

Appendix C: Practitioners vs. scientists—Full comparison

We compared French practitioners’ responses with the scientist sample from Altay, Berriche, Heuer, et al. (2023; hereafter Altay et al.). Given differences in instruments, response scales, survey language, populations, and timing between the two studies, we treat this comparison as descriptive and exploratory rather than confirmatory. Because the two instruments were not identical, we restricted the comparison to six items with close conceptual matching. Four additional items were excluded because the constructs do not align closely enough: “political or identity-based motivations” conflates Altay et al.’s separate identity, partisanship, and motivated-reasoning items; “low skills for verifying information” does not map cleanly onto “lack of digital or media literacy”; our platform-algorithm item is narrower than Altay et al.’s general social-media item; and our fact-check item differs in framing from Altay et al.’s intervention-effectiveness measure. Because the Altay et al. survey used 7-point scales while ours used 5-point scales, scientist responses were linearly rescaled to a 1–5 range using the formula: $1 + ((x - 1) \times 4/6)$ (Colman et al., 1997). One item required reverse-coding: Altay et al. asked whether people encounter more opposing viewpoints online than offline, whereas our instrument asked the logical inverse (offline than online); we reverse-coded the Altay et al. mean ($6 - M$ on the rescaled 1–5 scale) so that higher scores on both instruments indicate stronger endorsement of offline exposure. Effect sizes are Cohen’s d with 95% confidence intervals (CIs); p-values are two-tailed independent-samples t-tests corrected using the Benjamini–Hochberg (BH) procedure at $\alpha = .05$ across the six admissible tests. Altay et al. report a final analytic sample of $N = 150$; the publicly released dataset contains 148 cases, of which 143 satisfy a strict completeness criterion (Qualtrics Progress = 100 and Finished = 1), and we use this $n = 143$ subset for all cross-study comparisons reported below. As a sensitivity check, applying the BH correction across all ten mapped items (including the four excluded for construct mismatch, all of which show small, non-significant differences, absolute $d \leq 0.22$) leaves the same four items below the corrected .05 threshold (largest adjusted $p = .018$).

Four caveats apply. First, wording is not strictly equivalent across instruments: most notably, our 2016-election item asks whether disinformation played an “important role,” whereas Altay et al.’s asks whether it played a “decisive role in determining the outcome,” a stricter claim; the belief-driver block also uses intensity anchors in French while Altay et al. used agreement anchors in English, a difference that linear rescaling cannot remove. Second, for the reverse-coded item, disagreement with the online-exposure statement is not strictly equivalent to agreement with its inverse, and acquiescence bias operates in opposite directions across the two framings; the $d = 0.76$ reported for item 3 below is therefore best read as an upper bound. Third, the surveys were fielded roughly three years apart (2022 vs. September 2025–February 2026), an interval spanning the 2024 U.S. election and new evidence on electoral effects (Budak et al., 2024); responses to the 2016 item in particular may have shifted in both populations. Fourth, because academic researchers were excluded from our sample by design, the practitioner–scientist contrasts cannot be attributed to professional status alone; they may equally reflect the instrument, population, and timing differences noted above. These caveats reinforce the descriptive reading of the contrasts reported below.

C1. Items showing differences that remain after multiple-testing correction

1. Disinformation played an important role in Trump's 2016 election (Mechanisms/impact)

Practitioners: $M = 3.98$, $SD = 0.98$, $n = 102$.

Scientists: $M = 2.98$, $SD = 1.21$, $n = 143$.

Cohen's $d = 0.89$ [95% CI: 0.63, 1.16] (large), adjusted $p < .001$.

Practitioners rated the role of disinformation in the 2016 U.S. election substantially higher. This may reflect practitioners' operational experience tracking campaign attribution and high-stakes electoral interference, versus scientists' awareness of the weak causal evidence linking disinformation exposure to voting behavior; the wording asymmetry ("important" vs. "decisive") and the timing gap between the surveys also plausibly contribute to the size of this contrast.

2. Lack of trust in institutions or experts (Belief drivers)

Practitioners: $M = 3.81$, $SD = 0.84$, $n = 99$.

Scientists: $M = 4.18$, $SD = 0.77$, $n = 143$.

Cohen's $d = -0.46$ [95% CI: -0.72, -0.20] (medium), adjusted $p = .001$.

Scientists place more weight ($d = -0.46$) on institutional distrust as a driver of belief in disinformation. This aligns with the academic literature emphasizing trust as foundational to susceptibility (Ejaz et al., 2025; Humprecht et al., 2020). Practitioners, embedded within institutions, may underestimate the role that their own organizations' legitimacy deficits play in sustaining demand for disinformation.

3. People encounter opposing views more often offline than online (Mechanisms/impact)

Practitioners: $M = 3.20$, $SD = 1.12$, $n = 96$.

Scientists (reverse-coded): $M = 2.42$, $SD = 0.95$, $n = 143$.

Cohen's $d = 0.76$ [95% CI: 0.50, 1.03] (medium-to-large), adjusted $p < .001$.

After reverse-coding to align item direction (Altay et al. ask whether people encounter opposing views more often online than offline; our survey asks whether they encounter them more often offline than online), practitioners endorse the offline-exposure proposition substantially more than scientists. Practitioners' daily focus on online platform monitoring may lead them to overweight the digital environment and correspondingly see offline counter-exposure as relatively greater, whereas scientists, drawing on population-level evidence showing echo chambers are empirically overstated, give a more tempered assessment.

4. Limited reflection, reliance on intuition rather than analysis (Belief drivers)

Practitioners: $M = 3.80$, $SD = 0.93$, $n = 99$.

Scientists: $M = 3.46$, $SD = 0.98$, $n = 143$.

Cohen's $d = 0.35$ [95% CI: 0.09, 0.61] (small), adjusted $p = .011$.

Practitioners place somewhat more weight on cognitive laziness as a driver of belief. This may reflect a residual deficit-model framing among practitioners: a tendency to attribute disinformation belief to individual cognitive shortcomings rather than structural or contextual factors.

C2. Items showing no reliable difference after correction

Two items showed no reliable difference after BH correction (both adjusted $p > .05$):

- **Confirmation bias and selective exposure:**
Practitioners: $M = 4.36$, $SD = 0.69$; Scientists: $M = 4.19$, $SD = 0.74$; $d = 0.24$ [95% CI: $-0.02, 0.49$], adjusted $p = .082$.
- **Repetition that makes information familiar and ‘true’:**
Practitioners: $M = 4.03$, $SD = 0.71$; Scientists: $M = 3.96$, $SD = 0.81$; $d = 0.09$ [95% CI: $-0.17, 0.35$], adjusted $p = .478$.

C3. Summary

Of the six admissible items, four showed differences that remain after BH correction. The two largest effects were in opposing directions: practitioners attribute far more weight to the Trump-election item ($d = 0.89$) and to offline exposure exceeding online exposure ($d = 0.76$, after reverse-coding), while scientists place more weight on institutional distrust ($d = -0.46$). Practitioners also rate limited reflection higher ($d = 0.35$). The two remaining items (confirmation bias and repetition) showed no reliable difference, suggesting shared ground on core cognitive mechanisms. Overall, the comparison reveals selective divergence on politically salient and epistemically contested items, set against a baseline of agreement on well-established psychological drivers. An alternative reading is that divergence merely tracks residual wording mismatch; the institutional-distrust item, which is closely matched across instruments yet shows a clear difference ($d = -0.46$), argues against wording asymmetry as a full explanation, although it cannot be ruled out for the 2016-election and opposing-views items.