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Selecting Materials Outreach Topics from Community Repair Demand: Organizational Weighting, Temporal Transfer and Local Fit

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ABSTRACT

Community repair events bring materials science into contact with familiar products, but outreach teams must decide which objects deserve limited preparation time. This study evaluates topic selection under two representation rules: equal influence for each recorded repair attempt and equal influence for each repair group. The Open Repair Alliance collection contains 48,669 attempts. Selection used 17,805 attempts from 95 groups, followed by evaluation on 18,802 attempts from 126 groups. The four topics selected by attempt weighting were laptops, lamps, separate audio equipment and coffee makers. Group weighting replaced the latter two with mobile phones and small kitchen appliances. Its selection-period advantage in average group coverage contracted from 3.09 to 0.43 percentage points in the following year. Network-stratified group resampling gave a 95% interval of -1.29 to 2.28 points for that contrast, while attempt coverage decreased by 3.27 points. Local differences had opposing signs: 58 groups gained coverage, 63 lost coverage and five were unchanged. Resampling the selection groups recovered the complete topic sets in only 19.28% and 35.84% of draws. Increasing the kit to eight topics made the two rules select identical categories; ten topics were needed to exceed one-half of average group demand. These results establish the consequences of representation choices without attributing learning gains to category matching. A shared materials kit therefore requires an explicit intended beneficiary, host-specific preparation and an educational assessment independent of repair completion.

KEYWORDS

materials education; community repair; outreach planning; organizational weighting; temporal transfer; product disassembly.

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1. INTRODUCTION

Repair outreach places a materials decision inside an encounter that already matters to the public. A household object has stopped functioning, access to its components is uncertain, and someone must decide whether intervention is worthwhile. Questions about electrical contact, deformation, joining, thermal exposure and component replacement can arise before anyone uses the vocabulary of materials science. Such encounters create an opportunity to connect technical explanation with decisions about continued use. Circular product design distinguishes retaining an assembled product's usefulness from recovering its constituent matter after disposal. That distinction gives repair an educational purpose beyond demonstrating an isolated property: it exposes how material choices shape the persistence of a working object [1].

The opportunity is also organizational. An outreach team cannot prepare equally detailed explanations, examples and facilitation for every product that might arrive. A portable kit imposes limits on the number of contexts that can be represented, even when its contents are inexpensive. Selecting those contexts requires a decision about whose experience should influence preparation. Total recorded frequency privileges categories encountered by large recording groups. Giving organizations equal influence can elevate categories that occur across smaller groups. Neither choice is neutral, and neither automatically identifies the products of greatest concern to people who have never attended a repair event.

Repair communities connect local practices through wider networks, while retaining differences in membership, product interests and organizational arrangements. Research on translocal repair and plastics initiatives shows why a network should not be treated as a collection of interchangeable outlets [2]. Cooperation can circulate ideas and resources without making local participation uniform. For materials outreach, this means that an internationally common product example can still be a poor fit for a particular host. A shared inventory must therefore accommodate both transferable technical questions and differences in the products around which those questions become meaningful.

The material and social aspects of repair are closely connected. Community repair can support continued product use while also creating relationships around expertise, mutual assistance and the value attached to possessions [3, 4]. A damaged object does not arrive as an anonymous material sample. Its owner may associate it with convenience, cost, memory or dependence on a particular function. Those associations affect whether a technical explanation is welcome and what form it should take. Outreach that treats every encounter as a miniature laboratory lesson risks overlooking the reason that the person brought the object in the first place.

This tension is visible in research on the relationship between repair service and learning. Free assistance can attract visitors who would not independently seek instruction about product durability, yet the encounter can also settle into a service relationship in which the visitor watches rather than participates [5]. Educational analyses of repair tables further show how attention is organized through tools, objects and interactions, rather than through technical information alone [6]. Communal mending research likewise connects practical participation with changing relationships to materials [7]. Together, these findings justify preparing meaningful object encounters, while cautioning against equating an available explanation with an explanation that someone has understood.

Formal engineering education provides a related but distinct precedent. Teaching right-to-repair principles through product design connects sustainability with decisions about assembly, access and replacement [8]. University repair and sharing practices also extend beyond what students encounter in scheduled teaching, with everyday arrangements affecting whether circular intentions become routine actions [9]. A community outreach kit occupies the boundary between these settings. It can draw on technical expertise developed in universities, but its topics, duration and participation should respond to the host organization rather than assume the structure of a university course.

Participation research makes the intended beneficiary especially important. Low participation in informal science does not necessarily indicate low interest; institutional arrangements and social conditions can restrict access even when interest is present [10]. Studies of informal pathways into science emphasize how activities are organized and connected, rather than treating entry as an individual choice detached from opportunity [11]. Consequently, representing a repair group in a topic-selection calculation is an organizational commitment, not a measurement of whether marginalized residents are being served. That distinction is necessary whenever an arithmetic weighting rule is discussed alongside inclusion.

Equitable educational outcomes require attention to recognition, agency and the legitimacy of participants' own knowledge [12, 13]. Equal influence for organizations cannot supply that information, because organizations vary in the populations they reach and in the barriers surrounding participation. The present comparison therefore uses the precise term equal-group weighting. It does not use that term as a substitute for demographic equity. Its purpose is to expose the consequences of one planning choice so that community partners can decide whether the choice fits their responsibilities and circumstances.

Relationships between universities and informal education providers involve negotiation over objectives, resources and authority [14]. Experience from science centres similarly indicates that sustained work with community partners can change what institutions prioritize, including the balance between predetermined content and local relationships [15]. These observations suggest that selecting outreach topics should be open to discussion with the host. A statistical shortlist may make the trade-off visible, but it should not override local knowledge about what visitors need, what staff can facilitate or which activities would interfere with the repair service.

Repair behaviour also varies between product groups and depends on more than material accessibility. Research on household devices connects repair with competence, product use and the surrounding arrangements through which assistance becomes available [16, 17]. Comparisons across electronics, clothing and furniture further demonstrate that attachment, perceived repairability and financial considerations can have different importance for different objects [18]. A product category is therefore a context for investigation rather than a complete account of repair demand. Even a correctly selected category contains variation that must be addressed during preparation of the actual demonstration.

Cost, convenience and the timing of assistance can determine whether an opportunity to repair is taken up [19, 20]. Wider policy and market arrangements also affect access to instructions, spare parts and independent services [21]. Outreach cannot remove these conditions simply by explaining a material property. It can, however, help participants distinguish a product's physical construction from the surrounding restrictions on maintaining it. That distinction matters when a technically accessible component remains difficult to replace because the necessary information or part is unavailable.

Evidence from visits to online repair instructions shows that interest in repair changes over a product's lifetime and need not follow a repairability score [22]. The timing of topic selection therefore deserves explicit evaluation. A list that matches one period's activity may transfer poorly to the next as recording networks expand, local specializations change or the products presented at events shift. Splitting records by time tests a practical condition that a random division of attempts would leave largely unexamined: whether a kit selected from earlier activity still fits a later year's event records.

Large repair-record collections can answer questions that interview studies alone cannot address, but their variables constrain the permissible conclusions. A recent citizen-science investigation of household maintenance combines survey responses with detailed repair logs, illustrating the distinct information available from different forms of participation [23]. Work examining long-running repair records shows that recording practices and missing technical detail affect what can be learned about reparability [24]. Estimating opportunities for replacement parts similarly requires care when product descriptions do not identify the failed component or its functional requirements [25]. These studies support the use of repair records for explicitly delimited questions, rather than for unobserved explanations of why products failed.

Materials education makes those boundaries particularly consequential. Product names cannot establish polymer grade, alloy composition, contact resistance or crack-growth mechanism. Technical repairability methods instead inspect particular constructions and disassembly operations [26, 27]. Diagnostic guidance must also distinguish recognition of a symptom from locating the component responsible for it [28]. An outreach-planning study can therefore evaluate which contexts receive preparation while leaving material identification and failure diagnosis to evidence obtained from the selected object itself.

The question examined here is how the unit of representation changes a limited materials-outreach inventory, and whether the resulting choice persists across time, organizations and kit sizes. The analysis compares equal influence per attempt with equal influence per repair group, evaluates fixed choices in a subsequent year, quantifies topic-selection stability and follows the consequences of changing the number of prepared topics. The contribution is a concrete account of how a seemingly simple ranking decision changes organizational coverage. Its educational interpretation remains deliberately separate: the records establish topical fit, while explanations, participation and learning require observation during an outreach encounter.

2. MATERIALS AND METHODS

2.1. Repair records and eligibility

Community-based repair activities across eighteen countries, providing insights into product repair practices, organizational participation, and the role of collaborative repair initiatives in promoting sustainable consumption [29–31]. The investigation encompassed 48,669 recorded repair attempts contributed by volunteer repairers and community organizations, including anstiftung, Fixit Clinic, Repair Café International, Repair Cafe Wales, and The Restart Project. The research documented repair activities across different product categories and organizational settings, enabling comparisons of repair frequency, product representation, and patterns of community participation. Particular attention was given to the classification of products, the timing of repair activities, and differences in the contributions of participating organizations. These observations provide a foundation for examining how repair demand varies across communities and how organizational characteristics may influence the representation of particular product categories. The findings also offer opportunities for materials science education by connecting everyday repair activities with discussions of product construction, material selection, durability, and sustainable resource use. Quantitative comparisons of repair activity can further support the identification of relevant outreach topics and the development of educational approaches responsive to local repair interests.

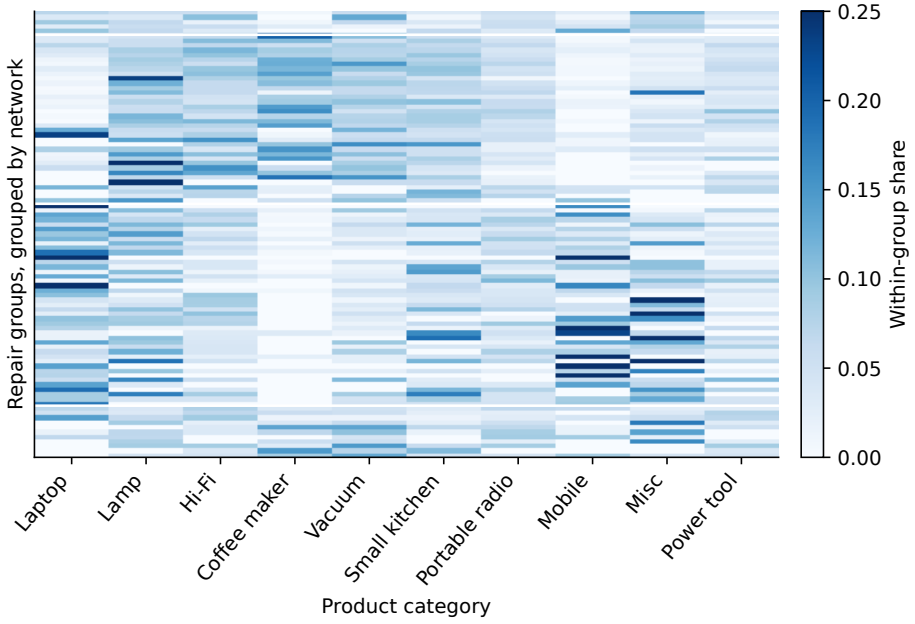
Provider and record identifiers form a unique key for every entry, so no duplicate-key deletion was necessary. One lower-case laptop entry was combined with Laptop, leaving forty categories. Group identifiers are present for 48,182 records and distinguish 275 organizations when combined with provider identity. The remaining entries lack a usable group identifier and cannot contribute to calculations that give organizations equal influence. A unique record key does not establish a unique device or visitor; the collection contains no common identifier that would resolve repeated visits across events.

A group must contain at least twenty recorded attempts in the relevant window. This restriction retains 17,805 selection attempts in 95 groups and 18,802 evaluation attempts in 126 groups. The selected windows avoid the oldest dates and the disruption of in-person activity during 2020. Eligibility is assessed independently in each period. Consequently, returning organizations and organizations that become eligible only in the later window can both enter evaluation. This arrangement reflects a changing operating environment rather than a balanced panel of permanently observed sites.

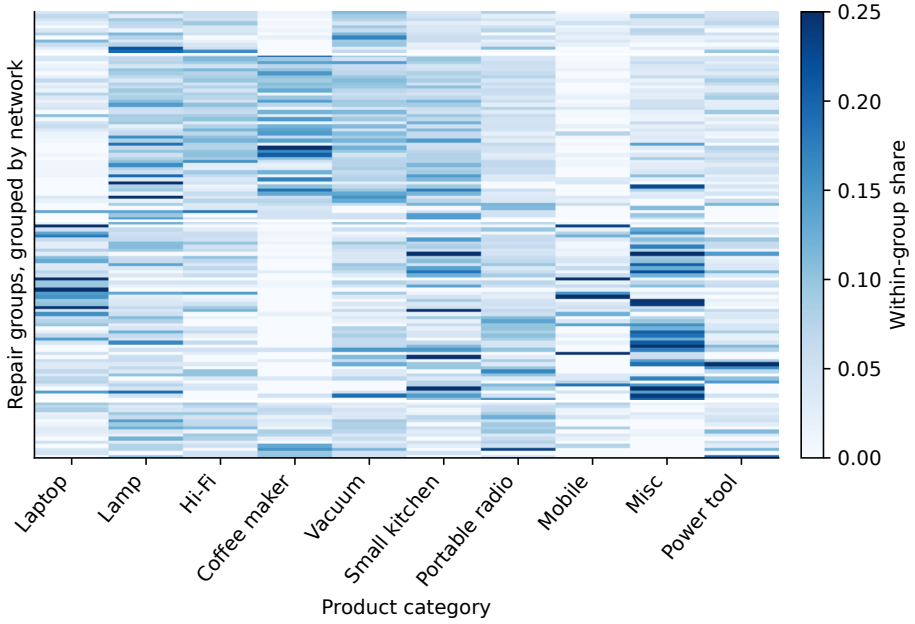
The eligibility restrictions retain 97.67% of attempts in the selection window and 97.04% in evaluation. High retention of attempts should not be confused with high retention of organizations. Small groups can contribute little to the pooled count while carrying substantial influence under equal organizational weighting. Excluding them may therefore have only a modest effect on attempt totals but a larger effect on the organizational average. This is why the threshold comparison remains necessary even when most individual records survive the restriction. The relevant question is not simply how many entries are retained, but which operational voices the restriction removes.

A category can enter a kit if it has at least one hundred selection attempts and is not labelled Misc. Thirty-five categories satisfy this condition. Misc remains in the denominator because its entries still form part of the demand encountered by a group. It is excluded from the selectable list because it does not identify an object around which a consistent technical discussion can be prepared. The eligibility restrictions stabilize the planning calculation but are not a calculation of educational sample size. Figure 1 displays the category shares within the eligible organizations and makes the grouping structure visible.

The records do not contain measured composition, mechanical properties, temperature, repair duration or participant learning. Problem descriptions are blank or contain NULL in 8,456 entries, and the barrier field is empty in 47,981. Neither field enters selection. Repair completion is also excluded from the objective because successful restoration and educational participation are different outcomes. This restriction avoids rewarding a topic for easy repairs or penalizing it for a missing replacement part. The question is which contexts receive preparation, not which product class offers the highest success rate.



(a) Selection period: 95 groups



(b) Evaluation period: 126 groups

Figure 1: Local category concentrations in the two observation windows.

Similarly, unavailable repair descriptions cannot be interpreted as evidence that no identifiable fault existed. A blank entry describes the recording process, not the physical condition of an object. The calculation avoids that ambiguity by using category and group membership only. It would be inappropriate to fill missing descriptions with a common failure mechanism, even when that mechanism seems plausible for the named product. Such replacement would create technical assertions that no observation in the analyzed fields could verify.

2.2. Representation and topic selection

Let n_{gc} denote the number of attempts in group g belonging to category c, and let N_g count all attempts in that group. The attempt-weighted share assigns every recorded attempt the same influence. The organization-weighted share calculates a proportion inside each group and then averages those proportions, as defined in Eq. (1).

$$p_c = \frac{\sum_{g=1}^{G} n_{gc}}{\sum_{g=1}^{G} N_g}, \qquad q_c = \frac{1}{G}\sum_{g=1}^{G} \frac{n_{gc}}{N_g}. \tag{1}$$

The distinction parallels the difference between individual-average and cluster-average targets in the analysis of grouped observations [32, 33]. Here, however, neither quantity is a treatment effect. They are descriptive shares of repair activity used to determine which product contexts enter a kit. If category prevalence is associated with group size, the two shares can differ even when every record is entered without error. A disagreement is therefore not evidence that one calculation is biased relative to the other; it indicates that they answer different planning questions.

The difference between the two category shares has an exact descriptive interpretation. Writing each within-group share as $r_{gc} = n_{gc}/N_g$, the difference equals the covariance between group size and that share, divided by mean group size. Eq. (2) uses the covariance calculated across the eligible groups with divisor equal to the number of groups. Positive values identify categories whose within-group share tends to be greater in larger recording organizations. Negative values identify the opposite association. This identity explains a weighting difference without requiring a fitted regression or suggesting that organizational size itself causes a preference for particular products.

$$p_c - q_c = \frac{\mathrm{Cov}_G(N_g, r_{gc})}{\overline{N}}, \qquad r_{gc} = \frac{n_{gc}}{N_g}, \quad \overline{N} = \frac{1}{G}\sum_{g=1}^{G} N_g. \tag{2}$$

A kit contains K distinct topics. The primary comparison uses $K = 4$ and assigns one planning slot to each topic. Maximizing the sum of attempt-weighted shares selects the four largest eligible attempt shares; maximizing the sum of organization-weighted shares selects the four largest eligible group shares. This solution follows because categories are mutually exclusive and the objective is additive. Topic slots are equal only in the calculation. Their financial costs, physical bulk and facilitation demands are unmeasured, so the model does not estimate the best staffing arrangement or equipment purchase.

A continuous weight connects the two objectives. Eq. (3) assigns weight λ to organizational representation and the remaining weight to attempt representation. Pairwise intersections between the linear category scores identify all candidate transition points. Evaluating the highest-scoring categories within each intervening interval gives the exact regions over which kit membership is constant. Rank changes inside an unchanged topic set are disregarded. Alphabetical ordering resolves exact ties at an intersection; this convention affects the boundary point rather than the interval on either side.

$$s_c(\lambda) = (1-\lambda)p_c + \lambda q_c, \quad 0 \le \lambda \le 1, \qquad S_K(\lambda) = \arg\max_{\substack{S \subseteq \mathcal{C} \\ |S| = K}} \sum_{c \in S} s_c(\lambda). \tag{3}$$

For a fixed topic set S, coverage is the fraction of attempts whose category is represented. Eq. (4) gives attempt coverage and average group coverage. The denominator always includes unselected and Misc entries. A category match indicates topical correspondence, not a completed lesson, an available spare part or sufficient repair capacity. Both quantities are calculated in the selection period and then recalculated for the unchanged set in the following year. Lower-tail coverage and paired group differences describe how unevenly that correspondence is distributed.

$$C_A(S) = \sum_{c \in S} p_c, \qquad C_G(S) = \sum_{c \in S} q_c, \qquad d_g = \frac{\sum_{c \in S_G} n_{gc} - \sum_{c \in S_A} n_{gc}}{N_g}. \tag{4}$$

2.3. Temporal comparison and resampling

Temporal separation is used because the practical task is preparation for subsequent activity. Structured observations can yield misleading assessments when dependence is ignored or closely related records enter both selection and evaluation [34]. The separation here prevents a later attempt from influencing its own topic score, but returning groups remain present across years. It therefore tests temporal transfer within an evolving network collection, not performance in an entirely independent population. No product-composition variable is reconstructed from its category name, and no later learning outcome is assumed.

The primary paired contrast subtracts coverage of the attempt-weighted kit from coverage of the group-weighted kit within each eligible evaluation group. To assess uncertainty while retaining network composition, groups are sampled with replacement within each network, preserving the number of groups contributed by that network. Each draw retains the complete category-count vector and group size. Ten thousand draws provide percentile intervals for both average group coverage and the ratio defining attempt coverage. The random generator is initialized with 27102026. This procedure accommodates dependence among attempts within a group while conditioning on the four observed networks and the selected topic sets.

A second calculation addresses uncertainty during selection. Five thousand network-stratified draws resample the selection groups, preserve the fixed category eligibility list and recalculate both four-topic kits. A repeated group contributes repeated weight to its full count vector; for organization weighting, each drawn occurrence contributes one within-group proportion. Category inclusion frequencies and the frequency of recovering the complete primary set quantify selection stability. Evaluating each selected set on the unchanged 2019 groups describes sensitivity to preparation evidence. Those ranges are not confidence intervals for future learning or for a new national population.

2.4. Organizational contrasts and preparation limits

Network-level contrasts are decomposed according to Eq. (5). Each network's within-group coverage difference is multiplied by its share of eligible groups. Summing these contributions recovers the overall average-group difference exactly. The identity separates the magnitude of a network's preference from its influence on the combined answer. An analogous calculation uses attempt shares for attempt-weighted coverage. The analysis does not assign a network-specific causal effect, because networks were neither randomized nor made comparable in their participants, equipment or recording practice.

$$\Delta_G = \frac{1}{G}\sum_{g=1}^{G} d_g = \sum_{h=1}^{H} \frac{G_h}{G}\,\bar{d}_h, \qquad \bar{d}_h = \frac{1}{G_h}\sum_{g\in h} d_g. \tag{5}$$

Sensitivity calculations repeat selection and evaluation with ten- and forty-attempt group thresholds. A separate deletion check removes one network at a time from selection, while retaining the combined evaluation population. Kit size varies from two to ten topics, with eligibility held fixed, to determine whether the primary disagreement persists as preparation broadens. Newly eligible and returning groups are compared descriptively. No significance tests are attached to these multiple exploratory comparisons. Inference based on a small number of independent clusters can be unreliable [35], so the four networks are treated as fixed organizational contexts rather than as sufficient replication for worldwide claims.

The mathematical definitions, threshold choices and resampling conditions are stated in the main text rather than in an appendix. Their purpose is to make the preparation decision inspectable by a host organization. The two photographic-style object views later in the article are digitally generated illustrations of ordinary assemblies; they do not document tested products or observed repairs. They are excluded from every calculation and support no claim about a particular polymer, alloy or failure mechanism. Their role is to show the kind of object on which an independently checked materials explanation could be developed.

3. RESULTS AND DISCUSSION

3.1. Topic substitution and its arithmetic consequences

The two representation rules produce different four-topic inventories despite using the same eligible observations. Attempt weighting selects Laptop, Lamp, Hi-Fi separates and Coffee maker. Group weighting selects Lamp, Mobile, Laptop and Small kitchen item. The labels follow the recording standard; in the discussion, separate audio equipment and small kitchen appliances refer to the corresponding recorded categories. Two topics are common to both sets, while two are replaced. This is a substantial preparation change because half the primary kit changes before any demonstration is written or equipment is assembled.

The selection-period figures confirm the intended trade-off. The attempt-weighted kit covers 27.56% of recorded attempts, compared with 25.48% for the group-weighted kit. Average group coverage moves in the opposite direction, from 24.19% to 27.28%. The organizational objective therefore gains 3.09 percentage points while surrendering 2.08 points of attempt coverage. Table 1 places these quantities beside the subsequent evaluation. Improvement in the objective used for selection is expected mathematically and cannot be treated as independent validation of an outreach strategy.

The later-year difference can be traced to the substituted categories rather than attributed to an unspecified change in overall fit. Figure 2 separates their signed contributions. Removing coffee makers and separate audio equipment loses 6.47 and 6.64 percentage points of attempt coverage. Adding mobiles and small kitchen appliances restores 3.16 and 6.68 points. Their sum gives the net attempt-coverage change. When each organization has equal influence, the same substitutions contribute −4.61, −5.50, 3.45 and 7.08 points. The different denominators explain why one inventory can appear slightly preferable organizationally while matching fewer attempts overall.

The substitutions also clarify the material-preparation decision. Both kits retain a computer context and a lighting context. Their disagreement concerns the remaining examples, not whether repair outreach should involve electronics at all. Replacing a coffee maker with a mobile phone changes the assembly, access and documentation that facilitators must understand. Replacing separate audio equipment with a broad kitchen category changes how specific the demonstration can be before the host identifies an actual object. Those practical consequences cannot be recovered from the numerical score alone, even though the score makes the change of emphasis explicit.

Mobiles illustrate the importance of that association. They account for 4.77% of selection attempts but 7.17% of the average organization's activity. Coffee makers show the opposite pattern, with corresponding shares of 6.21% and 4.27%. Small kitchen appliances have almost identical shares under the two rules, approximately 5.88% in each case, yet still enter the organizationally weighted kit because their competitors change position. Inclusion is therefore relative to the other candidate categories. A selected topic need not become more common in absolute terms when the weighting rule changes; it can enter because alternative topics lose influence.

A recorded category remains too coarse to establish intrinsic repairability. Comparative assessments of washing-machine evaluation systems show that different assessment procedures can emphasize different repair features [36]. Smartphone investigations likewise distinguish reliability, repairability and the product-design features captured by repair scores [37, 38]. The present coverage calculation operates at an earlier decision stage: it chooses the product context to prepare. Treating that choice as evidence of a material property would bypass the inspection that technical repairability assessment requires. A frequently encountered context merits preparation, but its frequency does not reveal why an individual unit stopped functioning.

3.2. Temporal performance and the distribution across groups

Average group coverage in 2019 is 22.83% for attempt weighting and 23.26% for group weighting. The difference is only 0.43 percentage points. Resampling complete groups within their networks gives a 95% interval from −1.29 to 2.28 points. This interval accommodates modest disadvantage as well as advantage. The conclusion is therefore not that organizational weighting improves subsequent outreach preparation. The supported conclusion is that the sizeable advantage obtained in the selection window does not persist with comparable magnitude in the next year's observations.

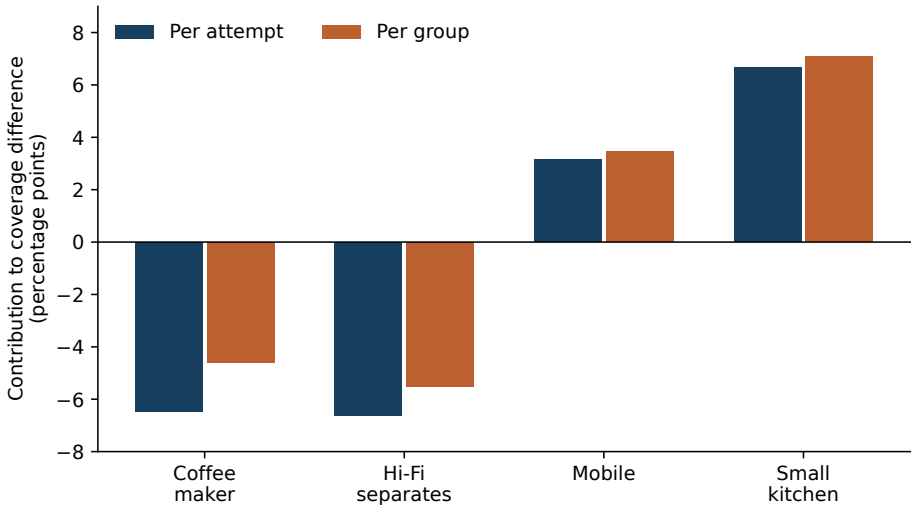


Figure 2: How the exchanged topics contribute to the later-year coverage difference.

Attempt coverage is 25.99% and 22.72%, respectively. The corresponding difference is −3.27 points, with a stratified interval from −6.00 to −0.60. Under the stated resampling assumptions, the cost in matching recorded attempts is clearer than any benefit in matching an average organization. The contrast matters because an outreach team could otherwise select the organizationally weighted inventory and describe its slightly larger average as an uncomplicated improvement. Table 1 instead shows the beneficiary and denominator beside every value, preserving the practical consequence of that choice.

The earlier calculation that allowed network composition to vary during resampling gives an average-group interval from −1.82 to 2.83 points. Its wider span is consistent with allowing another source of variation into the calculation. Both procedures retain the same point difference and both span zero. Their agreement on that limited conclusion is more useful than choosing whichever interval appears most favorable. Neither calculation establishes independence among organizations sharing a network, nor estimates how a new network outside the observed collection would behave.

Table 1: Coverage under the two representation rules.

Period and denominator	Attempt kit (%)	Group kit (%)	Difference (points)
Selection: per attempt	27.56	25.48	−2.08
Selection: per group	24.19	27.28	3.09
Evaluation: per attempt	25.99	22.72	−3.27 [−6.00, −0.60]
v Evaluation: per group	22.83	23.26	0.43 [−1.29, 2.28]
Evaluation: tenth percentile across groups	9.89	11.11	1.22

Finally, uncertainty intervals concern variation under a specified resampling procedure. They do not state the probability that a particular outreach kit will improve an event, and their inclusion of zero does not prove that the two kits are equivalent. Establishing equivalence would require an acceptable difference to be defined beforehand and evidence precise enough to exclude larger departures. The observed intervals instead support a narrower conclusion: the records do not establish a consistent organizational advantage for the four-topic choice across the conditions examined here.

The small positive mean also conceals uneven local changes. Coverage increases in 58 groups, decreases in 63 and is unchanged in five. The median paired difference is −0.27 percentage points. The middle half of differences lies between approximately −7.97 and 6.52 points, while the complete range extends from −29.55 to 55.73. A positive average is therefore compatible with more groups losing than gaining. The empirical distributions in Figure 3 retain this heterogeneity instead of reducing local experience to a single favorable aggregate.

This distribution changes the interpretation of organizational representation. Equal weighting gives the same influence to each eligible organization’s category proportions, but selecting the largest average proportions does not

maximize the number of organizations that benefit from a switch. A few large positive differences can outweigh several smaller negative differences. Nor does maximizing an average protect the least-covered site. The tenth percentile of coverage rises from 9.89% to 11.11%, but it remains low, and the objective never directly optimizes that percentile. A host concerned with minimum preparation must state that requirement separately.

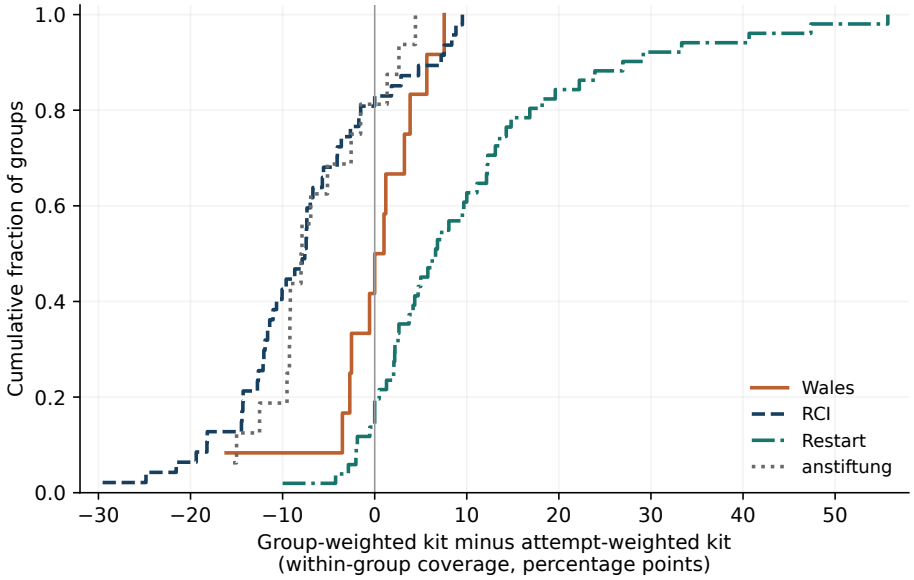


Figure 3: Distribution of local changes when the representation rule changes.

Returning and newly eligible groups show another distinction. In the 68 returning groups, average coverage changes from 25.09% to 24.77%. In the 58 groups eligible only during evaluation, it changes from 20.17% to 21.49%. These comparisons suggest different patterns of fit, but they do not establish organizational development or an effect of joining a network. Eligibility can change because of recording volume, incomplete earlier observation or actual expansion. The labels identify membership in the analytic windows and should not be interpreted as organizational histories.

3.3. Network composition and aggregate cancellation

The networks contribute different numbers of organizations and different volumes of attempts. Repair Café International accounts for 37.30% of eligible evaluation groups but 53.17% of attempts. The Restart Project contributes 40.48% of groups and 28.93% of attempts. Thus, changing the unit of representation also changes the relative influence of these two large contributors. Figure 4 combines organizational weight, within-network contrast and attempt volume without implying that network identity causes any particular product preference.

The within-network average-group differences are −0.24 points for Repair Cafe Wales, −7.67 for Repair Café International, 10.22 for The Restart Project and −6.48 for anstiftung. Multiplying by their respective group shares yields contributions of approximately −0.02, −2.86, 4.14 and −0.82 points to the overall difference. The near cancellation is the arithmetic reason that the combined answer is close to zero. It is not evidence that the topic sets are interchangeable for every organization. Table 2 retains both the local contrast and its contribution to the combined result.

Table 2: Network contributions to the combined group-average difference.

Network	Groups	Attempts	Local difference (points)	Contribution (points)
Repair Cafe Wales	12	1,183	−0.24	−0.02
Repair Café International	47	9,997	−7.67	−2.86
The Restart Project	51	5,439	10.22	4.14
anstiftung	16	2,183	−6.48	−0.82

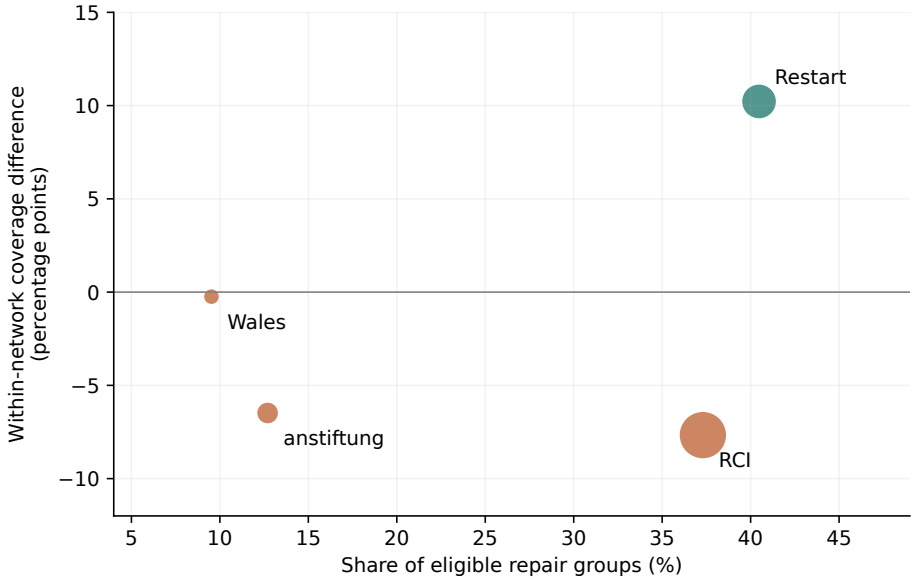


Figure 4: Organizational influence and within-network preference.

The positive contribution from one network outweighs negative contributions from the other three in the combined group average. A team serving a particular host would therefore lose important information by applying the international mean without inspecting its local context. At the same time, the network comparison does not justify labelling one network more educationally advanced or more efficient. Product specialization, record entry, visitor composition and available repair expertise can all contribute to the observed distribution. None is identified sufficiently to support a ranking of organizational quality.

Giving each of the four networks equal influence produces another descriptive answer: the mean of the within-network differences is −1.04 percentage points. This differs in sign from the average across individual organizations. The contrast does not identify a preferred policy; it shows that the hierarchy of representation continues above the repair-group level. A university partnership coordinated by networks might ask each network to have equal influence, whereas an outreach team scheduling visits directly with local groups might reasonably average over those groups. Both decisions should be stated before the combined result is interpreted. Otherwise, an unstated weighting convention can determine whether the same recorded differences are described as an advantage or a disadvantage.

Removing networks during topic selection makes their influence more apparent. When The Restart Project is omitted, both representation rules choose the same four contexts: coffee makers, vacuums, separate audio equipment and lamps. Removing Repair Café International changes the attempt-weighted selection more substantially than the organization-weighted selection. These deletions show dependence on the composition of the participating collection, rather than testing transfer to a new network. The distinction is essential: an evaluation that still contains the omitted network cannot be presented as an independent test on organizations never used by the wider analysis.

3.4. Continuous weighting and unstable topic boundaries

The continuous calculation identifies three exact changes in kit membership. Coffee maker gives way to Vacuum at approximately $\lambda = 0.0852$. Vacuum gives way to Mobile at 0.4647, and Hi-Fi separates gives way to Small kitchen item at 0.7655. Figure 5 displays membership over the full weight range. The result is a sequence of stable intervals separated by substitutions, rather than continuous change in the objects an educator must prepare. This distinction helps translate an abstract preference for organizational representation into a visible preparation decision.

The first substitution produces a kit whose average group coverage in evaluation is 24.58%, while its attempt coverage is 26.22%. Both exceed the corresponding values for the pure attempt-weighted four-topic kit in the later year. However, the intermediate kit containing mobiles with separate audio equipment has average group coverage

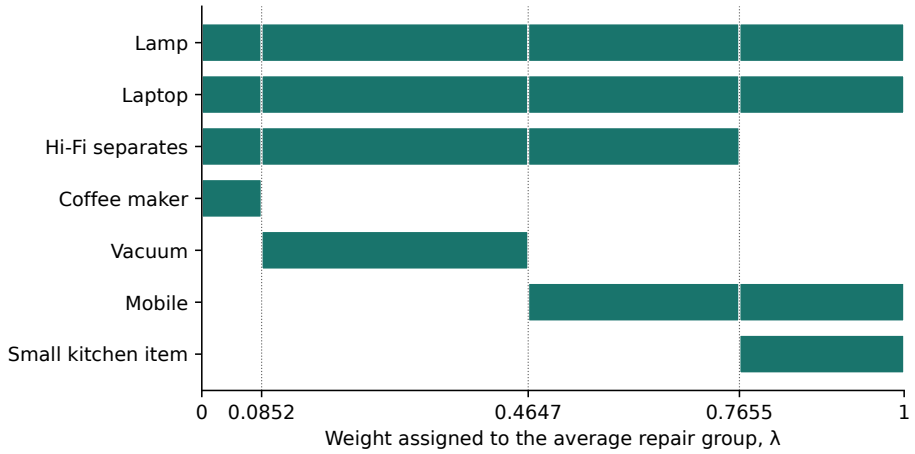


Figure 5: Intervals over which the four-topic inventory remains unchanged.

of only 21.67%. Increasing organizational weight therefore does not produce a monotonic improvement in later organizational fit. The category scores are calculated from earlier activity, and maximizing their weighted sum cannot guarantee the ordering of a different period's outcomes.

The favorable intermediate result should not be turned into a retrospectively selected optimum. Selecting the weight after examining all later outcomes would use evaluation observations to determine preparation, undermining the temporal comparison. A practical organization could choose a weight beforehand to express its intended beneficiary, or seek local evidence before replacing a topic. The present calculation establishes where membership changes and what happened to those fixed sets in the next year. It does not establish that the same numerical weight is appropriate for a different country, network or future event.

Threshold sensitivity provides an additional reason to avoid a universal recommendation. With a ten-attempt minimum, average group coverage is 22.74% for attempt weighting and 22.58% for group weighting. At forty attempts, the values are 24.09% and 22.48%, and the latter kit replaces its broad kitchen context with separate audio equipment. The two objectives therefore change relative ordering under reasonable restrictions on organizational recording volume. The thresholds alter both selection and the eligible evaluation population, so their contrasts describe different operating populations rather than repeated measurements of an unchanged population.

3.5. Stability during topic selection

Network-stratified resampling of the selection groups identifies a stable centre and uncertain peripheral topics. Lamp appears in 98.04% of attempt-weighted selections and 99.98% of group-weighted selections. Laptop appears in 69.62% and 85.52%, respectively. For organizational weighting, Mobile appears in 82.84% of draws, whereas Small kitchen item appears in 60.02%. Figure 6 displays the category inclusion frequencies. These values describe sensitivity to which eligible organizations are represented in the selection draw, conditional on the fixed networks and category list.

The last positions in the attempt-weighted kit are especially variable. Coffee maker appears in 52.92% of draws and Vacuum in 49.92%, while Hi-Fi separates appears in 86.94%. Complete recovery of the primary four-topic inventory occurs in only 19.28% of attempt-weighted draws. The group-weighted inventory recurs in 35.84%. Neither proportion should be interpreted as the probability that an inventory is correct. They indicate that the top-four boundary is sensitive to the organizational mix, even though the collection contains many individual attempts.

Stability is also affected by where preparation boundaries are drawn. An inclusion frequency concerns membership among four selected categories, rather than the rank of a category considered in isolation. Two categories with similar scores can alternate frequently even when neither score varies dramatically. Conversely, a category can retain membership while its internal rank changes. Reporting complete-set recovery alongside individual inclusion

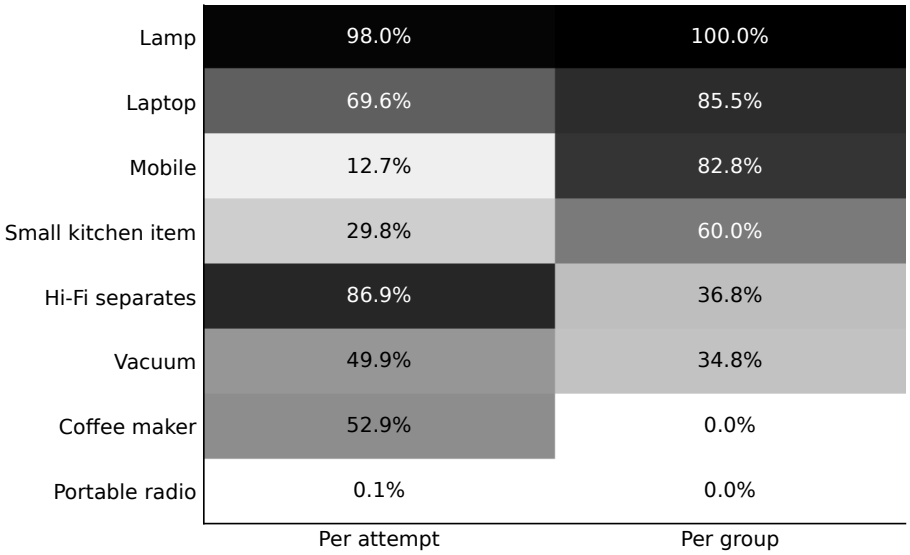


Figure 6: Sensitivity of topic membership to the selection groups.

frequencies captures both aspects. The present figures therefore support a distinction between a durable shared context and a contested final slot, rather than suggesting that all four entries possess the same evidential strength.

The distinction between the number of attempts and the number of operational groups explains why a large total cannot eliminate selection variability. Recording thousands of objects at a small number of specialized sites can make one category prominent without establishing that it is common across sites. Resampling complete groups allows that specialization to influence the shortlist as a block. Resampling attempts independently would disperse the association and could make selection appear artificially stable. The analysis therefore uses the organizational structure that is directly observable, while acknowledging that additional dependence may remain inside networks.

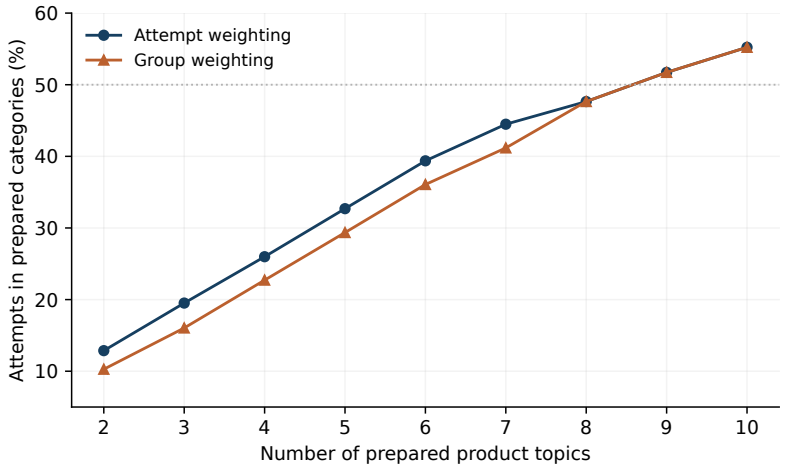
This finding has a practical implication for equipment preparation. A team can prepare the common lamp context with relatively strong support across the resampling exercise while treating marginal category choices as candidates for host discussion. It should not translate an inclusion percentage into a claim about educational effectiveness. The frequency concerns the topic's position in a planning calculation; effectiveness would depend on the chosen object, facilitation, participant interest and the evidence of understanding. Those factors require observation during delivery and cannot be inferred from the recurrence of a category label.

3.6. Kit size and the limits of four-topic preparation

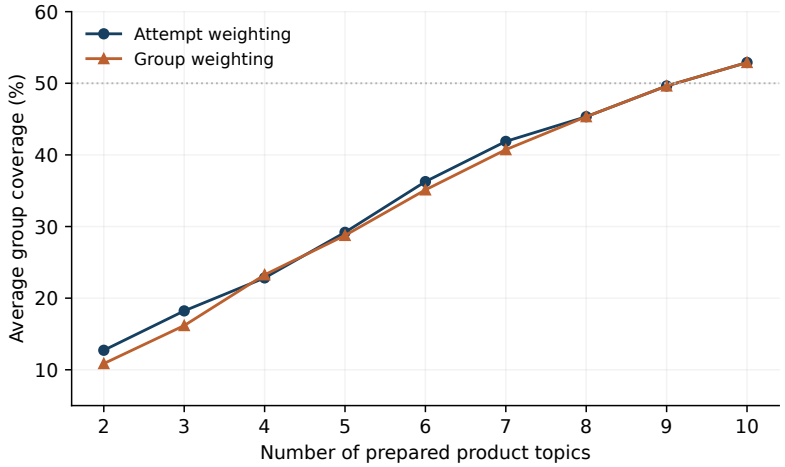
The effect of representation depends on the preparation budget. Figure 7 follows both rules from two to ten topics. At two topics, attempt weighting selects laptops and lamps, while organizational weighting selects lamps and mobiles. Their average group coverages are 12.72% and 10.87%. At four topics, the small organizational advantage appears, but it reverses again at five, six and seven topics. Thus, a conclusion about representation at one budget cannot be generalized to every portable kit without checking how the selected categories accumulate.

At eight topics, the selections coincide: lamps, laptops, separate audio equipment, coffee makers, vacuums, small kitchen appliances, portable radios and mobiles. Both kits then cover 47.65% of attempts and 45.34% of average group demand. Adding power tools increases average group coverage to 49.62%; adding food processors brings it to 52.90%. Within the examined range, ten topics are therefore required to exceed one-half of average group demand. This is a statement about the recorded categories, not an estimate that ten physical objects will educate one-half of visitors.

Convergence at a larger budget provides a useful qualification to the representation debate. The disagreement is strongest near a restrictive selection boundary, where closely competing contexts cannot all be included. Expanding preparation eventually accommodates the competing categories and removes the difference in membership. That



(a) Coverage of recorded attempts



(b) Coverage averaged across groups

Figure 7: Consequences of expanding preparation from two to ten topics.

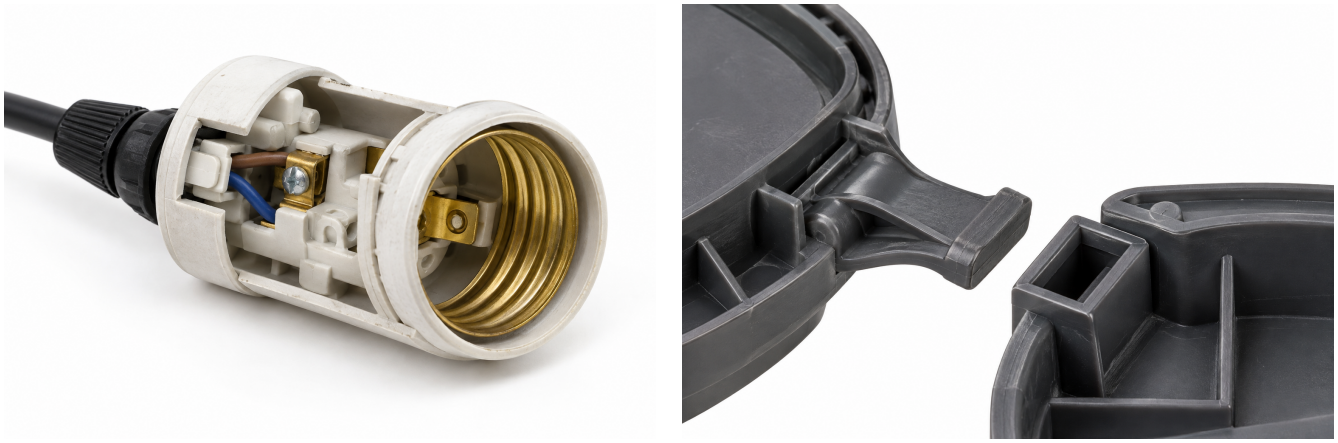
mathematical convergence does not establish affordability or ease of transport. Some product contexts require specialist access, documentation or equipment, and the equal-slot assumption ignores those demands. A costed inventory could select a different combination even if it pursued the same coverage objective.

Technical repairability indicators combine assembly complexity, instructions, replacement-part availability and related provisions [39]. The current category budget intentionally does not collapse those factors into a score for public learning. Its restriction makes the calculation transparent but also limits the scope of any management recommendation. A team may reasonably prefer fewer carefully prepared objects over a larger set that produces brief or inaccurate explanations. The kit-size curve describes the topical opportunity lost or gained under that choice; it cannot determine the educational depth that should be traded for breadth.

4. MATERIALS OUTREACH IN PRACTICE

4.1. Object-specific explanation and participant choice

A host-adjustable kit can use the calculated shortlist to organize preparation without presenting every selected product as a diagnosed failure. The materials question should be tied to the actual assembly that participants can inspect. A lamp holder offers a way to distinguish the roles of an electrically conductive contact, an insulating support and a cable restraint. A vacuum housing can support a discussion of how a flexible latch transmits force and permits access. Figure 8 illustrates those ordinary construction contexts without assigning an unverified material grade or claiming that either assembly was inspected during the recorded events.



(a) Lamp-holder functions (b) Housing access

Figure 8: Object contexts for materials explanation.

The two views serve different explanatory purposes. The lamp context concerns the allocation of functions across adjacent components: electrical continuity, insulation, positioning and restraint. The housing context concerns the relationship between shape, deformation and access. Neither requires a dramatic fracture photograph to be educationally useful. An intact connection can reveal a material requirement more clearly than an unexplained damaged object. Before delivery, the facilitator must verify the selected example's construction and decide which statements can be supported by direct inspection, manufacturer information or a separately documented material sample.

The distinction between an object label and a supported explanation is important when products are discussed across groups. A mobile-phone category can include different fastening arrangements, access paths and replacement procedures. A kitchen category can include several thermal and mechanical functions. Preparing a category therefore means establishing a specific question and an inspectable example, not assuming that every product under that label shares the same mechanism. Disassembly research provides tools for examining access and operations, while diagnostic research warns that symptom descriptions alone may not identify the component requiring intervention [27, 28].

The educator's explanation should connect an observed feature to a bounded inference. A visible retaining screw supports a statement about one fastening operation, but it does not establish the total time needed to reach a component deeper inside the product. A flexible latch suggests a designed deformation path, but its appearance alone cannot establish fatigue life, allowable strain or the resin's resistance to aging. A metallic contact supports discussion of the separation between conductive and insulating functions, while naming the contact alloy requires additional evidence. These distinctions give participants a technically useful habit: identify what the object shows directly before deciding what further information would be needed to justify a stronger conclusion.

A participant should be able to choose whether to join the explanation. A repair service can remain useful when someone has neither time nor interest in discussing material selection. Conversely, an unsuccessful repair can still support careful reasoning about why access, joining or an unavailable component limits intervention. Research on the social and political meanings of repair cautions against reducing its value to an instrumental account of recovered function.[40] Outreach can respect those meanings by allowing a visitor's own question to shape the technical conversation rather than requiring every encounter to follow the same instructional script.

4.2. Preparation agreements and educational assessment

The evidence supports a preparation agreement between the outreach team and the host. The team should identify which beneficiaries its weighting rule represents, and the host should identify which product contexts and participation conditions deserve local priority. The agreement can retain common contexts while permitting substitutions. Table 3 specifies the information needed for that discussion: a product context, the material function to examine,

the evidence supporting the object description and the participant action that would make understanding visible. The table describes a delivery specification, not an activity already evaluated at the historical events.

Table 3: A delivery specification for object-specific materials discussion.

Context	Material function or decision	Supporting evidence needed	Participant action
Lamp holder	Contact, insulation and restraint	Verified construction of a disconnected example	Identify distinct functions and explain why one component cannot replace another
Computer or mobile	Access through detachable and bonded joints	Device-specific disassembly information and an inspectable joint	Explain how joining affects access to a replaceable component
Vacuum housing	Retention, flexure and service access	Actual connection geometry and documented assembly method	Identify a release path and distinguish access from material durability
Coffee maker or kitchen appliance	Sealing, joining and thermal exposure	Identified component and documented operating requirements	State a required property and the evidence needed to justify material selection

Educational assessment should remain separate from category coverage and repair completion. A short explanation task can ask a participant to compare two fastening arrangements, identify which permits access and justify the answer through the joint's geometry or material function. A response that merely names a component differs from one that connects its role to a physical requirement. The assessment should allow spoken explanation, pointing or another appropriate form of response, so that success is not determined mainly by confidence with technical vocabulary. These choices make the learning claim observable without assuming that a visitor wants a formal test.

Evaluation of science outreach distinguishes several possible outcomes, including changes in critical thinking, practical investigation and engagement [41]. Findings from active learning in undergraduate science further show that instructional design and participation conditions can affect who benefits [42]. Those findings justify careful attention to facilitation, but their effect sizes cannot be transferred to a repair event. The population, duration and form of participation differ. A materials-outreach evaluation must establish its own outcome definitions and comparison conditions rather than borrowing an educational success claim from a related setting.

An assessment can make that habit explicit without requiring lengthy written answers. The facilitator can record whether the participant identifies a relevant feature, connects it to a function and states a condition under which the explanation might fail. These observations distinguish recognition from causal explanation and recognition of uncertainty. They should remain separate entries until their interpretation has been checked with participants and independent assessors. Summing them immediately into an apparently precise score would imply a validated scale that the present work has not established. Such preparation should also document the prompt and the assistance given, because an answer produced after substantial coaching is different evidence from an explanation offered independently.

A prospective comparison could assign host organizations or events to a fixed shared kit or to the same kit permitting a documented local substitution. The primary outcome could be a materials-explanation score defined before delivery, with participation, preparation time and topic correspondence treated separately. Allocation at the organizational or event level would match how the intervention is delivered and reduce contamination between visitors attending the same activity. Outcome variability and within-event association would need to be estimated before choosing the number of events; repair-attempt counts cannot supply those educational quantities.

For host organizations, local adjustment should be recorded as a decision rather than treated as an exception requiring statistical justification after the event. A host may prioritize a less frequent object because its visitors

repeatedly ask about it or because a facilitator can give a particularly well-supported explanation. Those reasons are legitimate educational inputs, but they should not be relabelled as quantities measured by the repair records. Recording the reason preserves the distinction between a category-based recommendation and an exercised professional judgment, allowing later evaluation to examine which choices actually supported worthwhile participant engagement.

The strategic value of the present analysis is therefore procedural and bounded. It makes visible whose observations determine preparation, how topic membership responds to that choice and whether the preference survives another year. The procedure leaves room for expertise and local negotiation because the recorded categories cannot capture every worthwhile question. Its strongest result is not a universal kit but the demonstration that apparently minor arithmetic choices can materially alter the products around which an outreach enterprise organizes its technical explanations.

5. CONCLUSIONS

Choosing the unit of representation changes which product contexts receive preparation in materials outreach. Equal influence for repair groups replaces two of the four topics selected from pooled attempts, but its apparent organizational advantage largely disappears in the next year and differs sharply between networks. Selection resampling reveals uncertainty near the four-topic boundary, while broader preparation eventually accommodates the competing categories. Both rules select the same eight contexts, and ten topics are needed to exceed one-half of average group demand in the examined records. A defensible outreach enterprise should therefore specify its intended beneficiary, retain local authority over substitutions and evaluate understanding independently of repair completion. Product-category arithmetic can organize preparation; it cannot establish material identity, diagnose a failure or demonstrate that learning occurred.

Conflict of Interests

The author declares that they have no conflicts of interest.

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Data Availability

The data supporting the findings of this study are included within the manuscript.

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