Once and Again: Repeated viewing affects judgments of spontaneity and preparation

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Abstract

Reality is fleeting, and any moment can only be experienced once. Re-watching a video, on the other hand, allows people to observe the exact same moment in time repeatedly. We propose that people apply their understanding of repetition in the real world to the replay context, failing to fully distinguish behavior that they merely *observe* again (through video replay) from that behavior being *performed* again in the exact same way. Nine experiments (N = 9,580) support this idea across a broad assortment of stimuli that includes auditions, dances, commercials, and public speeches. We demonstrate that re-watching makes a videotaped behavior appear more rehearsed and less spontaneous, as if the actor in the replayed video were simply precisely repeating the same set of actions. We rule out alternate explanations including repetition increasing accuracy of judgments, mere exposure leading to a positivity bias, and experimenter demand effects. These findings build on an influential literature showing that incidental video features like perspective or slow motion can meaningfully change how people judge the action of the video. Video re-watching may inadvertently shape judgments in contexts ranging from mundane to consequential. To understand how a video is going to influence its viewer, one will need to consider not only its content, but also how often it is viewed.

Statement of Significance

Video recordings are a primary source of information, communications, and entertainment, and for all those purposes, they are frequently watched more than once. Replays are necessarily a departure from reality, in which every experience is somewhat unique. How does the replay experience change the inferences people draw about real life?

We demonstrate that video replay makes people in the videos seem more rehearsed and less spontaneous. Viewers seem to interpret actors in replayed videos as though reenacting the same action again. This bias influences judgments in a broad array of domains, with replays making some actions seem better prepared and others less authentic. These findings are of practical importance for audiences as varied as business managers, policymakers, and content creators.

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Human experience is necessarily limited to our direct observations and perception. Video recordings, however, enable people to see and hear completely different observations. Moreover, they transcend the present-bound nature of real-world perception, allowing people to observe a unique moment in time more than once through replay. For those reasons, video recordings are a widespread conduit for information, communication, and entertainment. For example, most Americans get their news from video [1], and every day, more videos are watched on YouTube and Facebook alone than there are people in the world [2,3]. Contributing to this video consumption, people often watch videos more than once, whether because they are looped on television channels, shared on social media, or intentionally re-watched because the viewer finds them particularly informative, important, or entertaining.

Despite providing (repeated) access to moments that the viewer was not present for, videos are merely a close facsimile of real life. As such, viewer inferences may not be accurate, either. Even when intellectually aware that a given video clip is an alteration of unvarnished reality, viewers may still fail to fully account for their initial, intuitive impressions when making judgments. Pioneering research on this topic found that viewers failed to fully account for the distortion introduced by playing videos in slow motion [4]. Rather, viewers inferred that a sequence of events had taken longer to play out when they watched those events in slow motion, instead of regular speed. When videos of interpersonal transgressions (e.g., illegal football tackles) are played in slow motion, the actions seem more intentional, and the actors, therefore, seem more culpable. Relatedly, the visual perspective of a video may have a similar effect. Officers appeared to be less involved in police activity when it was shown via their body cam, which removes the officer from the scene, compared to a dash cam, which makes them clearly

visually present [5]. In other words, not seeing the actor in a scene reduced her perceived involvement.

Together, these slow-motion and body cam findings suggest that viewers may treat features of the video-watching experience, like replay speed and visual perspective, as though they are features of the actual event. We build on this foundation to make an analogous prediction: Watching the same recorded event more than once imparts an intuitive impression that the actors depicted in the video are simply repeating their actions or behavior—and repeating them in precisely the same way. In other words, we propose a "replay bias", wherein people intuitively feel that when a video is replayed, the actor is performing the exact same action again.

This hypothesis draws from a phenomenon underlying numerous perceptual and cognitive illusions in which the perceptual or cognitive system accurately interprets stimuli in a frequently encountered context, but then overapplies that interpretation to contexts where it should not [6,7]. In real life, the only way to see an action repeated is if that action truly occurs more than once. Appropriately, such repetition eliminates the uniqueness and reduces the perceived spontaneity of the actor's behavior, which may have consequences for interpersonal judgment. Gershon and Smith (2020) [8], for example, found that comedians delivering the same joke on different occasions seem inauthentic because they falsely present their performance as spontaneous; the same was true for other self-repetitions across multiple contexts (i.e., politics, tour guiding, interviews).

We propose that viewers inappropriately apply this understanding of *actual* repeated behavior to the apparent repetition of a single behavior afforded by replay. Therefore, just like when behavior in the real world is carried out more than once, a single video-taped moment loses

its uniqueness and spontaneity when it is replayed. Moreover, exact repetition is impossible—no two moments are identical—but in the real world, if an actor's repeated behavior nonetheless *appears* identical to their original, it is likely to be highly controlled, practiced, or deliberate. For example, a dancer who exactly replicates the same series of movements from performance to performance or a speaker who delivers identical monologues must have practiced and prepared extensively. Thus, if viewers do not fully account for the fact that the video captured a unique moment in time, and that the person in a repeated video is not simply repeating the same actions identically, they may infer that those actions are controlled and deliberate, potentially through extensive rehearsal.

The effect that this inference has on ancillary judgments should depend on the context. For example, in situations where uncontrolled, unplanned, or spontaneous behavior is viewed positively, repeated viewing might lead to negative judgments (e.g., the actor may appear less genuine and more contrived). Conversely, in situations in which controlled, planned, or rehearsed behavior is viewed positively, repeated viewing might make evaluations of the actor more positive (e.g., the actor may appear more prepared). We explore this bias and its consequences in 9 studies. All studies were pre-registered on aspredicted.com. The pre-registration, stimuli, data, and analysis code for each study is available at https://researchbox.org/151&PEER_REVIEW_passcode=UDTBCA.

Study 1

Study 1 provided an initial test of our hypothesis that when re-watching a video, the events in the video feel like they are simply happening again in the exact same way. If this is the case, then repeated viewing should make the videotaped behavior seem more rehearsed and less spontaneous. To test this prediction, participants saw four unique video clips (A through D) of

homemade audition tapes for the reality TV show Survivor. Videos were from the video-sharing platform YouTube and trimmed to ~8-11 s. Each video depicted the contestant introducing themselves. Either Video A or Video B was selected as the target video for each participant, and the other as the control. Participants viewed the target video three times and all other videos once, and then made judgments about the actors in the target and control videos, indicating how much they thought the contestant had planned and rehearsed what he would say in advance. Evaluating a closely related dimension, participants also rated how much effort he had put into making the tape. The basic experimental paradigm is illustrated in Figure 1.

As predicted, when a video was repeated rather than viewed once, participants rated the contestant as more rehearsed (*M*s = 5.26 vs. 4.66, $t(404.02) = 4.31$, $p < .001$) and putting in more effort ($M = 5.00$ vs. 4.68, $t(404.03) = 2.37$, $p = .018$).

Study 1 found that repeated viewing made audition tapes seem more rehearsed, providing initial support for our hypothesis. However, it is possible that these stimuli were unique in demonstrating the effect – for example, it may be specific to speech alone. Our next study worked to establish generalizability. Because Study 1 employed videos of individuals speaking, our next study used very different stimuli with no semantic or verbal content.

Study 2

Study 2 used short clips of complicated dance videos from TikTok, a popular video-sharing platform [9]. The design was identical to that of Study 1: Participants again saw a sequence of videos in which one video (either A or B) played three times and all other videos played once. They then evaluated the dancer in videos A and B, indicating how much they thought the dancer had practiced the dance in advance, and rating the dancer's skill level. If viewers fail to fully

account for the fact that they are merely *observing* the dance more than once—the dancer is not precisely repeating it—then the dancer should seem both more practiced and more skilled.

The results confirm this prediction. When participants saw their video thrice rather than once, the dancer was perceived to have practiced the dance more (*M*s = 5.91 vs. 5.19, $t(263.00) = 7.58$, $p < .001$) and as being more skilled (*M*s = 4.86 vs. 4.46, $t(263.00) = 4.57$, $p < .001$).

Repeated viewings made aspiring Survivor contestants appear more rehearsed (in Study 1) and TikTok dancers appear more practiced (in Study 2). Although these two domains are substantially different, both involve performers aiming to entertain the viewer. Thus, Study 3 sought to examine the influence of repetition in a domain in which the performer does not seek to entertain, but rather to communicate somber sincerity.

Study 3

Study 3 tests whether this replay bias generalizes to a more serious context, using videos of CEOs delivering public apologies. To add variation in stimuli, videos A and B were drawn at random from a pool of five. Either A or B was randomly chosen to be repeated. For both videos, participants evaluated how much the CEO had prepared the apology ahead of time. They also evaluated two qualities that may be associated with greater preparation, at least in the context of issuing a public apology: how seriously the CEO took the incident and how likely the CEO would be to make changes within the company to prevent it from happening again.

As anticipated, when participants saw the apology three times (as opposed to once), they judged the CEO as having prepared more (*M*s = 5.41 vs. 5.16, $t(601.44) = 2.58$, $p = .010$); the CEO also appeared to have taken the incident more seriously (*M*s = 4.99 vs. 4.78, $t(599.85) = 2.58$, $p = .010$) and to be more likely to make changes to prevent the incident from happening again (*M*s = 4.92 vs. 4.74, $t(602.76) = 2.53$, $p = .012$).

Repeated viewing made the CEO seem like he took the incident more seriously and would be more likely to try to prevent it from recurring, in part because it made him appear to have prepared more. Exploratory analyses find that perceived preparation significantly predicted judgments of how seriously the CEO took the incident, $t(1110) = 3.19$, $p = .001$, and of how likely he is to make changes to prevent the incident from recurring, $t(918.66) = 3.14$, $p = .002$. Further, bootstrapping 5,000 samples yielded a significant indirect effect of number of viewings on perceived seriousness through perceived preparation, b = .019, 95% CI = [.004, .039]. When running the same model for change likelihood, the indirect effect was similarly significant, b = .017, 95% CI = [.004, .033]. Thus, this effect on preparation appears to impact important related judgments about the CEO's handling of the crisis—judgments that may influence a company's ability to recover from scandals and regain consumer trust.

Across the first three studies we have demonstrated that repeated viewing makes actors appear more rehearsed and prepared, and evaluated more positively on adjacent qualities like effort or seriousness. These findings align with our hypothesis that at least to some extent, viewers mistake the *apparent* repetition from video replay for actual repetition, and thus the lack of variability between replays makes the target seem more rehearsed. But our results are just as compatible with a very different explanation—that repeated viewings simply make any evaluation more positive. This alternative account is consistent with the mere exposure effect [10,11], wherein repeated exposure leads to a largely unmediated increase in positive affect. To help distinguish these two accounts, in Study 4 we sought stimuli where actors would be judged more *negatively* for rehearsing or controlling their behavior. This approach allows us to determine whether our results are better explained by our account or by a mere exposure account.

Study 4

To test our hypothesis against a mere exposure explanation, Study 4 examines behavior that will be judged more negatively if it is performed more than once. Participants watched clips of "unboxing" videos, a popular genre of YouTube that blurs the line between entertainment and commercial advertising. These videos feature someone voicing their first impressions while unpacking a product that they have supposedly never seen before. For example, Video A showed a young woman opening a subscription box and Video B showed a woman opening a "mystery box" from Amazon. In both cases, the person purportedly saw the product for the first time and reacted accordingly, expressing surprise and excitement. Such displays of emotion are supposed to be spontaneous and involuntary reactions. A person can only have one true first-time reaction to a stimulus, so if participants fail to fully distinguish replay from actual repetition, the replayed reaction may seem more staged and less authentic. Thus, we expected that repeated viewing would make these spontaneous reactions seem less authentic.

The anticipated effect emerged: When participants saw the unboxing video multiple times rather than once, they thought that the target's reaction to the product was less genuine (*M*s = 3.95 vs. 4.40, $t(492.02) = -5.02$, $p < .001$). These results provide further support for our account and directly challenge an explanation based on mere exposure.

Although Study 4 did not support a mere exposure account, another alternative possibility is that replay simply enables people to judge the actors' behavior more accurately. After all, the unboxing reactions may have been faked, the Survivor auditions heavily prepared, and so on. To rule out this explanation, we sought stimuli where we knew reactions were authentic, letting us compare participants' judgments to a known ground truth. We found these stimuli in a unique database of videos that had been developed with the explicit goal of capturing authentic emotional displays.

Study 5

To address the possibility that repeated viewing simply makes judgments more accurate, Study 5 replicates the previous study using reactions that were designed to be authentic. Stimuli were taken from the Berkeley Reactions to Affective Video Elicitors (BRAVE) database, which contains over 45,000 videos of people reacting to previously unseen emotionally evocative content [12]. We selected videos of surprised reactions. Importantly, these reactions should be genuine; the people in these videos were research participants who had no access to the stimuli ahead of time and no incentive to fake their reactions. If repeated viewing makes observers more accurate in their judgments, then these genuine expressions of surprise should seem *more* authentic when watched multiple times.

However, the results do not suggest that repeated viewing makes judgments more accurate. Instead, we again find that the target's surprised reaction was rated as less genuine when watched multiple times rather than once (*M*s = 4.09 vs. 4.36, $t(759.00) = -2.75$, $p = .006$).

Our next study aimed to make two conceptual advances. First, we examined our effects in a context that often incidentally exposes viewers to the same video more than once—commercial advertising. Second, we attempt to address possible concerns of experimenter demand. Participants may infer that if experimenters show them a video more than once, it must be "special". This account cannot explain all our results, since repeated viewing seems to make the videos appear more extreme in some studies (1, 2 and 3), but less extreme in others (4 and 5). Still, the best way to assess possible demand effects is to change what being "special" implies and examine whether responses change accordingly. The next study takes this approach. If our effect persists in the presence of those explicit communication norms, then a demand interpretation becomes less viable.

Study 6

Commercials often show people who are purportedly *not* paid actors interacting with or learning about a product, ostensibly for the first time; notable examples include Chevrolet's "real people, not actors" campaign and the "Pepsi Challenge". Study 6 examines whether repeated viewing makes these supposedly naïve, would-be customers appear less like "real people" and more like actors. This study used the same design as the previous studies, but to reduce experimenter demand, we varied the framing of the dependent measure. Participants either rated the probability that the target person was an actor, not a "real person", or the probability that they were a "real person," not an actor. If they infer that experimenters use repetition to convey that a target is "special", then participants should give higher ratings to repeated (vs. single) targets, regardless of frame. However, if our replay bias holds, then the repeated target should seem more like an actor, and not a "real person", in both conditions.

We subtracted scores in the "not actor" condition from 100 to place responses on the same scale (wherein higher values reflect a higher probability of being an actor). As predicted, when participants saw a commercial thrice rather than once, they thought that the people it featured were more likely to be actors (*M*s = 67.57 vs. *M* = 62.97, $F(1,1414.00) = 21.46$, $p < .001$). Those in the "actor" frame gave marginally higher ratings than those in the "not actor" frame (*M*s = 66.19 vs. 64.31, $F(1,1415.00) = 2.74$, $p = .098$). Importantly, our results do not support an explanation based on experimenter demand: There was no significant interaction between framing and repetition, $F(1,1414.00) = 0.002$, $p = .968$.

These results suggest, once more, that repeated viewing makes a target's behavior appear less spontaneous and more prepared, as if they were actually performing it again in each replay. However, all studies thus far have employed three notable design choices that may limit our

understanding of this effect. First, every study compared one viewing with three, leaving several open questions about the relationship between number of viewings and perceived preparation (e.g., are two viewings sufficient to produce our effect? Are three viewings the same as, say, five, or does video content seem more and more prepared with more viewings?). Second, because repetitions were separated by intervening "filler" videos, it is unclear whether the effect would emerge without them (i.e., if the repeated video replayed back-to-back). Third, participants always rated both a video that they saw once and a video that they saw repeatedly, raising questions about robustness (e.g., might this design produce an implicit comparison between the two videos, thereby exaggerating any perceived differences? Would we continue to observe the effect in a purely between-subject design?) To answer these questions, the next study uses a between-subject design with back-to-back replay that examines the impact of different amounts of repeated viewing.

Study 7

Study 7 investigates whether the *number* of repetitions affects perceptions of preparedness. Each participant watched the same video, taken from an improvisational ("improv") theater skit—which is, by definition, entirely unprepared—and evaluated the extent to which the performance seemed improvised rather than scripted. Critically, we manipulated the number of viewings between-subject: Each participant watched the video anywhere from one to six times in a row. These design choices allow us to probe the robustness of our effect, examining whether it holds without filler videos and for different numbers of repetition.

The results reveal that a single replay affected ratings to the same extent as multiple. The actor's line was rated as less improvised (more scripted) for participants who watched the video

twice (M = 3.79) rather than once (M = 4.14, $t(920) = -2.86$, $p = .004$). The same was true for those who watched it three (M = 3.84, $t(875) = -2.29$, $p = .02$), four (M = 3.78, $t(904) = -2.85$, $p = .005$), five (M = 3.58, $t(933) = -4.53$, $p < .001$), or six times (M = 3.87, $t(909) = -2.13$, $p = .033$). In short, any amount of repetition made the video content seem more scripted. Moreover, the five repetition conditions did not significantly differ from each other, $F(4, 2351) = 1.71$, $p = .145$.

These results again suggest that viewers apply their real-world understanding of repetition in the replay context. In the real world, regardless of whether a person performs precisely the same behavior two, three, or five times, that behavior typically appears more controlled and deliberate than if it is done once. It is also intuitive, at least in the context studied here, that one repetition would not differ from many. Consider the inferences one might make when watching an actor *actually* repeat a line word-for word with precisely the same inflection, cadence, facial expression, body movements, and so on. One repetition is likely sufficient to give viewers the impression that the performance was scripted, not improvised, and watching the actor deliver that same performance a third or fourth time is unlikely to change this impression.

Besides adding to our general understanding of the phenomenon, these results demonstrate that the effect occurs between-subjects, with back-to-back replay, and for varying amounts of repetition. Documenting our effect under these conditions supports the robustness of the phenomenon, but all our studies still share a potentially limiting feature. Specifically, repetition occurs for no apparent reason—participants are merely told that one of the videos "may be shown more than once, so that you can examine it more closely." While we rule out experimenter demand explaining our effect in Study 6, this form of repeated exposure is nonetheless unnatural. In the next study, all repeated exposures are incidental; participants must play and replay the clip in service of answering various questions. This design makes the

manipulation incidental to the reasons for the repetition. If the effect persists under such circumstances, we can differentiate whether the replay bias comes from mere repetition (as we have hypothesized) results from the peculiarity of the presentation.

Study 8 also makes a notable stimulus change: Participants listened to audio clips, with no accompanying video. The use of audio clips allows us to test the scope of our effect, examining whether repeated listening is sufficient or whether there is something unique about the visual aspect of repeated viewing.

Study 8

In Study 8, continuing our focus on authenticity, we selected audio clips of spontaneous (real) and volitional (fake) laughter. There is some evidence that laughs from the same person sound more similar when they are fake than when they are genuine; that is, genuine laughs are more variable [13]. Thus, if a listener hears the same laugh repeated and, at least to some extent, interprets the repetition from replay as true repetition, the complete lack of variability between replayed laughs may make them sound fake.

We selected our stimuli from unrelated research on laughter authenticity [14]. In that research, spontaneous laughs were recorded from different speakers while they were talking with friends, and volitional laughs were recorded by instructing research participants to laugh into a microphone (i.e., “Now, laugh.”). All laughs came from female university students. Participants listened to pairs of laughter recordings and were asked to identify whether the two laughs were the same. Over the course of the experiment, participants heard one laugh repeated several times. Participants then evaluated the authenticity of either that laugh (repeated condition) or a laugh that they had not heard before (single condition). Thus, number of viewings was manipulated between-subject, as was laughter type; all laughs were either spontaneous or volitional.

Participants reliably distinguished spontaneous laughter from volitional laughter, rating the former to be more genuine (*M*s = 4.12 vs. 3.10, $F(1,1286) = 111.71$, $p < .001$). But despite their ability to detect that difference, judgments were also influenced by repetition: Participants thought that the laughter was less genuine when they heard it multiple times compared to once (*M*s = 3.39 vs. 3.86, $F(1,1286) = 24.91$, $p < .001$). In fact, the effect of repetition was nearly half as large as the effect of true spontaneity (i.e., .47 vs. 1.02). Moreover, we did not observe an interaction between laughter type and repetition, $F(1,1286) = 1.33$, $p = .249$. These results reveal that repetition affected real and fake laughs to a similar extent, and more broadly indicate that repeated listening produces the same effect as repeated viewing.

Study 8 furthers our understanding in several ways. It shows that incidental repetition is sufficient to produce the effect, as is the audio component of a recorded behavior. Study 8 also reconfirms the conclusion from Study 5 that repetition does not simply enhance accuracy. And, as with the previous studies, repeated exposure affected perceptions of behaviors for which authenticity requires total spontaneity.

Our final study examines a context where reducing spontaneity may help credibility. Specifically, we investigate how repeated viewing affects judgments of ostensibly factual statements. We chose those that cite statistics, because absent a lucky guess, numerical statements are false if they are generated off-the-cuff. Thus, given the results of the previous studies, we predict that repeated viewing reduces the perception that a factual statement had been made up or fabricated on the spot. The reduced spontaneity should also make repeated statistics appear more credible. This study extends the effects found in our previous studies, where repeated viewing changed judgments of individuals or their actions-- here, we investigate if repeated viewing can change the plausible veracity of a fact.

Study 9

Our final study explores whether repeated viewing makes purportedly factual statements—which are false if spontaneously generated—seem truer. To this end, participants saw two different videos of politicians citing statistics during a persuasive speech. One video featured United States President Donald Trump and the other, United States Representative Tim Ryan. One politician's video was shown thrice and the other once, and videos were presented in random order. Participants evaluated the preparedness of each politician, indicating i) whether they thought he had memorized the statistic ahead of time, or had made it up on the spot, and ii) how credible they thought the statistic was.

As anticipated, repeated viewing made the statistic seem more likely to have been memorized, instead of made up spontaneously (*M*s = 4.83 vs. 4.59, $t(481.06) = 3.37$, $p < .001$). Similarly, the statistic seemed more credible after three viewings compared to one (*M*s = 4.12 vs. 3.97, $t(481.05) = 2.99$, $p = .003$).

The latter result is consistent with the "truth effect", whereby the more times a person evaluates or encounters a statement, the more they judge it to be true [e.g., 15,16]. However, our results for spontaneity cannot be explained by this phenomenon. If it uniformly increased the credibility of any information, repeated viewing should not make first-time reactions appear less authentic, but in multiple studies we find that it does (Studies 4-6, Study 8).

Our findings may have implications not only for judging truth, but for categorizing types of falsehood. Information that is made up spontaneously is almost always false. Therefore, information that viewers believe is true should not seem any more or less spontaneous when viewed repeatedly. However, information learned in advance is not necessarily true. False information can be fabricated spontaneously *or* memorized ahead of time. For example, whether

strategically or unwittingly, a politician might have planned to cite information that is incorrect or misleading. So, when viewers deem information to be false, repeated viewing should make that information seem more prepared and less spontaneous, because preparation need not imply veracity.

This mismatch may help explain why we still observe an effect of repeated viewing on judgments of spontaneity when looking solely at respondents who indicated that the statistic was "not credible at all". In an exploratory analysis, we found that although these individuals in both experimental conditions thought that the statistic was entirely false, those who saw the video thrice thought that the statistic seemed more memorized (and less spontaneous) than those who saw it once, $t(32.42) = 2.60$, $p = .014$. This may reflect the fact that there are different kinds of falsehoods—those that are uttered extemporaneously and those that are premeditated. Unsurprisingly, the same was not true for those who rated the statement as "extremely credible"; in this case, participants who saw the video once rated it to be just as memorized as those who saw it three times, $t(17.52) = -1.00$, $p = .333$.

General Discussion

In the modern world, recording, sharing, watching, and re-watching videos is easier than ever before. Along with unprecedented ease, the growing frequency of video consumption and replay underscores the importance of understanding how these behaviors affect viewer judgments. The present research examines the consequences of video re-watching for impressions of spontaneity and preparation. We find that re-watching videos creates a replay illusion: Actors in replays are judged as though they are reenacting the same behavior in exactly the same way. The lack of variability between apparent repetitions leaves viewers with the impression that the actors have spent more time preparing, and that their actions are less

spontaneous. We document this phenomenon for a wide range of video content, judgments, and experimental designs. Further, we systematically test and eliminate alternate accounts considering experimenter demand, mere exposure, or enhanced viewer accuracy.

Of course, it is not necessarily the case that these distortions will make people *less* accurate. In some contexts, individuals may systematically underestimate preparation after only a single viewing, and repeated viewing therefore brings their judgments closer to the truth. Indeed, re-watching is sometimes necessary for a full understanding, and future research may examine the tradeoffs between the distortions of the replay bias and the enhancements of that extra information. But in other contexts, repeated viewing may make perceivers less accurate; we observed this in Studies 5 and 8, where repetition made genuine expressions of emotion appear contrived. In those cases, repetition may be especially damaging. First, if viewers believe that re-watching improves the fidelity of their perceptions, they may not actively correct for its distortion, potentially even amplifying its effects. Second, in general, the most important videos are most likely to be re-watched. News channels replay essential video clips, and those are more likely to be shared (and re-encountered) on social media, or deliberately re-watched. Thus, given that viewers may be especially likely to re-watch important video clips, any replay bias is more likely to influence consequential videos than the inconsequential alternatives.

As the mélange of topics explored in the present paper suggests, repeated viewing may inadvertently shape a wide range of consequential judgments. Replays can make important speeches seem more thoughtful, measured, and thorough, attributes that might then influence judgments of the speaker. Indeed, Study 3 found that because repeated viewing made public apologies issued by CEOs seem more prepared, the CEO consequently appeared to have taken the incident more seriously. Moreover, to the extent that preparation signals quality, repeated

viewing may make behavior seem more intentional, controlled, and skillful; this is supported by Study 2. The opposite may be true in situations where preparation is viewed negatively. An accidentally articulated offensive remark may sound calculated and intentional on replay. Conversely, widespread replay of a public figure's winsome, off-the-cuff joke might dampen its charm, in part because the joke may seem scripted and the speaker, disingenuous. Importantly, any effects on public opinion may also impact a variety of associated support behaviors, such as voting, purchasing, and shareholding.

These results also speak directly to a certain type of commercial advertising. Companies may want to deprioritize repeated exposure to commercials that feature the supposed first-time reactions of "real people". The effectiveness of such commercials may depend in part on whether the consumer believes that the reactions are genuine, not contrived or scripted. In both traditional (i.e., product commercials, Study 6) and non-traditional advertising contexts (i.e., unboxing videos, Study 4), repeated viewing rendered reactions from "real people" less authentic. Thus, such commercials may be less compelling for consumers who watch them more than once.

In contrast, re-watching a video of a speaker citing facts and statistics—which are more likely to be true if learned ahead of time—made that information appear both more memorized and more credible (Study 9). Therefore, video replay may have consequences for public trust of information and misinformation. Overexposure may change viewer conclusions. For example, if a news station replays a video in which a speaker cites an inaccurate or questionable statistic, this repeated viewing may make the statistic seem more legitimate. Similarly, when a viewer believes that the speaker is telling a lie, repeated viewing may affect perceptions of whether the lie was made up on the spot or crafted in advance. This may have implications for judgments of culpability, as the premeditated versus spontaneous distinction is central to judgements of

criminal intent. For example, in cases requiring jurors to judge whether the actions of the accused were carefully practiced or spontaneous, video replay could have unwanted and potentially substantial consequences.

The accessibility of recorded audio and video content has placed people into a new world of social perceptions. Much as altered perspectives and slow motion seems to alter the judgments of viewers, so too does mere repetition. To understand how a video is going to influence its viewer, one will need to consider not only whether it is viewed, but whether it is viewed again.

Materials and Methods

Ethics Statement. All studies were approved by the Institutional Review Board at Haas School of Business, UC Berkeley.

Open Science Statement. All studies were pre-registered on aspredicted.com. The pre-registration, stimuli, data, and analysis code for each study is available at https://researchbox.org/151&PEER_REVIEW_passcode=UDTBCA.

Recruitment, Participants, and Exclusions. Participants were recruited from Amazon Mechanical Turk (Study 2), the Luth Research participant pool (Study 8), or Prolific Academic (Studies 1, 3, 4, 5, 6, 7, and 9). We limited recruitment to participants from the United States and who were fluent in English; the latter restriction was only available on Prolific Academic. Participants received between $0.16 and $.32 in compensation. The 9 studies reported in this paper involved 9,580 participants. Upon entering the study, participants in Studies 1-6 and 9 were told that they would see a series of videos, one of which would play more than once so that they could examine it more closely. Participants in Study 7 were told only that they would answer questions about a single video (and in the repetition conditions, that they would watch more than once). In Study 8, participants were simply told that they would hear clips of laughter and answer

questions about them. Our pre-registrations specified various exclusion criteria, typically asking participants to write what object was depicted in a picture and/or identify a video that they did not see as English comprehension and attention checks, respectively (see SI Materials and Methods). Across all studies, 12.6% of participants were excluded (Table S1). The key test for each study remains significant without exclusions (Table S2).

Materials. Unless noted otherwise, all stimuli were taken from YouTube and trimmed using Kapwing, a free online video editor.

Stimulus sampling. Any research program should be concerned about how experimental stimuli are chosen, and the ability to generalize any finding will depend in part on that process [17,18]. We tried to handle this issue in a handful of ways. First, across the nine experiments we report, we use 20 recordings, and those recordings are sampled from domains as diverse as public apologies, choreographed dances, actor auditions, television advertisements, and experimentally induced reactions. While this is not a random sample, we believe it is extensive enough that stimulus sampling issues are unlikely to drive our results. Second, in any given study we manipulate whether the participant experiences a stimulus just once or repeatedly, meaning that our critical manipulation is never confounded with stimulus selection. Finally, across the studies we develop and refine our predictions so that variation in stimuli is a meaningful component of our primary predictions and conclusions.

Analyses. Except for Studies 7 and 8, which were entirely between-subject, our pre-registered analysis plan for every study was to perform a mixed-effects regression that included random effects of participant and video clip; we used the lme4 package in R [19]. However, a few regressions showed an issue with singularity due to overfitting. Thus, following the prescription offered by Bar, Levy, Scheepers and Tily (2013) [20], the statistics in our paper were obtained

from the most complex model consistent with the experimental design, removing only the terms required to produce non-singular fit.[1] Note that all results hold with a standard linear regression that simply controls for variance explained by participant and video. Moreover, in each study, our basic effect remains significant (and marginally significant in Study 9) when we include no controls and just use paired-samples *t*-tests (see Table S2 for all alternate analyses).

Study 1. Participants (43% female, 55% male, 2% other/prefer not to answer, M_{age} = 29) watched four unique clips from Survivor audition tapes. We label these A through D. Participants were randomly assigned to see video A three times (and B once), or video B three times (and A once). Videos C and D were "fillers" preventing the repeated clip from playing back-to-back, and the order of the last two videos was counterbalanced such that either the repeated video or the single video came last. Thus, the viewing sequence was either A-C-A-D or B-C-B-D, followed by either A-B or B-A. Each clip played automatically and advanced to the next when finished.

Participants rated clips A and B in random order on two dimensions. On a 7-point scale, participants indicated how much they thought the contestant hopeful had planned and rehearsed what he was going to say in advance (1 = I don't think he rehearsed it at all, 7 = I think he rehearsed it a lot). They also rated how much effort he had put into making the tape (1 = I don't think he put much effort into making this, 7 = I think he put a lot of effort into making this).

Study 2. Participants (48% female, 50% male, 0.8% other/prefer not to answer, and 1.6% left the question blank, M_{age} = 34) saw four unique dance videos from Tik Tok in a sequence of either A-C-A-D-A-B or B-C-B-D-B-A. Videos were between 7 and 10 seconds in length. Participants

[1] Specifically, to resolve the singularity, Studies 2, 4, and 6 treat video as a fixed effect and participant as a random effect, while Study 1 treats participant as fixed and video as random. The remaining studies showed no issue with overfitting and thus performed the regression as pre-registered.

provided ratings on 7-point scales for preparation (1 = I don't think they prepared or practiced at all, 7 = I think they prepared and practiced a lot) and skill (1 = I think they are very bad at dancing, 7 = I think they are very good at dancing).

Study 3. Participants (39% female, 59% male, 1.7% other/prefer not to answer; 2.0% left the question blank, M_{age} = 35) saw a series of video clips of CEOs apologizing for some misdeed or wrongdoing. For example, one video showed the head of Boeing apologizing for recent mechanical issues and the resultant deadly crashes; another showed the CEO of Starbucks apologizing for racial discrimination. Clips were between 7 and 10 seconds. The design was identical to that of the previous studies, except videos A and B were randomly drawn from a larger pool of five videos. Thus, the viewing sequence was always A-C-A-D-A-B. Participants answered the following questions, each presented on a separate page and in random order, on a 7-point scale:

Preparedness. "For his apology, how much do you think the CEO prepared and practiced what he was going to say in advance?" (1 = I don't think he prepared or practiced at all, 7 = I think he prepared and practiced a lot).

Seriousness. "How seriously do you think he took the incident he is apologizing for?" (1 = I don't think he took it seriously at all, 7 = I think he took it very seriously).

Implementing change. "How likely is he to make changes within the company in order to prevent the incident from happening again?" (1 = Extremely unlikely, 7 = Extremely likely).

Study 4. Participants (48% female, 50% male, 0.2% other/prefer not to answer, and 1.2% left the question blank, M_{age} = 34) saw four unique video clips presented in a sequence of either A-C-A-D or B-C-B-D followed by either A-B or B-A. Video clips were between 5 and 11 seconds in

length. They were taken from YouTube "unboxing videos", which feature a person removing a product from its packaging for the first time. Video A showed a young woman opening a subscription box of Pusheen-themed items[2], and Video B showed a woman opening an Amazon "mystery box" of Christmas lights. Filler videos C and D respectively featured a woman unboxing an Apple computer monitor and a man unboxing a PlayStation. In addition, videos began automatically but did not auto advance, as they had in previous studies. Rather, participants had to press the "next" arrow, which only appeared after the clip had finished playing, to proceed to the next clip. This requirement better ensured that participants would attend to each video in the sequence. Ratings were provided on a 7-point scale for authenticity (1 = I think her reaction was completely fake, 7 = I think her reaction was completely genuine).

Study 5. Study 5 used the same design and dependent variable as Study 4 but different stimuli. Participants (54% female, 44% male, 0.6% other/prefer not to answer, and 1.2% left the question blank, $M_{age} = 34$) saw videos of an induced surprise reaction that had been elicited in an experimental context. These stimuli were obtained from the Berkeley Reactions to Affective Video Elicitors (BRAVE) database. Videos were between 1 and 6 seconds in length.

Study 6. Participants (45% female, 54% male, 1.0% other/prefer not to answer, $M_{age} = 31$) saw clips of commercials that featured surprised reactions from ostensibly naïve customers interacting with a product. For example, in a commercial for Chevrolet, a group of people learn that the brand-anonymized car that they thought was a luxury vehicle is actually a Malibu. Similarly, in a commercial for Suave haircare, a woman discovers that the product she has been using is Suave and not a high-end alternative. These two videos were clips A and B for this

[2] Pusheen is a popular cartoon cat.

study, while clips from a Chevy Cruise Hatch commercial and a "Febreze smell test" commercial acted as filler videos C and D. Videos were between 3 and 10 seconds in length.

We manipulated the framing of the dependent measure between-subject. In the "actors" frame, participants answered "What is the probability that the people in this video are actually actors, not 'real people'?" on a 101-point slider scale (0 = They are definitely *not* actors, 100 = They are definitely actors). Participants given the "not actors" frame answered "What is the probability that the people in this video are actually 'real people', not actors"? on the same scale with reversed endpoints (0 = They are definitely actors, 100 = They are definitely *not* actors). To put responses on the same scale, ratings in the "not actors" condition were subtracted from 100.

Study 7. The video was 2 seconds long and showed a man talking animatedly while performing an improvisational ("improv") skit onstage for an audience. Participants (47% female, 50% male, 1.0% other/prefer not to answer, $M_{age} = 33$) were not told that the performance was improvised; rather, it was introduced as a "satirical skit at a community theater." They saw this clip anywhere from one to six times in a row; in the repetition conditions, the video played back-to-back with no "filler" videos. Participants answered "Do you think that the actor's line was improvised or scripted?" on a 7-point scale (1 = I think it was scripted, 7 = I think it was improvised).

Study 8. Stimuli were 1-second audio clips of laughter. Participants answered a series of three questions designed to manipulate their exposure to a given laugh. Each question asked whether two laughs were the same or different, and participants had to press buttons to hear them (i.e., "Is this laugh {button 1} different than this laugh {button 2}?"). All participants experienced the same structure— they compared laugh A to laugh A for the first and third questions and compared laugh A to laugh C for the second. Next, participants were randomly assigned to rate

the authenticity of either laugh A (repetition condition) or a laugh that they hadn't heard before (laugh B, single condition).[3] This authenticity question, answered on a 7-point scale (1 = I think it is totally fake, 7 = I think it is totally genuine), was on the same page as a question about the gender of the person laughing—a filler intended to make our interest in authenticity less obvious.

Importantly, both the repeated laugh and the target laugh were either spontaneous *or* volitional. That is, participants rated the authenticity of a real or fake laugh that had either been played earlier (repeated condition) or preceded by a different laugh that was also real or fake (single condition). Thus, this study employed a 2(repeated vs. single) x 2(spontaneous laughter vs. volitional) between-subjects design.

Study 9. Participants (46% female, 52% male, 0.2% other/prefer not to answer, and 1.8% left the question blank, $M_{age} = 36$) saw two unique videos of politicians (President Trump and Representative Tim Ryan) citing statistics during a persuasive speech. Videos were between 3 and 5 seconds long. To isolate only the statistic, excerpts from their speeches were intentionally brief and (relatively) context-free. For example, clips might show President Trump remarking that Democrats had left him "142 openings for judges", and Tim Ryan asserting that General Motors received a "157-million-dollar tax cut"[4]. Participants were randomly assigned to see one of two different possible video clips from each politician.

Participants answered "Do you think that this politician memorized the statistic ahead of time, or is he making it up on the spot?" on a 7-point scale (1 = I think he memorized it ahead of

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[3] Note that between the three comparison questions and the authenticity rating, participants in the repeated condition heard the target laugh six times, while those in the single condition heard it once.

[4] At the time that they were spoken, the statement from President Trump was false and the statement from Tim Ryan was true.

time, 7 = I think he is making it up on the spot). They also answered, “How credible do you think this statistic is?” (1 = Not at all credible, 7 = Extremely credible).

Table 1. *Observer ratings for each experiment.*

Study	*N*	Video content	Dependent Measure	Number of Viewings	Mean (SD)	*d*
1	223	Survivor auditions	Rehearsal	1x 3x	4.66 (1.74) 5.26 (1.62)	.358
2	365	TikTok dances	Preparation/Practice	1x 3x	5.19 (1.50) 5.91 (1.23)	.529
3	604	CEO apologies	Preparation	1x 3x	5.16 (1.69) 5.41 (1.70)	.151
4	501	"First-time" reactions (product unboxing)	Authenticity	1x 3x	4.40 (2.04) 3.95 (2.24)	-.218
5	799	"First-time" reactions (captured during an experiment)	Authenticity	1x 3x	4.36 (1.89) 4.09 (2.14)	-.131
6	1501	"First-time" reactions (product commercials)	Probability that the target is an actor Probability that the target is *not* an actor	1x 3x 1x 3x	63.87 (27.18) 68.51 (29.11) 37.98 (27.89) 33.41 (29.58)	.165 -.159
7	3033	Improv Acting	Probability that scene is improvised (vs. scripted)	1x 2x 3x 4x 5x 6x	4.14 (1.84) 3.79 (1.85) 3.84 (1.96) 3.77 (1.95) 3.59 (1.91) 3.87 (1.94)	 -.188 -.155 -.189 -.297 -.142
8	2042	Audio clips of fake and real laughter	Authenticity of spontaneous (real) laughter Authenticity of volitional (fake) laughter	1x 3x 1x 3x	4.26 (1.78) 3.95 (1.80) 3.33 (1.65) 2.86 (1.53)	-.167 -.325
9	512	Politicians citing statistics	Was statistic memorized or made up on the spot	1x 3x	3.97 (1.79) 4.12 (1.85)	.122

Figure 1. *Experimental paradigm as illustrated by Study 1. Faces are redacted.*

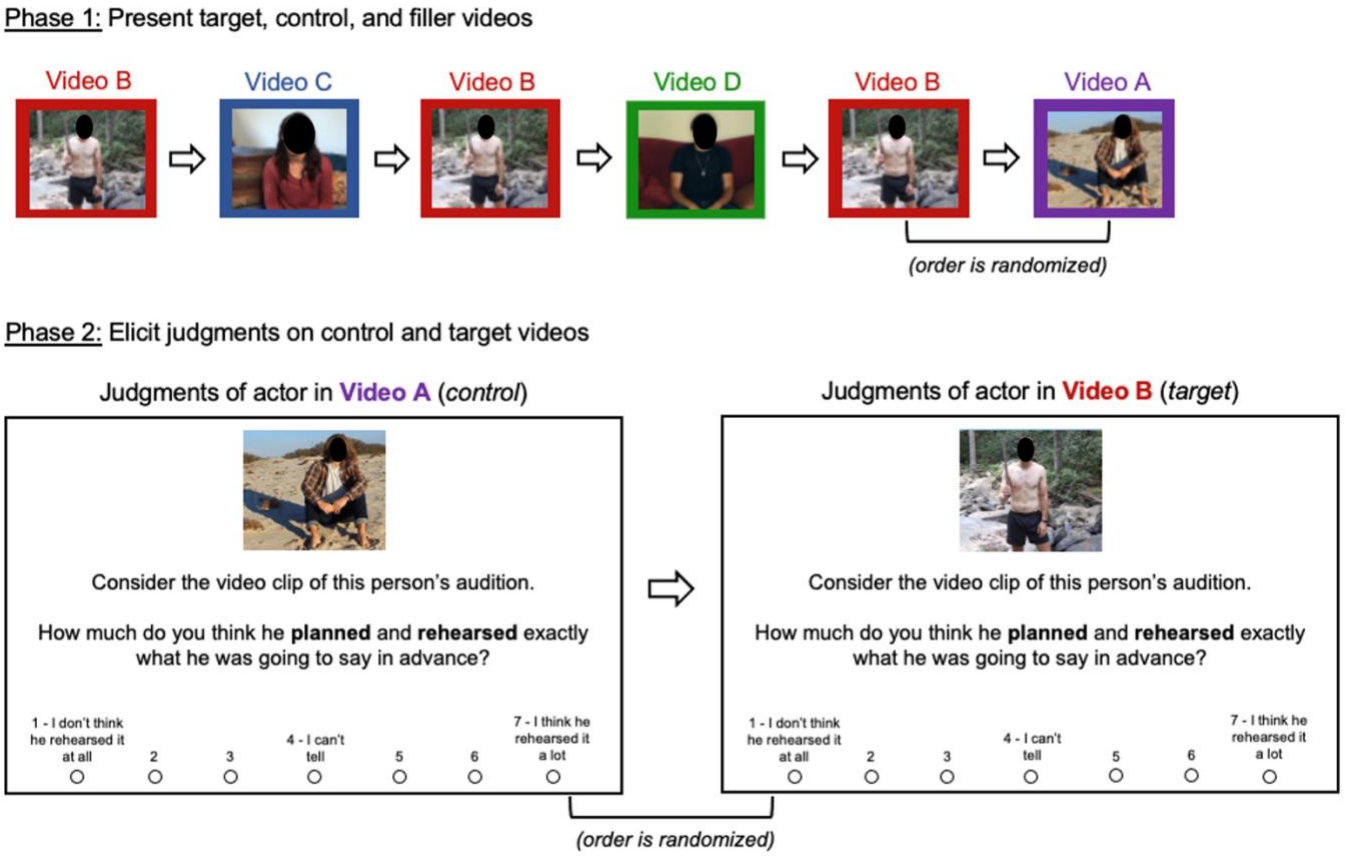
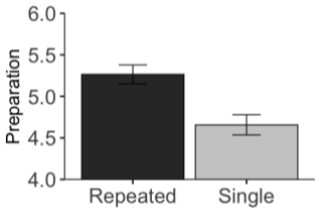


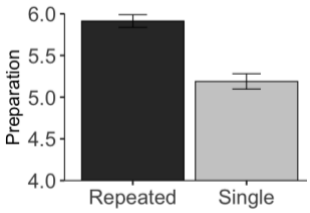
Figure 2. *Summary of findings.*

A. Repeated viewing made behavior seem less spontaneous and more prepared *(7-pt scale)*

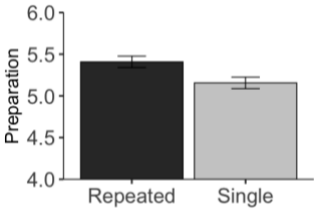
Study 1. Survivor auditions



Study 2. TikTok dances

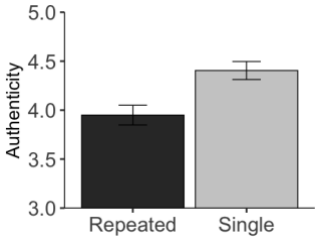


Study 3. CEO apologies

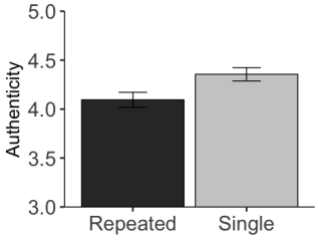


B. Repeated viewing made spontaneous reactions seem less spontaneous and more contrived *(7-pt scale)*

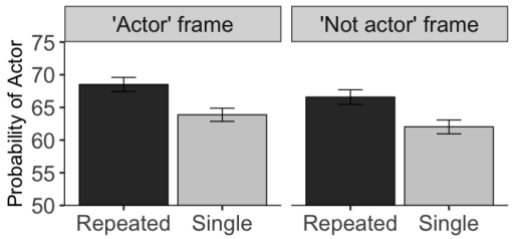
Study 4. Unboxing videos



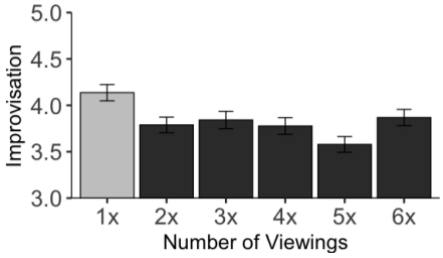
Study 5. Reaction videos



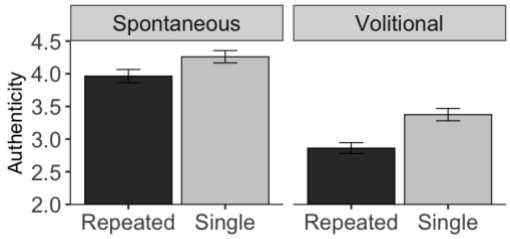
C. Repeated viewing made "real people" in commercials seem more like actors. *(101-pt scale; Study 6; scores in "not actor" frame are subtracted from 100)*



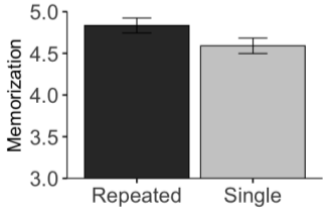
D. Repeated viewing made improv theater seem less improvised (more scripted). *(7-pt scale; Study 7)*



E. Repeated listening made both genuine and fake laughs seem less genuine. *(7-pt scale; Study 8)*



F. Repeated viewing made statistics seem more memorized. *(7-pt scale; Study 9)*



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Supporting Information

SI Materials and Methods

The videos and all other study materials are available on Research Box (https://researchbox.org/151&PEER_REVIEW_passcode=UDTBCA). The sample size, hypotheses, exclusion criteria and analysis plan were pre-registered for all studies on aspredicted.org. Our Research Box repository links these preregistrations and includes the de-identified data and R code for each study.

Exclusion criteria

English comprehension check. Except for Study 3, which accidentally omitted it, an English comprehension check was used in every study. The question showed this image:

Participants were given a text entry box to answer the question, "What is the man throwing in this picture?" We considered a "passing" response to be either "basketball" or "basket ball" (any capitalizations allowed). Participants who did not pass this check were excluded (see Table S1 for failure rates broken down by study).

Attention check. Studies 1, 2, 4, 6 and 9 included an attention check in which participants had to select which of four images (stills taken from videos) were from a video that they did not see. Studies 3, 5 and 7 did not include an attention check. Participants who failed to identify the

correct video were excluded. Study 8 specified stringent exclusion criteria in lieu of an attention check. In Study 8, by way of manipulating replay frequency, participants had to indicate whether two laughs were the same or different on three different occasions; the correct answers were "same", "different" "same". Participants who provided a different answer on any of these questions were excluded.

Table S1. *Information about exclusions and sample size*

Study	Original N	Percentage Failing Attention Check	Percentage Failing English Check	Final N
1	223	4.0%	5.4%	204
2	365	16.4%	21.1%	265
3	604	n/a	n/a	604
4	501	0.2%	1.2%	494
5	799	n/a	3.6%	770
6	1501	1.7%	4.3%	1417
7	3033	n/a	0.6%	2847
8	2042	32.2%	8.9%	1290
9	512	1.0%	5.3%	483

We note that Study 2 shows an extremely high failure rate for both the attention check and English comprehension check questions, and participants in Study 8 had an even higher exclusion rate based on failing the attention check. It is not clear why participants in these two studies failed at such a higher rate than other studies, but it may be worth noting that both were the only studies not run on Prolific Academic. Specifically, Study 2 was run on Amazon Mechanical Turk and Study 8 was run using the Luth participant pool. As described earlier,

Study 8 also employed a more stringent attention check than did other studies. Importantly, the results from all studies hold without any exclusions (see Table S2).

Alternate analyses

As a robustness check, in the table below we report results from each study 1) without exclusions, 2) using linear regression, instead of a mixed model, that simply controls for participant and video, and 3) with a basic paired-samples t-test. Note that not all alternate analyses apply to every study. Namely, there were no exclusions made in Study 3 due to experimenter error, and Studies 7 and 8, which are entirely between-subject, already use linear regression and cannot be examined using a paired-samples t-test. Table S2 displays the results from these alternate specifications. Note that for studies that employed them, both primary DVs (e.g., rehearsal, authenticity) and auxillary DVs (e.g., seriousness, credibility) are included.

Table S2. *Alternate analyses for each study.*

Study	DV	Without exclusions	Linear regression	Paired t-test
1	Preparation	$t = 4.46$, $p < .001$	$t = 4.31$, $p < .001$	$t = 3.38$, $p = .001$
	Effort	$t = 2.97$, $p = .003$	$t = 2.38$, $p = .018$	$t = 1.95$, $p = .053$
2	Rehearsal	$t = 6.85$, $p < .001$	$t = 6.07$, $p < .001$	$t = 7.61$, $p < .001$
	Skill	$t = 4.67$, $p < .001$	$t = 3.52$, $p = .001$	$t = 4.55$, $p < .001$
3	Preparation	n/a	$t = 1.98$, $p = .048$	$t = 2.59$, $p = .001$
	Seriousness		$t = 1.94$, $p = .053$	$t = 2.39$, $p = .017$
	Change		$t = 1.77$, $p = .077$	$t = 2.45$, $p = .015$
4	Authenticity	$t = -4.99$, $p < .001$	$t = -4.86$, $p < .001$	$t = -3.10$, $p = .002$
5	Authenticity	$t = -2.63$, $p = .009$	$t = -2.56$, $p = .011$	$t = -2.73$, $p = .006$
6	Probability of target being an actor	$F = 19.75$, $p < .001$	$F = 18.52$, $p < .001$	$t = 4.64$ $p < .001$
7	Probability that actor's line is improvised, not scripted	1x vs. 2x: $t = -2.80$, $p = .005$ 1x vs. 3x: $t = -2.17$, $p = .031$ 1x vs. 4x: $t = -1.90$, $p = .058$ 1x vs. 5x: $t = -4.39$, $p < .001$ 1x vs. 6x: $t = -2.12$, $p = .034$	n/a	n/a
8	Authenticity	$F = 17.72$, $p < .001$	n/a	n/a
9	Memorization	$t = 3.22$, $p = .001$	$t = 3.19$, $p = .001$	$t = 1.86$, $p = .064$
	Credibility	$t = 2.82$, $p = .005$	$t = 2.78$, $p = .006$	$t = 1.24$, $p = .217$