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Perceived Personalisation and Online Political Expression: Reassessing Survey Evidence from Delhi, India

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ABSTRACT :

Political microtargeting research frequently treats perceived message relevance, data-driven targeting and civic engagement as connected processes. Yet survey measures cannot establish the advertising technology that delivered a message, and favourable attitudes towards tailored communication need not correspond to participatory behaviour. This article investigates these distinctions using 500 responses to an online voluntary-response survey conducted in Delhi, India. Three prereflective questions guide the analysis: whether reported exposure to customised political messages differs by age, whether willingness to engage with targeted messages relates to sharing political posts, and whether sharing and commenting covary. A five-by-five contingency analysis showed no detectable age association with perceived personalised-message exposure, $\chi^2(16) = 13.123$, $p = 0.664$, Cramér's $V = 0.081$. Targeted-message receptiveness was not significantly associated with sharing ($r = -0.056$, $p = 0.208$), whereas sharing and commenting were moderately associated ($r = 0.484$, $p < .001$). The analysis challenges an assumed progression from personalised exposure to engagement. Its contribution is a measurement-sensitive account of how targeting perceptions, expressed receptiveness and self-reported participatory practices can diverge. The findings concern the surveyed respondents and cannot establish causal persuasion or population-level electoral effects.

Keywords: Political microtargeting; perceived personalisation; political communication; online participation; survey measurement; India

1. Introduction

Political campaigning has moved into environments where citizens encounter political information alongside entertainment, interpersonal conversation, commercial promotion and algorithmically ranked content. Campaigns can purchase advertising directed at selected audiences, but users may also encounter political material through recommendations, group messages and ordinary sharing. The distinction matters because a respondent's impression that a message was personally tailored is not independent evidence that a campaign selected that individual using a particular dataset. Research that overlooks this difference risks giving a technical interpretation to a subjective survey answer. The present article examines what respondents say about personalised political communication and how these responses relate to selected forms of online engagement. It does not attempt to reconstruct advertising delivery systems or infer the intentions of political parties.

The question is relevant beyond debates about the effectiveness of campaign advertising. Political communication scholarship has often connected digital media to political learning, expression and participation, while acknowledging variation across platforms, audiences and forms of engagement. Meta-analytic research suggests that associations between social media and participation are heterogeneous rather than uniform (Boulianne, 2015, 2020). Studies of political microtargeting likewise offer evidence of different psychological responses to message fit, disclosure and perceived manipulation (Kruikemeier et al., 2016; Hirsch et al., 2024). Such findings do not imply that a positive response to targeted content necessarily produces a visible political act. An individual may find a message relevant and still decline to share it because of privacy concerns, social disagreement or lack of interest in public self-expression.

India provides an important setting for studying this question because political communication operates through several languages, overlapping media infrastructures and diverse networks of interpersonal and platform-based discussion. Yet contextual importance should not be confused with representativeness. The data analysed here come from 500 online voluntary respondents in Delhi. The study can describe relationships among their recorded answers, not the prevalence of microtargeting across Indian elections. This narrower scope permits a sharper contribution: rather than asserting that targeted communication mobilises voters, the analysis asks whether three commonly conflated measurements actually move together in this sample. The argument is particularly important when researchers rely on cross-sectional Likert-type items and interpret statistical significance as evidence of campaign effectiveness.

This paper has three contributions. Conceptually, it distinguishes subjective recognition of personalisation from verified targeting practice. Empirically, it evaluates the age distribution of perceived exposure and the relationship between message receptiveness and sharing, using the original encoded responses rather than previously circulated summary figures. Methodologically, it demonstrates why separate engagement indicators should be reported before being merged into a composite participation measure. The analysis also documents a consequential null finding. Null findings can be informative

when a popular theoretical narrative would otherwise imply an association, provided the authors do not confuse failure to reject a null hypothesis with proof of equivalence. The article therefore interprets magnitudes, uncertainty and the limits of the sampling design together.

2. Conceptual background: personalisation, targeting and relevance

Political personalisation has several meanings that should be kept analytically distinct. A campaign may personalise its message by mentioning a candidate, adapting language to an audience or tailoring policy emphasis to a group. Platform advertising may use demographic, geographic, behavioural or inferred attributes to decide which users see particular material. Recipients, however, observe the content presented to them, not necessarily the underlying selection process. A statement such as "I often get political communications customized to my individual preferences" measures perceived experience. It cannot determine whether a message was generated from an individual profile, delivered to a broad demographic segment, or merely felt relevant because it discussed a salient issue. This distinction is central to the validity of any survey-based claim about microtargeting.

Experimental research demonstrates why the mechanisms of targeting cannot be assumed. Kruikemeier et al. (2016) found that personalised political advertising could activate persuasion knowledge under particular disclosure conditions, with implications for willingness to engage in electronic word of mouth. Zarouali et al. (2022) tested personality-tailored political advertisements in experimental settings. Hirsch et al. (2024) examined the fit between targeted advertisements and recipients' preferences, showing that perceptions of fit and misfit can carry different consequences. These studies observe or manipulate characteristics of messages that a general survey item does not capture. They establish a useful benchmark for interpretation: evidence that someone reports receiving relevant messages is weaker than evidence about the content, delivery and experimental comparison of those messages.

Perceived relevance may also reflect existing political interest. People who already follow public affairs may notice issue-specific messages more readily, recall them more accurately or interpret ordinary campaign material as tailored. Conversely, people with little interest may overlook personalised appeals even when they encounter them. Such selective attention creates a potential reverse pathway from engagement to reported exposure. Cross-sectional surveys cannot resolve this temporal ambiguity. In addition, the meaning of "customized" may differ across respondents. One person may associate the term with advertisements bearing their demographic identity, while another thinks of issue recommendations in a social feed. Without cognitive interviewing or validation against observed advertisements, researchers should avoid treating all endorsements of the item as equivalent indicators of the same technical intervention.

2.1 Democratic opportunities and concerns

Targeted political messages may have informational value when they connect people with policy topics that matter to them. A relevant message can reduce search costs and draw attention to otherwise neglected issues. But relevance can coexist with selective disclosure, privacy intrusions and manipulation. The normative evaluation depends on who controls targeting criteria, whether messages can be publicly scrutinised, how personal information is collected and whether recipients can recognise sponsored communication. Simchon et al. (2024) demonstrate that personality-matched persuasion can be investigated experimentally, but their results cannot be imported as effect estimates for a different population or observational design. The Delhi survey records respondents' opinions and self-reported activities, not the advertising content needed to evaluate these democratic trade-offs directly.

The transparency problem has two layers. First, a person may not know why a particular message appeared. Second, researchers may not know whether the person encountered the message through paid advertising, algorithmic recommendation, party communication or interpersonal forwarding. Both uncertainties matter for democratic accountability. Campaign-level transparency could be improved by searchable advertisement archives and records of targeting criteria, but these sources are not included in the present dataset. Consequently, the article avoids attributing respondents' answers to specific parties, technologies or electoral events. This is a substantive boundary, not merely a technical caveat, because it determines which political explanations the data can sustain.

2.2 Digital participation as differentiated behaviour

Political participation online includes activities that vary in effort, visibility and social risk. Reading political information is different from discussing it with family. Sharing a post exposes an opinion to a network, whereas commenting can involve public argument, criticism or endorsement. Membership in a political group may represent sustained identification, occasional monitoring or simple curiosity. These actions should not be collapsed automatically into a single engagement construct. Boulianne's (2015) meta-analysis indicates that the operationalisation of both social media use and participation affects reported associations. A measurement-sensitive study therefore asks which specific activity is related to which particular attitude. The present analysis focuses on sharing and commenting because they are separately recorded and have identifiable behavioural meanings.

Sharing and commenting may be associated because both require users to express themselves within social media environments. Yet the activities differ in their audience and cognitive demands. A user can share material without adding a position, or comment without redistributing content. Sharing may also reflect humour, disagreement or a desire to inform friends rather than endorsement of the political claim. A positive correlation between these items consequently supports the existence of overlapping self-reported practices, not a conclusion about political persuasion or democratic quality. This distinction is particularly important in polarised environments, where more frequent expression could represent deliberation, hostility, mobilisation or entertainment. The questionnaire does not independently classify the content or tone of each act.

2.3 Demographic differences and exposure

Age is a plausible correlate of platform habits, political experience and the kinds of messages people recognise. Younger and older users may differ in platform repertoires, digital literacy, social networks and attention to advertising. Nevertheless, the direction of an age difference cannot be deduced from

these possibilities. A sample can contain substantial demographic variation while showing little association between age and a specific targeting perception. Moreover, a cross-tabulation cannot distinguish age effects from cohort effects, life circumstances or unmeasured exposure opportunities. For that reason, the present analysis treats age as a descriptive grouping variable and reports an association test without converting the result into a developmental explanation.

3. Research questions and analytical expectations

The first research question asks whether perceived exposure to personalised political communication differs across age groups. The second asks whether respondents who report greater willingness to engage with targeted messages also report sharing political posts more often. The third asks whether sharing and commenting on political posts are associated. The questions were selected because they map directly onto questionnaire items and allow comparison with the earlier draft's statistical claims. They are not framed as causal hypotheses. The theoretical literature makes both positive and negative relationships plausible: personal relevance may attract attention, but privacy concerns, distrust or a preference for private political discussion may weaken public expression. Reporting the actual direction and magnitude is therefore more informative than imposing an assumed positive relationship.

The analytical logic distinguishes exposure, evaluation and activity. Perceived exposure refers to a respondent's recognition of customised communication. Receptiveness is an expressed inclination to engage with a specifically targeted message. Sharing and commenting are self-reports of activities undertaken online. These variables occupy different conceptual positions, even though they all appear within a single questionnaire. The article tests pairwise associations as a first step and does not construct a causal path model from their ordering. Such a model would require stronger measurement evidence, plausible temporal assumptions and consideration of confounders such as political interest, platform use, party identification and social norms. The available data cannot establish those requirements.

4. Research design and data

The research uses a quantitative cross-sectional survey design. The analytical dataset comprises 500 completed records in an encoded spreadsheet. Respondents were recruited through online voluntary participation in Delhi. The unit of analysis is an individual completed questionnaire. No probability sampling frame, selection probabilities or recruitment-response denominator was supplied for the present analysis. Accordingly, conventional confidence intervals for population proportions and claims about the prevalence of attitudes among Delhi voters would be unwarranted. Statistical tests describe relationships conditional on the achieved sample and the assumptions of the particular procedure. They should not be presented as evidence that the sample represents India or Delhi as a whole.

The instrument includes six demographic or voting-background fields followed by 48 agreement statements. For this article, the primary items are the age group, the statement about often receiving political communications customised to personal preferences, the statement about being more inclined to engage with a political message if specifically targeted, and statements about sharing and commenting on political posts. Responses are stored as integer codes from one to five. The analysis treats them as ordered categories. For the descriptive cross-tabulation, the codes are categorical rather than metric. Pearson correlations are reported for comparison with the earlier analysis, but their interpretation is conditional on an approximately interval treatment of the ordered response scale. Rank-based sensitivity checks are also appropriate.

4.1 Data verification and reproducibility

The data were re-read from the encoded workbook rather than copying statistical values from an earlier prose draft. Each of the 500 rows was retained for the selected variables. The age-by-exposure table contains five categories on each dimension, yielding sixteen degrees of freedom for Pearson's chi-square test. Cramér's V summarises association strength on a standardised scale. Pearson's r summarises linear covariation between the two selected five-point response items, and Spearman's rank correlation provides a sensitivity check that respects their ordinal character. This distinction matters because a numerical association can change when researchers confuse item positions, recode variables inconsistently or substitute an adjacent questionnaire statement.

The audit revealed that two statistics in the earlier working manuscript were not reproducible from the encoded dataset. The earlier age-by-exposure chi-square and the earlier regression of sharing on targeted-message receptiveness suggested statistically significant positive relationships. Direct recomputation produced a nonsignificant age association and a small negative receptiveness-sharing correlation. Rather than preserving the earlier narrative, the revised paper reports the recalculated values and treats the discrepancy as a reason to avoid inferential overstatement. The audit does not establish why the earlier values differed. Possible explanations include different variable selections, coding, an earlier dataset version or transcription. No explanation is asserted without an analysis log establishing it.

4.2 Statistical procedures

Pearson's chi-square evaluates whether the distribution of reported customised-message exposure is independent of age category in the observed contingency table. The test is omnibus and does not identify which cells would drive a significant association. Expected frequencies were inspected because small expected counts can compromise the chi-square approximation. Cramér's V is reported alongside the test because a p value alone does not convey the magnitude of association. For the two pairs of five-category items, Pearson's r is supplemented by Spearman's rho. These analyses are descriptive and associational. The study does not estimate an average treatment effect, predict an election outcome or measure a change in political attitudes before and after exposure.

Missingness and coding decisions are especially important in survey research. The selected variables in the encoded workbook were checked for numeric

values within the expected category range. Any missing values would require an explicit rule for pairwise or listwise deletion and reporting of the effective sample size. The present results are based on 500 available paired observations for the reported comparisons. No survey weights were used because inclusion probabilities were not established. A robust interpretation also recognises that the recruitment method can produce self-selection bias, while single-source self-reports can generate common-method associations. Neither concern is eliminated by a large nominal sample size.

5. Results: perceived exposure across age groups

Table 1. Age group by reported exposure to customised political communications (counts)

Age code	1	2	3	4	5	Total
1	3	22	20	53	1	99
2	0	21	22	56	2	101
3	0	22	19	50	4	95
4	1	20	20	58	4	103
5	1	24	28	45	4	102
Total	5	109	109	262	15	500

Note. Age categories are recorded as codes 1–5 in the encoded dataset. Exposure uses five ordered agreement codes. Pearson $\chi^2(16, N = 500) = 13.123$, $p = .664$, Cramér's $V = .081$. This table displays observed counts, not population estimates.

Table 2. Descriptive statistics for selected political communication and engagement items

Survey item	N	Mean	SD	1	2	3	4	5
Customised-message exposure	500	3.35	0.89	5	109	109	262	15
Willingness to engage with targeted messages	500	3.25	0.88	3	130	111	249	7
Sharing political posts	500	2.84	0.87	6	218	127	148	1
Commenting on political posts	500	2.75	0.88	8	245	113	133	1

Note. All four items use the five-category response codes in the supplied workbook. Means and standard deviations describe the encoded response distributions. Category frequencies sum to N = 500 for each item.

Table 3. Pearson correlations between selected political communication items

Variable 1	Variable 2	N	r	p
Targeted-message engagement willingness	Political-post sharing	500	-0.056	0.208
Political-post sharing	Political-post commenting	500	0.484	< .001
Customised-message exposure	Political-post sharing	500	-0.024	0.590

Note. Two-sided Pearson correlations calculated from the 500 encoded respondent records. Correlations describe association and do not establish causal effects.

The age-by-perceived-exposure contingency analysis produced $\chi^2(16, N = 500) = 13.123$, $p = 0.664$. Cramér's V was 0.081, indicating a small observed association. This test does not reject the null of independence at a conventional .05 threshold. It is therefore inaccurate to claim that perceived exposure differed significantly across age groups in these data. The result should not be inverted into a statement that every age group experiences precisely the same distribution. A nonsignificant omnibus test leaves room for small differences, measurement limitations and limited sensitivity to particular patterns. The appropriate statement is that this analysis did not detect a statistically persuasive age association in the achieved sample.

The observed exposure item has an overall mean above the scale midpoint, but that average does not show that respondents were actually microtargeted. The item captures perceived customised communication and is vulnerable to recall and interpretation differences. The distinction is important because respondents could endorse the statement after encountering general-interest political posts that happened to match their concerns. Furthermore, an age-by-exposure association would not reveal whether campaigns deliberately targeted particular age groups. Such a conclusion would require campaign advertising records or an experimental design. The absence of a strong age association in this sample also discourages the untested assumption that demographic differences must be central to every form of digital political communication.

5.1 Receptiveness to targeted messages and sharing

The Pearson correlation between willingness to engage with a specifically targeted political message and self-reported sharing of political posts was $r = -0.056$, $p = 0.208$, $N = 500$. The observed coefficient is small and negative, and the test does not provide evidence of a nonzero linear relationship at the conventional .05 level. The direction is also inconsistent with the earlier working draft's positive regression claim. A simple linear regression with these

exact variables would have an R^2 equal to the square of their Pearson correlation, approximately 0.003, rather than the previously asserted .099. This identity provides a direct reproducibility check for single-predictor ordinary least-squares regression with an intercept.

A null or weak association can arise for several substantive reasons. Willingness to engage with targeted communication may refer to reading, clicking or thinking about content rather than publicly redistributing it. Users may value relevant information while avoiding public political expression. Others may share posts for social reasons that have little connection to whether messages are personally tailored. These possibilities are interpretations, not established mechanisms. The survey did not independently measure the motives for each act of sharing. The data therefore warrant a modest conclusion: endorsing the receptiveness statement did not meaningfully distinguish frequent from infrequent sharers on the selected item within this sample.

5.2 Sharing and commenting

Sharing political posts and commenting on political posts were moderately and positively correlated, $r = 0.484$, $p < .001$, $N = 500$. The squared correlation is approximately 0.234, indicating that the two items share some linear variation while remaining far from interchangeable. This pattern is more consistent with a general tendency toward visible online political expression than with a specific personalisation mechanism. It also provides a contrast with the near-zero relationship between targeted-message receptiveness and sharing. The same respondents can display coordinated expression practices without linking those practices to favourable views of targeted communication.

The result does not establish that sharing causes commenting or vice versa. Both activities may be influenced by general political interest, network norms, personality, digital confidence or time spent on platforms. Because the survey is cross-sectional and based on self-report, the observed correlation could also reflect a general tendency to endorse participation-related statements. Researchers should therefore avoid presenting the association as evidence of increased civic efficacy or improved deliberative quality. A more informative next step would be to distinguish endorsement, criticism, humour and neutral redistribution, then compare these content types with the respondent's perceptions of targeted messages.

6. Discussion: a broken assumed sequence

A common account of digital campaigning implies a sequence: users receive tailored messages, recognise them as relevant, become more willing to engage and then participate visibly online. The present results caution against treating this sequence as an empirical default. The age association with perceived exposure was weak and nonsignificant. Receptiveness to targeted messages did not predict sharing in the selected bivariate comparison. Sharing and commenting were related, but that relationship concerned two activities rather than exposure to targeting. These findings suggest that the conceptual pathway may contain missing conditions or may operate differently across engagement outcomes. A cross-sectional dataset cannot resolve the pathway, but it can demonstrate that the simple assumed chain is not supported by these particular pairwise results.

The findings can be situated alongside experimental scholarship without claiming that the studies measure the same thing. Kruikemeier et al. (2016) manipulated exposure to personalised advertising and investigated persuasion knowledge. Hirsch et al. (2024) studied perceived fit in a panel design, allowing temporal ordering across waves. Simchon et al. (2024) investigated personality-matched persuasion in controlled studies. The Delhi survey instead records respondents' general recollections and attitudes. Differences between these designs are consequential: an experiment may isolate an advertising feature, while a survey of general experience can reveal how respondents interpret communication in their everyday information environment. The two approaches can complement each other, but their effect estimates and causal claims are not interchangeable.

6.1 Measurement validity and construct separation

The paper's most important methodological lesson concerns construct validity. The term microtargeting describes a process in which information about an audience is used to select or tailor political messages. A questionnaire statement about receiving customised messages is an imperfect proxy for that process. It may be influenced by political attentiveness, platform familiarity, perceived issue fit and the salience of particular campaigns. A respondent's agreement does not identify the sponsor, targeting criteria or message delivered. For future research, validated exposure measures could combine participant recognition tasks, screenshots, ad-library data and platform delivery records where ethically and legally accessible. Such triangulation would allow investigators to distinguish perceived personalisation from observed targeting.

The engagement side of the model requires equal care. A willingness statement is a self-assessment of inclination, not a record of clicking, sharing or commenting. A sharing statement is itself a self-report rather than a digital trace. Neither item establishes the number of posts shared, the political direction of the content, the audience reached or whether the activity was voluntary, strategic or habitual. These gaps are not reasons to discard survey research. They are reasons to specify what the survey measures and to avoid stronger language than the evidence permits. When authors use broad constructs such as mobilisation or political efficacy, they should provide evidence that the component indicators capture those constructs reliably.

6.2 Why nonsignificant findings matter

Publication incentives sometimes encourage researchers to prioritise statistically significant results, particularly when the theoretical topic attracts public concern. In the field of political microtargeting, this can lead to an exaggerated picture of uniform persuasive power. The present analysis demonstrates why verifying the exact dataset and variable mapping is essential before developing a strong narrative. The nonsignificant associations do not prove that personalised political communication lacks effects. They show that the selected self-report indicators do not exhibit the hypothesised relationships here. A rigorous article should treat this as a boundary condition and motivate improved measurement, rather than replacing the findings with speculation or searching many alternative specifications until significance appears.

A statistically nonsignificant result is also not an equivalence test. The sample is non-probability based, the variables are coarse five-category reports and measurement error may attenuate observed associations. A stronger claim of negligible association would require an equivalence margin specified in

advance, an appropriate uncertainty analysis and attention to design limitations. The present paper instead reports the coefficient magnitude and its test result transparently. This avoids the two symmetrical errors of declaring a causal effect from a small p value and declaring definitive absence from a large p value. The substantive inference is deliberately narrower: no convincing association was detected for the selected measures in this dataset.

6.3 Implications for political communication theory

The analysis suggests that models of digital political participation should separate three layers. The first concerns informational opportunity: which political content becomes available to a person and through what delivery process. The second concerns interpretation: whether the person notices, trusts, understands or appreciates the content. The third concerns action: whether the person discusses, shares, comments, donates, volunteers or votes. These layers can be related without forming a simple progression. Social norms, privacy concerns, partisan identities and platform affordances may alter transitions between them. The survey's null receptiveness-sharing relationship is consistent with the possibility that public expression has determinants distinct from message relevance, although it cannot identify those determinants.

This layered approach also helps distinguish political interest from democratic participation. An individual may consume highly personalised political information without acquiring balanced knowledge. Another may participate actively through public comments but rely on broad, non-targeted sources. Participation counts therefore cannot be used as a direct quality measure for information or deliberation. The normative debate about microtargeting should address the transparency and fairness of message delivery as well as the observable responses of recipients. The present dataset informs only a limited portion of that larger debate. Its value lies in preventing a category mistake: interpreting reported online activity as proof of targeted campaign persuasion.

6.4 Implications for research in India

Indian political communication research can benefit from designs that account for multilingual messages, varied platform access, interpersonal forwarding and the difference between party-originated content and user-generated commentary. These characteristics can shape how citizens recognise personalisation. A message that names a local issue may feel tailored without using personal data. A forwarded appeal may appear highly relevant because it comes from a trusted friend, even when the original campaign distributed it broadly. The current survey cannot distinguish these channels. Future studies could recruit across multiple regions and languages, include validated political-interest measures and collect actual advertisements alongside respondents' interpretations. This would make claims about targeting practices more defensible.

The Delhi setting is analytically useful but insufficient for a national inference. Online voluntary respondents are more likely to have the access and confidence required to complete a digital questionnaire, and the recruitment process does not establish how people with limited connectivity were represented. Generalisation would also require attention to differences in electoral context, language, platform use and local campaign organisation. A probability sample could support population estimates, while a carefully designed purposive sample might illuminate mechanisms in specific groups. Neither strategy is interchangeable with the present voluntary-response design. The article accordingly avoids claims that its numerical results describe Indian voters generally.

6.5 Practical implications

Campaign practitioners should not interpret respondents' general approval of targeted messages as evidence that targeting will generate sharing or visible mobilisation. Relevance may help a message receive attention, but additional conditions shape whether people wish to express political views publicly. Communication evaluation should therefore measure each intended outcome separately. A campaign seeking informational reach should track verified exposure and comprehension. A campaign seeking discussion should evaluate the quality and inclusiveness of conversation. A campaign seeking participation should use appropriate behavioural measures and respect privacy. The present findings do not justify a particular targeting strategy, but they do argue against using a single favourable attitude item as a proxy for political action.

For researchers and regulators, transparency remains central. Where platforms and campaigns disclose how political advertisements are targeted, scholars can compare observed delivery with user perceptions. Public repositories could also reveal whether different audiences receive substantively different policy claims. However, data access must protect individual privacy and avoid creating new risks through the disclosure of sensitive political attributes. The ethical challenge is to make campaign practices accountable without normalising intrusive profiling. The survey offers no evidence about the compliance of any particular campaign. It instead underscores the need for stronger empirical infrastructures before attributing political consequences to targeting systems.

7. Limitations

Several limitations constrain interpretation. First, the sample is voluntary and online, so inclusion probabilities are unknown and sampling bias cannot be estimated conventionally. Second, the design is cross-sectional, preventing temporal or causal conclusions. Third, the main measures are single self-report statements rather than validated multi-item scales or observed digital behaviour. Fourth, the exposure item records perceived customisation rather than verified microtargeting. Fifth, the five-point response format is ordinal, making rank-based checks relevant and requiring caution with interval-based models. Sixth, the study does not include campaign-level advertising records, actual message content, or platform exposure histories. Finally, the analysis focuses on a narrow set of preselected relationships and does not establish the full network of possible confounders.

The data audit also revealed a discrepancy between earlier prose statistics and direct recomputation from the encoded workbook. The present manuscript treats the workbook as the operative data source, but the authors should confirm that it is the final approved dataset and document the exact survey coding before submission. The analysis does not attempt to explain discrepancies through unsupported claims about previous files. A reproducibility package

containing the codebook, de-identified data where consent permits, and analysis syntax would allow reviewers to inspect the variable mapping. This is especially important when multiple thesis and manuscript versions have circulated during revision.

8. Future research

A stronger test of personalisation effects would combine experimentally varied messages with realistic social-media contexts. Researchers could manipulate whether content is personalised, whether targeting is disclosed and whether the message is congruent with recipients' policy priorities. Outcomes could include attention, comprehension, persuasion knowledge, sharing intention and observed sharing behaviour. A longitudinal component could distinguish immediate response from persistent engagement. Such a design would separate the effects of message content from the effects of delivery and perceived relevance. It would also permit explicit testing of whether privacy concern or political interest moderates the relationship between tailored content and action.

Observational work could complement experiments by linking consented exposure diaries or advertising archives with repeated surveys. Participants might be asked to classify actual political messages they encountered, identify their sources and describe why they appeared personally relevant. Researchers could then compare subjective reports with observable targeting information. Cross-language cognitive interviews would help determine whether terms such as customised, targeted and personalised are understood consistently. Finally, mixed-methods interviews could clarify why respondents who appreciate targeted messages do not share them. These steps address the measurement and mechanism gaps revealed by the present analysis without assuming that every relevant message was algorithmically microtargeted.

9. Conclusion

This article reassesses the relationship between perceived personalised political communication and online political engagement using 500 encoded survey records from Delhi. The verified age-by-exposure association was small and nonsignificant, and willingness to engage with targeted messages was not meaningfully related to sharing political posts. Sharing and commenting, by contrast, showed a moderate positive relationship. These results do not support a simple progression from perceived targeting to visible political expression. Their central contribution is to distinguish delivery, perception and action, and to show how an empirical claim can change when the precise source variables are checked. The findings are limited to the achieved sample and the measured statements. Stronger claims about campaign targeting, persuasion and electoral effects require designs that observe those processes directly.

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