-day basis.

larger in magnitude. These findings consistently support H3b, confirming that the novelty of published contributions declines and demonstrating robustness across specifications and samples. Results from the relative time models (Columns 1 and 2 of Table A6 in Appendix A) show that all pretreatment coefficients are statistically insignificant, with all but one nonnegative. Most posttreatment coefficients are negative (8 out of 12), and two reach statistical significance. Finally, Table A7 in Appendix A confirms that the results are consistent when using the paraphrase-multilingual-mpnet-base-v2 model.

Next, I examine how the policy affected user engagement, measured by the number of likes (H4). Column 1 of Table 10 shows no significant change in likes in the baseline model, while the matched sample in Column 2 indicates a marginally significant increase. The relative time models (Columns 3 and 4 of Table A6 in Appendix A) also reveal no clear post-treatment effect, with only one marginally significant pre-treatment coefficient appearing several months before the policy change in each model. As a robustness check, I analyze the number of comments (Columns 3 and 4 of Table 10); the coefficients are positive but not statistically significant. Overall, these findings suggest that the pre-moderation policy had no strong effect on user engagement, providing no consistent support for either H4a or H4b.[9]

6 IMPLICATIONS AND CONCLUSION

Firms operating OBCs face a central challenge: how to align user contributions with brand values while maintaining a steady stream of fresh, engaging content. This paper presents one of the first empirical examinations of how a pre-moderation policy influences user behavior in a large recipe-sharing community. Leveraging a natural experiment, I find that the policy reduced the number of published contributions (H1), with the decline driven primarily by inexperienced users (H2). The pattern in published content suggests two underlying mechanisms: inexperienced users appear to publish fewer recipes in already well-covered categories, indicating that the policy constrains content in oversaturated areas. Meanwhile, experienced

[9] On July 5, 2018, Rezeptwelt implemented a new policy requiring comments with star ratings, a restriction not present on Espace Recettes. I therefore focus on the number of likes as the primary metric. I note that the new rating policy could have induced a spillover effect on likes, likely making the observed effects a conservative estimate.

Table 9. Effect of Pre-Moderation on Novelty of Recipe Titles

	TitleNovelty			
	All users (1)	All users (2)	All users + CEM (3)	All users + CEM (4)
Treat×*After*	−0.049***	−0.034***	−0.062**	−0.063**
	(0.013)	(0.013)	(0.027)	(0.025)
log(*TitleLength*)		0.059***		0.056***
		(0.009)		(0.015)
Fixed Effects				
User	✓	✓	✓	✓
Month	✓	✓	✓	✓
Day-of-week	✓	✓	✓	✓
Observations	2,744	2,744	912	912
R-squared	0.335	0.366	0.391	0.415

Notes: Columns 1 and 3 show the baseline DID. Columns 2 and 4 add log(*TitleLength*). Control variables include log(*PreCumRecipes*) and log(*Tenure*). Robust standard errors are clustered at the user level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 10. Effects of Pre-Moderation on Number of Likes and Comments

	log(*NumLikes*)		log(*NumComments*)	
	All users (1)	All users + CEM (2)	All users (3)	All users + CEM (4)
Treat×*After*	−0.012	0.252*	0.112	0.183
	(0.076)	(0.128)	(0.090)	(0.189)
Fixed Effects				
User	✓	✓	✓	✓
Month	✓	✓	✓	✓
Day-of-week	✓	✓	✓	✓
Observations	2,744	912	2,744	912
R-squared	0.628	0.616	0.405	0.431

Notes: Control variables include log(*PreCumRecipes*) and log(*Tenure*). Robust standard errors are clustered at the user level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

users—especially those facing a higher likelihood of delay—are unintentionally deterred by the friction of the moderation process, leading them to contribute less. The policy also lowered the novelty of published recipes (H3b), suggesting a narrower acceptable solution space. However, it had no consistent effect on user engagement, providing no support for H4a or H4b.

This study contributes to the literature on governance in OBCs (Bapna et al., 2019; Liang et al., 2024; Safadi et al., 2025). Prior research has examined numerous drivers of user behavior in these communities, including the introduction of likes (Liang et al., 2024) and reputation systems (Hanson et al., 2019), yet the consequences of a strict pre-moderation policy have received little empirical attention. My results—showing a 4.4% drop in published contributions alongside reduced novelty—underscore the risks of "over-formalizing" the informal (Safadi et al., 2025), that is, replacing community-based governance with more formal firm-controlled mechanisms. Community members themselves expressed dissatisfaction, noting that they had previously engaged in peer-checking recipes and describing the policy as "completely pointless." Such perceptions are meaningful because they suggest a homogenization of content and potentially reduced creative freedom. These findings also extend qualitative work on Reddit, where "tough rule enforcement" was perceived as creating "an uncomfortably strict environment that limited creativity" (Kiene et al., 2016, p. 1155). Here, I generalize that insight to an OBC in which a vibrant ecosystem is integral to the product's appeal—implying that diminished creativity could ultimately undermine, rather than enhance, the brand.

These insights have broader implications for governance and oversight that extend beyond online communities into other creative and intellectual domains. Similar "chilling effects" may arise in academia—for example, in the "top five" economics journals, where declining acceptance rates and lengthier review processes (e.g., involving multiple referees) have been argued to come at the expense of creativity (Heckman & Moktan, 2020). The parallel is instructive: as gatekeepers increase scrutiny over what gets published, contributions from the periphery are more likely to be deterred, potentially introducing biases into the surviving content. In the context of an OBC, such a narrowing of the solution space can erode the creative appeal of recipes shared through official channels. This highlights a delicate balance for firms: maintaining appropriate control without stifling the very creativity that underpins the value of their communities.

A further contribution lies in analyzing user heterogeneity and its influence on UGC (e.g., Wang & Greenwood, 2025). Results show that responses to the pre-moderation policy vary markedly with experience: experienced users are better able to sustain their output, whereas inexperienced users are more likely to reduce their contributions. Why are newcomers more easily deterred by the hurdles of pre-moderation? Prior research finds that newcomers in online communities are particularly sensitive to early experiences and barriers to contribution (Aaltonen & Wattal, 2025; Pethig et al., 2025). Kraut et al. (2012), for example, emphasize the challenges of socializing newcomers and the risk of disengagement when they encounter friction or feel unwelcome. Thus, a strict moderation policy may send a discouraging signal: delays or rejections can be interpreted as evidence that contributions are not valued or that participation is too burdensome.

By contrast, experienced contributors often have deeper commitment to the community and stronger reputations or identities tied to their contributions. Many continued to post recipes despite the new policy, suggesting resilience built on loyalty or intrinsic motivation. Yet even in this group, those facing longer approval delays or greater friction eventually reduced their output. This highlights a key insight: timely visibility and feedback are powerful motivators for contributors. When pre-moderation introduced lag and uncertainty, it weakened the immediate gratification of seeing content published and appreciated. This interpretation aligns with motivation theories in user-generated content: contributors are often driven by rapid feedback, recognition, and the satisfaction of helping others (Forderer & Burtch, 2024; Wasko & Faraj, 2005). Removing this instant feedback loop appears to have frustrated even the community's most committed members, diminishing their willingness to contribute.

The results contribute to the broader understanding of content moderation effects (Cao et al., 2024; Jiang et al., 2023; Mudambi et al., 2024). Most empirical research has examined post-moderation systems, where content is removed or altered after publication (Srinivasan et al., 2019, e.g.,). By contrast, only a few studies investigate pre-moderation (or "post-approval"), where content must be approved before becoming visible. Ribeiro et al. (2022) find that such a policy can increase engagement with the remaining posts, interpreting this as a quality-filtering effect. In my setting, however, I find no consistent increase in engagement following the introduction of pre-moderation. This divergence underscores that the effects of pre-moderation are highly context-dependent (Ridings & Gefen, 2004). Whereas Ribeiro et al. (2022) studied Facebook groups—fast-paced conversational spaces where filtering may enhance perceived quality—Rezeptwelt also functions as a public knowledge repository, where value hinges on a steady flow of new, relevant content. Reducing this flow may lower the community's perceived value, particularly during periods with no new publications. This points to an important contingency: pre-moderation may improve engagement in highly interactive, fast-paced communities, but in knowledge-oriented communities, it risks undermining activity by slowing the pace of visible contributions.

For practitioners—community managers, platform designers, and firms running OBCs—the findings of this study offer several important lessons. Foremost, when considering the introduction of stricter moderation policies like pre-moderation, it is crucial to weigh the benefits of content quality control against the potential costs in user engagement. My results show that a blanket pre-moderation policy, applied universally, can reduce community activity especially among newer contributors. Community managers should therefore ask: Is the quality problem severe enough to justify potentially losing contributions? When the answer is yes—for example, if unchecked content could cause legal issues or serious reputational harm—managers and moderators should proceed with caution, showing empathy toward moderated users and considering the situation from their perspective (Feng et al., 2024).

One practical approach is to implement targeted or conditional pre-moderation rather than a one-size-fits-all policy. The Rezeptwelt case revealed that inexperienced users were most negatively affected, whereas experienced users were relatively resilient (unless delays occurred). This suggests a possible compromise: apply pre-moderation selectively to content from less experienced members or first-time posters. Indeed, as noted earlier, some OBCs like SAP's Community Network have adopted this strategy—new users are screened until proven trustworthy. Such a graduated system could maintain quality for content by inexperienced users (who are more prone to mistakes) while preserving immediacy for experienced users. This could socialize newcomers into the OBC (through feedback on initial submissions) without alienating loyal contributors.

There are several limitations. First, this study focuses on a specific type of community—a branded recipe—sharing platform in a particular domain (cooking) and cultural context (predominantly German-speaking users on Rezeptwelt, with a French-speaking comparison community). User behavior in this setting may not generalize to other types of communities, such as social networking, open-source software, or political discussion forums, where the content, goals, and stakes differ (Ridings & Gefen, 2004). Second, I examine user behavior for six months after the policy change, capturing immediate and medium-term effects. The long-term trajectory remains unclear: communities may rebound as members adapt to the new rules, or further decline if friction continues to deter contributions. Future work could investigate whether community composition shifts over time, with highly self-motivated contributors persisting while casual participants disengage. Third, I can only observe *publications*—not submissions—which limits my ability to test the proposed mechanisms directly. This also prevents analysis of the content flagged or rejected by moderators (e.g., number and reasons for rejections) and of users' experiences with the pre-moderation process. Surveying members about their satisfaction and perceptions of fairness could complement engagement metrics, while qualitative approaches—such as interviews or forum content analysis—could reveal how contributors perceive content novelty, governance fairness, and their own role in the community.

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APPENDIX A. SUPPLEMENTARY TABLES AND FIGURES

Table A1. Effect of Pre-Moderation on Recipes (Relative Time Models)

	log(*NumRecipes*)					
	All users (1)	Inexperienced (2)	Experienced (3)	All users + CEM (4)	Inexp. + CEM (5)	Exp. + CEM (6)
$Treat \times Month_{-6}$	−0.026	−0.012	−0.045	−0.013	−0.011	−0.016
	(0.033)	(0.035)	(0.060)	(0.041)	(0.047)	(0.074)
$Treat \times Month_{-5}$	0.006	0.002	0.013	−0.005	−0.019	0.015
	(0.027)	(0.032)	(0.047)	(0.035)	(0.044)	(0.060)
$Treat \times Month_{-4}$	−0.040	−0.009	−0.082	−0.036	0.021	−0.123*
	(0.030)	(0.031)	(0.057)	(0.038)	(0.045)	(0.069)
$Treat \times Month_{-3}$	−0.039	−0.005	−0.081*	−0.033	−0.001	−0.081
	(0.025)	(0.029)	(0.044)	(0.035)	(0.041)	(0.061)
$Treat \times Month_{-2}$	0.002	0.009	−0.007	−0.022	0.006	−0.065
	(0.025)	(0.027)	(0.045)	(0.032)	(0.038)	(0.057)
$Treat \times Month_{-1}$	Baseline	Baseline	Baseline	Baseline	Baseline	Baseline
$Treat \times Month_{0}$	−0.038*	−0.049**	−0.025	−0.039	−0.091***	0.040
	(0.023)	(0.025)	(0.042)	(0.031)	(0.031)	(0.060)
$Treat \times Month_{1}$	−0.035	−0.049**	−0.017	−0.030	−0.043	−0.011
	(0.023)	(0.024)	(0.044)	(0.031)	(0.034)	(0.058)
$Treat \times Month_{2}$	−0.057**	−0.073**	−0.036	−0.079**	−0.085**	−0.071
	(0.024)	(0.030)	(0.039)	(0.032)	(0.040)	(0.054)
$Treat \times Month_{3}$	−0.076***	−0.073**	−0.079*	−0.084**	−0.071*	−0.103*
	(0.026)	(0.033)	(0.043)	(0.033)	(0.043)	(0.052)
$Treat \times Month_{4}$	−0.101***	−0.111***	−0.087*	−0.117***	−0.116***	−0.118**
	(0.025)	(0.028)	(0.045)	(0.031)	(0.037)	(0.054)
$Treat \times Month_{5}$	−0.056**	−0.061**	−0.047	−0.059*	−0.070*	−0.042
	(0.024)	(0.027)	(0.043)	(0.030)	(0.036)	(0.054)
Fixed Effects						
User	✓	✓	✓	✓	✓	✓
Month	✓	✓	✓	✓	✓	✓
Observations	13,800	7,620	6,180	5,472	3,288	2,184
R-squared	0.240	0.077	0.319	0.153	0.082	0.215

Notes: Users are defined as experienced if the number of posted recipes before the observation period is above the sample median. Robust standard errors are clustered at the user level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A2. Effect of Pre-Moderation on Recipes From Existing and Newly Recruited Users (Relative Time Models)

	log(*NumRecipes*)		
	All users (1)	Existing users (2)	Newly recruited users (3)
$Treat \times Month_{-6}$	−0.143	0.077	−0.398
	(0.207)	(0.201)	(0.236)
$Treat \times Month_{-5}$	0.073	0.220	−0.161
	(0.236)	(0.229)	(0.197)
$Treat \times Month_{-4}$	−0.319	−0.292	−0.207
	(0.212)	(0.199)	(0.265)
$Treat \times Month_{-3}$	−0.105	0.028	−0.276
	(0.224)	(0.203)	(0.279)
$Treat \times Month_{-2}$	0.007	0.131	−0.228
	(0.188)	(0.235)	(0.208)
$Treat \times Month_{-1}$	Baseline	Baseline	Baseline
$Treat \times Month_{0}$	−0.257	0.048	−0.597***
	(0.232)	(0.299)	(0.174)
$Treat \times Month_{1}$	−0.216	−0.152	−0.418
	(0.245)	(0.203)	(0.258)
$Treat \times Month_{2}$	−0.444**	−0.341**	−0.373
	(0.156)	(0.144)	(0.245)
$Treat \times Month_{3}$	−0.688***	−0.428*	−0.992***
	(0.193)	(0.204)	(0.253)
$Treat \times Month_{4}$	−1.345***	−0.947***	−1.547***
	(0.218)	(0.195)	(0.233)
$Treat \times Month_{5}$	−0.974***	−0.617**	−1.092***
	(0.259)	(0.275)	(0.200)
Fixed Effects			
Platform	✓	✓	✓
Category	✓	✓	✓
Month	✓	✓	✓
Observations	360	360	360
R-squared	0.682	0.650	0.609

Notes: Robust standard errors are clustered at the user level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A3. Alternative Estimator: Poisson Regression Estimates

	NumRecipes		
	All users	Inexperienced	Experienced
	(1)	(2)	(3)
Treat×After	−0.527***	−1.121***	−0.228
	(0.154)	(0.214)	(0.215)
Fixed Effects			
User	✓	✓	✓
Month	✓	✓	✓
Observations	13,800	7,620	6,180

Notes: Robust standard errors are clustered at the user level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A4. Alternative Experience Measure and Alternative Experience Threshold

	log(*NumRecipes*)			
	Alternative Experience Measure		Alternative Experience Threshold	
	Inexperienced	Experienced	Lower Tercile	Upper Tercile
	(1)	(2)	(3)	(4)
Treat×After	−0.073***	−0.017	−0.050***	−0.015
	(0.019)	(0.017)	(0.016)	(0.027)
Fixed Effects				
User	✓	✓	✓	✓
Month	✓	✓	✓	✓
Observations	6,912	6,888	5,244	4,212
R-squared	0.139	0.325	0.068	0.358

Notes: Robust standard errors are clustered at the user level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A5. Alternative Time Window: Omitting the First Month Before and After the Announcement

	log(*NumRecipes*)		
	All users	Inexperienced	Experienced
	(1)	(2)	(3)
Treat×After	−0.046***	−0.071***	−0.013
	(0.015)	(0.018)	(0.023)
Fixed Effects			
User	✓	✓	✓
Month	✓	✓	✓
Observations	11,500	6,350	5,150
R-squared	0.246	0.086	0.327

Notes: Robust standard errors are clustered at the user level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A6. Effect of Pre-Moderation on Title Novelty and Number of Likes (Relative Time Models)

	TitleNovelty		log(*NumLikes*)	
	All users (1)	All users + CEM (2)	All users (3)	All users + CEM (4)
$Treat \times Month_{-6}$	0.026	0.065	−0.078	−0.067
	(0.028)	(0.061)	(0.158)	(0.342)
$Treat \times Month_{-5}$	−0.013	0.049	0.241	0.447*
	(0.026)	(0.057)	(0.150)	(0.252)
$Treat \times Month_{-4}$	0.025	0.090	0.235*	0.323
	(0.028)	(0.055)	(0.137)	(0.275)
$Treat \times Month_{-3}$	0.031	0.089	0.026	0.289
	(0.039)	(0.088)	(0.139)	(0.310)
$Treat \times Month_{-2}$	0.040	0.068	−0.009	−0.077
	(0.031)	(0.079)	(0.151)	(0.318)
$Treat \times Month_{-1}$	Baseline	Baseline	Baseline	Baseline
$Treat \times Month_{0}$	−0.018	−0.061	0.015	0.409
	(0.033)	(0.070)	(0.159)	(0.306)
$Treat \times Month_{1}$	−0.021	0.037	0.136	0.324
	(0.028)	(0.062)	(0.176)	(0.273)
$Treat \times Month_{2}$	−0.039	−0.015	0.271	0.456
	(0.031)	(0.057)	(0.175)	(0.370)
$Treat \times Month_{3}$	0.004	0.095	−0.158	0.266
	(0.034)	(0.093)	(0.135)	(0.331)
$Treat \times Month_{4}$	−0.071**	−0.053	0.131	0.763
	(0.035)	(0.104)	(0.147)	(0.578)
$Treat \times Month_{5}$	−0.053*	0.000	0.020	0.479
	(0.031)	(0.077)	(0.175)	(0.376)
Fixed Effects				
User	✓	✓	✓	✓
Month	✓	✓	✓	✓
Day-of-week	✓	✓	✓	✓
Observations	2,744	912	2,744	912
R-squared	0.339	0.405	0.631	0.624

Notes: Control variables include log(*PreCumRecipes*) and log(*Tenure*). Robust standard errors are clustered at the user level. Robust standard errors are clustered at the user level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A7. Effect of Pre-Moderation on Novelty of Recipe Titles (Multilingual Model)

	TitleNovelty			
	All users (1)	All users (2)	All users + CEM (3)	All users + CEM (4)
Treat×*After*	−0.026*** (0.007)	−0.020*** (0.007)	−0.034** (0.014)	−0.034** (0.014)
log(*TitleLength*)		0.023*** (0.005)		0.021*** (0.007)
Fixed Effects				
User	✓	✓	✓	✓
Month	✓	✓	✓	✓
Day-of-week	✓	✓	✓	✓
Observations	2,744	2,744	912	912
R-squared	0.329	0.342	0.412	0.421

Notes: Columns 1 and 3 show the baseline DID. Columns 2 and 4 add log(*TitleLength*). Control variables include log(*PreCumRecipes*) and log(*Tenure*). Robust standard errors are clustered at the user level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.