Initial Elevation Bias in Subjective Reports

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Significance

People's reports of their own thoughts, feelings and behaviors are essential assessment tools in biomedical and social science. They be used to take a snapshot of how people are doing and to track change and the effects of interventions. When subjective states have been studied over time, researchers have often observed an unpredicted and puzzling decrease with repeated assessments. Our results across multiple outcomes in four field experiments suggest that this pattern is due to an initial elevation bias. This effect is larger for reports of internal states rather than behaviors and for negative mental states and physical symptoms than for positive states. This initial elevation bias needs to be considered in all types of research using subjective reports.

Abstract

People's reports of their thoughts, feelings and behaviors are used in many fields of biomedical and social science. When these states have been studied over time, researchers have often observed an unpredicted and puzzling decrease with repeated assessment. When noted, this pattern has been called an "attenuation effect", suggesting that the effect is due to bias in later reports. However, the pattern could also be consistent with an initial elevation bias. We present the first systematic, experimental investigations of this effect in four field studies (Study 1: N = 870, Study 2: N = 246, Study 3: N = 870, Study 4: N = 141). Findings show clear support for an initial elevation bias, rather than a later decline. This bias is larger for reports of internal states than behaviors, and for negative mental states and physical symptoms than positive states.

We encourage increased awareness and investigation of this initial elevation bias in all research using subjective reports.

Conflicts of Interest

None of the authors have conflicts of interest associated with this article or its recommendations.

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People's reports of their thoughts, feelings and behaviors are used in many fields of biomedical and social science as well as clinical practice. Epidemiologists, for example, use symptom reports in studies of disease; sociologists and economists use social survey reports to study economic and social behavior; and neuroscientists use reports of emotional states to understand patterns of brain activation. Often participants are asked to provide reports at multiple points in time, and investigators use these to test predictions about causal processes and to describe developmental change.

However, at least in the case of negatively toned states such as mental and physical symptoms, concern has been expressed about the validity of repeated assessments. For over 50 years, researchers have noticed that reports of levels and severity of certain symptoms and maladaptive experience diminish over repeated assessments (1-4).

In a commentary on the National Institute of Mental Health Epidemiological Catchment Area study results, Robins noted that, " in second interviews respondents frequently fail to report [lifetime] symptoms that they reported in the first interview. The strange result is that the proportion of life-time cases at reinterview seems to shrink if the second interview rather than the first is used to calculate prevalences"(2). This pattern of decrease after initial report in longitudinal studies has been called the "attenuation effect" (5-7), suggesting that the pattern is due to a decline in later reports.

This label is premature in our view because previous evidence cannot distinguish whether the decline is due to upward bias in initial reports or downward bias in subsequent reports, or a combination of both. Upward bias would occur if respondents report at the initial survey higher levels or severity of the target state than they actually

are or were experiencing, and downward bias would occur if participants tend to report lower levels or severity than they experience. In addition to downward bias, there is the possibility that the initial survey experience literally changed the course of the subjective process. Called measurement reactivity (8), the subsequent reports are not necessarily biased, but they would not reflect the longitudinal process in the part of the population that was not assessed. Distinguishing bias in the initial assessment or subsequent assessments from measurement reactivity is challenging. We attempt to do so in this article, but we first document the phenomenon that level and severity of subjective reports often decline over repeated assessments. We began our work using a neutral label for the typical pattern: The Initial Elevation or Later Decline (IELD) effect.

We present the results of four field experiments: The first, second, and fourth experiments used intensive longitudinal data collection with daily diaries to examine moods, symptoms, and study habits before stressful examinations. The third experiment used a bi-monthly panel survey design to examine these outcomes in the broader context of freshmen adjustment to college. These target outcomes were selected to allow a contrast between more subjective reports (e.g. moods) and more objective behavior (e.g. study time). They also allow a contrast between negative (e.g. physical symptoms) and positive (e.g. vigor) states, because the previous literature has tended to report the IELD effect primarily on negative outcomes. Previous results are also possibly confounded by naturally occurring events or time-period variation. Each of our experiments removed this important confound by disentangling the timing of the report (e.g. relative to an exam) from the serial position of the report within a longitudinal

series. Participants were randomly assigned to different starting times, allowing for an experimental examination of the IELD effect.

Across all four experimental studies and across multiple outcomes, we find a robust IELD pattern. Overall, the findings converge to support an initial elevation bias, rather than a later decline. The standardized effects range from small to medium, with larger and more reliable effects for reports on internal states than behaviors, and for reports on negative states and physical symptoms than positive states. The effects were also larger and more reliable in the daily diary design than the panel design, and when we compared persons to other person starting at different times rather than to themselves over time.

The third and fourth field experiments were also designed as preliminary tests of two potential mechanisms of the IELD effect. The first mechanism is a phenomenological process, whereby the experience of assessment drives real changes in participants' internal states. This process could encompass either initial elevation, where increased self-awareness exacerbates emotional intensity (7, 9) a later decline through a therapeutic process, whereby expressing distress causes actual declines in subsequent distress and other negative outcomes (10, 11), or both. This was examined in the third field experiment by contrasting reports a target person made about themselves with reports roommates made about the target. Evidence was inconsistent with either effect because the initial elevation and later decline pattern was present for both self- and roommate reports.

The second mechanism involves conversational norms, whereby participants interpret each repeated assessment as part of an ongoing conversation. This means

that irrespective of the exact questions asked of them, participants respond in ways that are normative in a conversational context. These norms include (i) only providing what participants perceive to be relevant information and (ii) in later assessments, only providing what they perceive to be new information (12, 13). The former could lead to an initial elevation bias (where participants provide exaggerated reports of their current states because they include information from earlier or future time periods) and the latter to a decline in bias (by ignoring their actual current states and only reporting what is new about those states).

In the fourth field experiment, participants were randomly assigned to experience either a single two-week diary study about stress approaching an exam (i.e. one conversation) or to experience two sequential but ostensibly unrelated one-week diary studies, the first about health and the second about the approaching exam (i.e. two conversations). Comparisons between the one-study and two-study participants did not support the conversational norms mechanism. We note that these are only preliminary tests of these mechanisms and further research is necessary. Moreover, these are only two potential mechanisms; other mechanisms are discussed below.

That said, we can state that the IELD effect is robust, appearing for multiple outcomes across four field experiments. It is evident in both diary and panel designs, in the context of acute or chronic stress, for reports about the self and about a familiar other. We judge that the IELD effect is due to initial elevation bias and that it can occur in all types of research that use subjective reports. This claim and the broad implications are discussed further below.

Study 1: Demonstrating the IELD in a Diary Study of Bar Examination Preparation

Study 1 was a survey of recent law school graduates and their intimate partners carried out in the period 2001-2003 during the four-week period before the graduates took a state bar admissions examination, as well as the week following the examination. Couples were randomly assigned to one of four conditions. The majority of couples (*N* = 393) were in Condition 1: They completed twice-daily reports (morning and evening) for 44 days, starting 35 days before the exam, including the 2 exam days, and continuing for 7 days afterwards. The twice-daily design allowed the initial report to be studied in absolute terms (first morning report) and more relative terms (reports in first day of study). In Conditions 2, 3 and 4, groups of couples were asked to make initial reports on Day 22 (2 weeks before; *N* = 66), Day 35 (one day before; *N* = 27) and Day 44 (one week after; *N* = 34). We examine IELD patterns for reports of anxious and vigorous mood, physical symptoms and time spent studying (examinees only), using both within-subjects comparisons of respondents' initial reports (Day 1) to matched subsequent reports (Day 8) and between-subjects comparisons of persons in Condition 1 (the daily diary) to persons responding for the first time closer to the exam (Conditions 2-4). Figure 1 illustrates the design and results for evening anxious mood reports for the examinees.

The examination was on days 36 and 37. The survey form for all four conditions was the same; it obtained "right now" reports of moods and retrospective reports of physical symptoms and time spent studying during the prior 24 hours. One can see from Figure 1 that evening anxiety generally increased from the first week of exam preparation to the days of the exam, and that there was a remarkable recovery to low daily anxiety in the week following the exam. This pattern is completely consistent with

theoretical and intuitive expectations of the effect of stressor temporal imminence (14) and supports a claim of construct validity of the daily measure. We argue that this pattern allows us to consider the diary reports on days 22, 35 and 44 as a standard that allows bias of the initial reports on those days to be estimated. We also use this pattern to examine a more conservative estimate of bias for the first diary day of the series. Stressor imminence predicts that days closer to the exam will be more distressing than days more distal to the exam. Anxiety on day 8, which is the same day of the week as day 1, should therefore be higher on the average than day 1. Insofar as the opposite is observed, an IELD effect can be conservatively estimated.

The Day 1 minus Day 8 comparison[1] for AM anxiety yielded a Cohen's *d* of 0.47 ($t(285) = 8.28$, $P < 0.0001$). The comparison for PM anxiety yielded a *d* of 0.26 ($t(284) = 4.50$, $P < 0.0001$). Cohen's *d* (10, p. 20) estimates were obtained by calculating the mean within-subject change and dividing it by the pooled between-subject standard deviation. According to Cohen, small, medium and large effect sizes for *d* are 0.2, 0.5, and 0.8, respectively; the AM anxiety effect size would thus be classified as medium, and the PM anxiety effect size as small.

Table 1 shows the results for the within-subject IELD effects for anxious mood as well as for AM and PM vigorous mood, PM physical symptoms and PM reports of how many hours the examinee spent studying. Like anxious mood, there was a significant IELD effect for reports of physical symptoms, ($d = 0.39$, $t(288) = 5.28$, $P < 0.0001$), but not for vigorous mood in the AM or PM, nor for study time.

[1] We report IELD effects as if they are initial elevations rather than later declines. This allows us to represent the expected effects as positive in sign rather than negative.

We next compared the anxious mood reports of the diary group at days 22, 35, and 44 to the reports of the three panels that completed the same survey form for the first time on those days. The between-subjects comparisons at day 22 revealed significant IELD effects for the negative experiences of anxious moods and physical symptoms, but not for vigorous mood or study time. In comparison to the within-subjects tests, the effect sizes of the significant IELD effects tended to be larger for the between-subjects tests. In Cohen's classification, the AM anxious mood effect (d = .82, $t(332)$ = 5.46, P <.0001) and the PM physical symptoms effect (d = 0.80, $t(332)$ = 5.72, p<.0001) were large, and the PM anxious mood effect was small-to-medium (d = 0.35, $t(331)$ = 2.29, P < 0.03).

On Day 35, the eve of the examination, there was only one outcome that showed an IELD effect: Those in the diary group reported fewer physical symptoms than those in Day 35 only group. As is apparent in Figure 1, there were no group differences in PM anxiety. In contrast, on Day 44 there were large IELD effects for AM anxious mood (d = 0.84, $t(263)$ = 3.83, P < 0.001) and physical symptoms (d = 0.88, $t(266)$ = 4.32, P < 0.001) and a medium effect for PM anxious mood (d = 0.58, $t(263)$ = 2.62, P < 0.001). Even though the participants in Condition 4 who were responding to the survey were typically on vacation following the exam, they reported elevated current anxious mood and physical symptoms in their first experience with the study survey. This finding is perhaps the most compelling reason from Study 1 to conclude that the IELD pattern is due to an initial elevation, not a later decline.

To check if the IELD effects were specific to examinees, i.e., only those members of each couple who were directly exposed to the stressor, we looked at the analogous

pattern in their partners. Partners were in the same condition (diary or one of the panels) as their examinee. Table 1 shows that like the examinees, the partners tended to report more anxious mood and physical symptoms on their first day relative to Day 8, but they also reported a slightly elevated level of vigorous mood in the evening (d = .18, $t(268) = 2.56$, $P < 0.001$) on Day 1 versus Day 8. The between-subjects contrasts at Days 22, 35, and 44 for the partners produced consistent results: Comparing partners in panels to the relevant days for partners in the diary condition, panelist-partners reported higher levels of anxious moods in the AM and PM, as well as physical symptoms. These effects were typically large in magnitude, and it is noteworthy that they were observed even on the day before the examination, a point in time where a comparison of examinee outcomes showed almost no differences. We found no IELD effects, however, for vigorous mood in any of the between-subjects comparisons for partners.

In sum, Study 1 showed that IELD effects for daily reports of mood, physical symptoms and study time were as large as d = 0.88 for examinees and d = 1.09 for partners, with median effect sizes of 0.26 (examinees) and 0.24 (partners) across all variables and times. Effect sizes for subjective negative internal states and physical symptoms were larger (median effect of .54) than those for the more concrete behavior of study time (median effect .00), and for positive states (median effect .07). Further, these results support an initial elevation bias rather than a later decline: As the exam approaches, participants are typically more anxious, but for Group 1, the within-person comparison shows a decrease in anxiety from Day 1 to Day 8. Groups 2 and 4 both show a distinct elevation on their starting day relative to Group 1; this is particularly noteworthy for Group 4, who on day 44 are reporting during the recovery period after

the exam, when anxiety should be low. Both examinees and partners who make their initial report on day 44 report levels of anxiety and physical symptoms that are remarkably elevated relative to what is expected.

Study 2: Demonstrating the IELD Effect in a Diary Study of College Exam Preparation

Study 2 conceptually replicates Study 1, focusing on a shorter period of time before a less critical exam, and varying the start dates in the diary design. It did not include partners or any start dates after the exam. Participants were college students preparing for pre-medical science examinations between the fall semesters of 2010 and 2011. They were randomly assigned to start the diary survey on one of seven different days (ranging from 9 days to 2 days before the examination). Each participant, irrespective of when they started, was required to complete diaries for 14 consecutive days. They reported mood at waking, and at bedtime they reported mood, physical symptoms and amount of time spent studying. Figure 2 shows how the average anxious mood (top-left) and physical symptom count (bottom-left) varied over days in each of the seven groups. Again, if there were no IELD effects anxiety would be expected to increase until the exam and then drop afterward. Ignoring the initial responses, the expected pattern is observed. However, the initial responses reveal a pattern that is more consistent with an initial elevation bias rather than a later decline: One can see that the initial report tended to be higher than the adjacent reports across all seven groups, before coming back to what is presumably the phenomenologically real change (e.g. an increase in anxiety) as the exam approaches.

Table 2 shows the estimated IELD effects for the morning and evening anxious mood and vigorous mood, and evening physical symptoms and study time. These estimates are based on comparisons of the first reports of persons in groups 2 through 7 to the reports on the same day of participants who had started the diary process earlier. There was a medium effect for AM anxiety (d = .56, $t(225)$ = 5.98, P < 0.0001) but no evidence of an IELD effect for PM anxiety on the same day. In contrast, there was no evidence of an IELD effect for AM vigor, but there was a small to medium effect for vigor reported in the evening (d = .38, $t(225)$ = 3.27, P < 0.002). There was also evidence of a small to medium effect for physical symptoms, which were only reported in the evening (d = .34, $t(225)$ = 4.15, P < 0.0001), and also for evening reports of study time (d = .43, $t(225)$ = 4.36, P < 0.0001).

Study 2 results can be compared to the between-person examinee results from Study 1. For AM anxious mood, the IELD effect was replicated, although smaller than the average effect reported in Study 1; for the PM anxious mood effect there was no IELD replication. In addition, Study 2 found IELD effects that were not apparent in Study 1: positive mood (vigor), as well as for the more objective report of study time.

To what extent does the IELD effect confound inferences about the size of temporal effects? The data from Study 2 provides a clue. In Figure 2 (top left plot) one sees that there is a sharp increase in anxiety as the exam draws near, but that it drops dramatically after the exam. From the day before to the day after the exam the average drop in AM anxiety is 1.00 (SE=.12) in standardized effect units. Had the day before been the initial survey, we would expect from Table 2 that the change would be .56 larger — 1.56 rather than 1.00 (1.56 times too large). In contrast, the change in reported

PM physical symptoms from the day before to the day after the exam (Figure 2, bottom left) is 0.18 (SE=.08). The IELD effect from Table 2 is .34. Had the first measure been biased by the IELD effect, the estimated effect of the exam would have been nearly three times (2.89) too large. We conclude from this exercise that the impact of the IELD effect on substantive findings is not constant and needs to be considered in each context.

These first two studies were both limited to the context of upcoming stressful events, and to the use of diary designs, with reports made on sequential days about recent or current events and feelings. Is the IELD effect limited to this context and design? In Study 3 we examined whether IELD effects are found in a milder chronic stress context (everyday college life) rather than acute stressor context (exam preparation). This study uses bimonthly measurement over 8 months with a design that reduces the confounding of possible IELD effects with period effects (Beginning of Fall Semester, Thanksgiving, etc.) from first-interview effects.

Further, Study 3 sought to specifically examine a potential phenomenological mechanism for the IELD effect. That is, the IELD may not result from a bias in reporting, but from a phenomenologically real, albeit measurement-driven, change in state. Previous researchers (8) have proposed that the IELD may be observed for negative states -- not because of a bias in reporting-- but because of genuine ameliorative effects of self-disclosure and study participation leading to later decline. Alternatively, participants beginning the study may experience increased self-awareness, which could increase emotional intensity (9), contributing to initial elevation. To examine this possibility, Study 3 includes surveys of college roommates' perception of each other's

level of distress. For roommates providing repeated reports about another person instead of the self, a phenomenological effect would not be plausible.

Study 3: Demonstrating the IELD Effect in a Panel Study of College Roommates and Testing a Therapeutic Mechanism

Study 3 participants were undergraduate students (N=870) who were recruited in the 2009-2010 academic year for a study of their experiences of "college life"; the timing and content of the surveys were unconnected with specific stressors. Eighty-five percent of the participants were recruited as roommate pairs, and half of these were randomly chosen to report on their own college experience and the others on their roommate's experience. Repeated online surveys were scheduled for October, December, February and April, with participants randomly assigned to groups that began in October (*n* = 171 self report; n=124 roommate report), began in December (*n* = 158 self report; n=127 roommate report), or began in February (*n* = 162 self report; n=128 roommate report). Self-report participants revealed their current anxious and vigorous mood, six-week recall of physical symptoms, and six-week recall of mental distress symptoms (K10 scale) (15). Roommate-reporting participants rated mental distress symptoms for their college roommates.

Table 3 shows estimates of the IELD effects for the self- and roommate-reports. The effect estimates, which are based on repeated between-person comparisons, were obtained using a mixed-effects model that included an indicator of the initial assessment, an adjustment for periods close to scheduled examinations (December and April), and a person random intercept. All the IELD estimates for self-reports were significantly different from zero, but the magnitude of the effects was small (ranging

from d = .15 for current vigor to d = 0.29 for own mental distress). Taken together, studies 1 through 3 showed that IELD effects were omnipresent for self-reports of negative states and physical symptoms, with smaller, less robust effects for positive states. The effect was smaller in this study as compared to the previous two; this could be attributable to the change in design from diary to panel, the change in context from acute to chronic stress, or a combination of both.

We were especially interested in whether the IELD would be present for participants' reports of their roommates' mental distress. If the IELD is driven by a phenomenological effect, wherein one's experience of measurement drives an actual change in one's internal state, then the IELD would not be present when reporting about another person. Contrary to this hypothesis, there was a significant IELD effect for mental distress reports on roommates (d = .13, $t(341) = 2.45$, $P < 0.015$).

Another explanation of the IELD effect that remains plausible is the conversational norm mechanism, whereby participants interpret the repeated measurements as part of an ongoing conversation and, in an effort to follow norms, focus on providing relevant information and on updating previous information (12). For example, in Study 1 participants were recruited from law schools to report on their bar exam preparation. One group was randomly selected to give their first report *after* the exam had taken place. Although they were likely to have recovered from the stress of the exam seven days afterward, they might have assumed that the investigators were interested in how stressful the exam *had* been, even though the questions were phrased for current symptoms and problems. Study 2 was also framed around stressful experiences; participants' initial reports of negative states may therefore have been

elevated by mixing their current state with their global sense of anxiety about the stressor. Although study 3 was not framed about a specific stressful exam, it was announced to be about "College Life", which many students at competitive private universities might associate generally with stress.

The design of the first three studies confounded experience with the measures with the framing of what the researcher might want to know. Moreover, participants' first reports were also their first experience completing the measures. Study 4 was designed to test the conversational norms mechanism by separating experience with the measures from the initiation of the conversation with the researcher.

Study 4: Testing the Conversational Norms Mechanism for IELD in a Single Study versus Sequential Studies Design

The final study, like Study 2, again focused on undergraduates preparing for a difficult exam. All participants were initially recruited (from 2011 to 2012) to participate in a study that was called the *Exam Preparation Study.* They were randomly assigned to two groups: Participants in Group 1 (n = 53) were simply asked to complete fourteen days of diaries for the *Exam Preparation Study* (identical to Group 1 in Study 2), whereas participants in Group 2 (n = 66) were given a more complicated story. On the night before the study was to begin, they were told that the *Exam Preparation Study* had reached its quota of subjects, and that they would not be needed until a week later. They were then told that, if they were interested, they could participate in the interim in a one-week diary study that paid the same amount, but that was on health run by a different faculty investigator. They were told that the *College Health Study* focused on everyday health behaviors in college students and was not at all concerned with the

upcoming examination. Two thirds of those in Group 2, a subgroup we call Group 2a (*n* = 44), agreed to be in the health study. They gave new informed-consent for an ostensibly new faculty investigator, and completed a week-long diary study. Participants in the remaining subgroup, which we call Group 2b (*n* = 22) chose not to participate in the health study, but all of them chose to begin the *Exam Preparation Study* a week later. Thus we had three groups: Group 1 completed diaries on 14 consecutive days, from 11 days before to 2 days after their examination. Group 2a completed diaries for the unrelated health study for 7 days, and switched to completing diaries for the *Exam Preparation Study* on Day 8 and continued until Day 14. Group 2b enrolled in the *Exam Preparation Study* but didn't begin completing diaries until Day 8 and then continued to Day 14 (identical to Group 7 in Study 2). These three groups are shown in the right side panel of Figure 2.

If the conversational norm mechanism were operating, on day 8 participants in both Group 2a and 2b would show an elevation compared to Group 1: For Group 2a and 2b, this day marked the start of a new conversation, whereas for Group 1 this day merely continued the conversation they had already been having. Table 4 and the right side of Figure 2 shows that there were no between-persons IELD effects on Day 8 for Group 2a compared to Group 1. There were, however, IELD effects for physical symptoms in within-subjects comparisons of Day 1 with Day 8 for Group 1 and for both anxious mood and physical symptoms when Group 2b was compared to Group 1. Again, consistent with Studies 1 and 2, the effects appear to be initial elevations rather than a later declines: The approaching exam would not be consistent with a reduction in anxiety and physical symptoms. The first report for Group 2b is elevated compared to

Groups 1 and 2a in a deviation from the presumably phenomenologically real increase associated with the exam.

Discussion

Previous researchers who noticed the unexpected and puzzling pattern of decline after initial assessments have labelled the pattern an "attenuation effect", implying an artefactual change (i.e. later decline) in later reports (5, 6). However, little systematic research about the size, scope, or basis of the effect has been done. We addressed this gap in four field experiments.

Overall, we find "attenuation" to be a misnomer: Our findings are generally consistent with an initial elevation bias, not a later decline. This is evident within persons, with reports of anxiety initially decreasing, in spite of an approaching exam in Studies 1, 2, and 4. It is also strikingly evident between persons: Those participants who gave their first reports of anxiety 10 days after the Bar Examination showed marked elevation compared to those who had already provided reports. Also, as illustrated in Figure 2 for both Studies 2 and 4, initial reports for each group are elevated before subsequently converging into a coherent longitudinal pattern. Furthermore, the hypothesis that respondents were essentially receiving a treatment that ameliorated distress, leading to a later decline, was inconsistent with the results of Study 3. In that study roommate's reports on each other showed the same IELD pattern, contrary to the later decline mechanism.

The overall IELD effect is robust, appearing across multiple outcomes in four studies. It appears across different study designs (intensive longitudinal vs. panel, reporting on the self vs. reporting on one's roommate, within-person and between-person) and context (acute vs. chronic stress, for stressed persons vs. their romantic partners). The size of the effect also varied, with larger effects for internal states than for behaviors, and for

negative states and physical symptoms than for positive states. Nearly all effect sizes we observed (median Cohen's *d* in Studies 1, 2 and 3: .26, .34 and .16) are likely to be of practical significance, whether in establishing clinical cutoffs for depression or physical discomfort, or simply in establishing benchmarks for responses to questions of emotions, physical symptoms and a host of other subjective reports. We argue that the robust IELD findings are likely to be due to initial bias and therefore that initial bias effects need to be considered when interpreting survey data on subjective reports.

The present work has several important strengths: It is, to our knowledge, the first to conduct experimental investigations of the pattern of decrease after initial reports in longitudinal research, and the first to systematically consider whether the pattern is a bias in initial elevation or later decline. We present a large body of evidence collected in four field experiments. The experimental design allows us to demonstrate the effect between-persons as well as within-persons, and further, the between-person comparisons support our proposal that it is the process of starting the study that is causing the initial elevation bias. Finally, the replication of the initial elevation bias across each of these four field experiments eliminates potential artefactual explanations for the effect, such as an unanticipated secular event that affects reporting.

There are also limitations in the present work. First, our four experiments were carried out with students or law school graduates who were either facing scheduled examinations or engaged in an academic semester. We studied these participants because they were easily recruited and they were willing to provide the intensive longitudinal data we required. Although they come from specialized populations, the IELD effects they displayed are consistent with similar effects that have been reported in epidemiological surveys (2, 16) of general populations. Similar drops in self-reported symptoms from the first to second wave of data continue to be reported in studies of psychopathology (17, 18) and health

behavior (19). We hope that the magnitude of the IELD effects will be studied as investigators become aware of the phenomenon.

A second limitation is the range of possible mechanisms considered. We proposed and tested two mechanisms, but further work is needed. Although we present paradigms for testing the phenomenological mechanism and the conversational norms mechanism, each of these merit a program of research beyond the scope of this paper. Our initial findings are inconsistent with these two mechanisms but they should not yet be ruled out. Further, these are but two of a variety of potential mechanisms. One in particular that we believe to be promising is a learning mechanism, whereby increasing familiarity with a measure leads to less extreme reporting (6).

A similar mechanism—meaning-making—has been studied by Knowles and colleagues to explain an IELD effect at the micro-level: Shifts in a single measurement session, within a single instrument, where participants' responses to items presented early are more extreme than items presented later (1). Although this pattern is consistent with IELD, it is operating on a different scale than we have examined in the present research, and so may operate by different psychological principles.

Other examples of IELD effects that differ from the present work would include those on longer timescales or with less subjectivity in measurement. We previously cited Robins (2) who noted that incidence rates of mental disorders cannot be accurately estimated from retrospective lifetime prevalence surveys that show the IELD pattern. The IELD effects of complex retrospective health surveys might be due to processes such as learning that endorsing screening questions leads to additional questions about the scope and severity of symptoms (6, 20). We sought to eliminate such additional mechanisms in the work reported here. Our work speaks primarily to repeated measurement of subjective reports on current internal states and recent behavior.

The implications of the initial elevation bias as demonstrated in our present work are nonetheless wide-ranging. A bias due to later decline would have been problematic for research using repeated measurements. But an initial elevation bias potentially extends to all research using subjective reports, including cross-sectional designs. In such designs, the potentially biased first report is the *only* report. When social scientists study reactions to national or world events, or when medical researchers screen for disorders, elevation biases lead to false conclusions and to screened participants without the disorder. Randomized studies will be resilient to the bias, but when baseline measures are taken the IELD effect can contribute to placebo effects in the control condition. Although the IELD effect would not affect correlational studies if it were constant across individuals and variables, it could confound correlations if the magnitude varied with some characteristics of respondents. At the request of a reviewer, we checked to see if the IELD effect interacted with gender or majority/minority status in Studies 1-3 but we found no consistent effects for these variables. This does not mean that the IELD effect does not vary with other personality or communication characteristics. We simply urge survey researchers to add this effect to the list of other processes than can bias results, such as fatigue or acquiescence bias.

Recognizing an elevation bias leads to arguments in favor of designs that use repeated measurements, as they allow for the observation of and potential to adjust for the initial elevation bias, for example by dropping initial observations (e.g. (21)). Alternatively, researchers interested in subjective reports could consider providing prior experience with the given instrument before taking measurements of focal interest. This could be seen as analogous to common procedures with other types of measurement that require practice or establishing a baseline (e.g. computer tasks using reaction times, physiological recordings).

We hope that the present paper will increase awareness of IELD effects generally, and the possibility of initial elevation bias in particular.

Materials and Methods

Study 1: Establishing IELD in a Diary Study of the Bar Exam

Participants and Design. Participants were recent law school graduates who prepared for the state bar examination (N = 436, 55.5% female; age: M = 29.7) and their romantic partners (N = 434, 46.1% female; age: M = 30.2). Participants were recruited over three years (2001-2003) for a study of stress and support (22). They started five weeks before the exam (preparation: Day 1-35, exam: Day 36-37), and continued for one week afterwards (recovery: Day 38-44). In 2001 and 2002, couples were randomly assigned to one of four groups: Daily Diary from Day 1-44, Panels on Days 22/35/44, Single assessments on Day 35 only and Day 44 only. In 2003, all participants were assigned to the Daily Diary condition. For the current analyses we only analyzed Day 22 in the second group. After eliminating eight persons with incomplete data, the sample sizes were 326 (Daily diary), 60 (Assessment on Day 22), 19 (Assessment on Day 35), and 23 (Assessment on Day 44). Couples were paid $150 in the daily diary condition and $50 in the other three conditions; all participants were entered into a lottery to win $1000. This study and the following studies were approved by one or both of the New York University or Columbia University Institutional Review Boards and informed consent was obtained from all participants in all studies.

Measures. For all studies in this article, we focused on one negative mood (anxiety), one positive mood (vigor), and physical symptoms. In Study 1, 3, and 4,

examinees also reported their study time. In Study 1, anxiety and vigor were measured using 3 items each from the Profile of Mood States (23). These have been shown to be reliable measures of between person and within person mood (24). Participants reported their current mood twice, once upon awakening and once before going to bed. Response categories ranged from 1 (not at all) to 5 (extremely)(rescaled to a 0-4 range in the analysis). To assess physical symptoms, participants reported if any of four symptoms occurred in the previous 24 hours: back or muscle ache, headache, upset stomach, and insomnia. These binary reports were averaged into a physical symptom index that ranged from 0 to 1. The index represents the overall burden of physical symptoms on a given day. To assess hours spent studying, the examinees were asked to report how many hours they spent studying for the exam. Reports ranged from 0 to 15, with an average of 8 on most days. The measures in Study 1 were given as paper-and-pencil forms containing a total of 122 questions about mood, social support, stressors, and coping.

Data Analysis. All five outcomes were divided by the average between person standard deviation so that differences can be interpreted as Cohen's d effect sizes (25). We estimated the within-person IELD effect by subtracting Day 8 scores from Day 1 scores and computing one sample *t tests* on the differences as well as computing 95% confidence bounds. We used Day 8 as the comparison score to adjust for day of the week effects. We estimated and tested between-person IELD effects by comparing the responses of the participants who gave their first response on day 22, 35 or 44 to the responses of participants in the daily diary condition on the corresponding day. Independent sample *t tests* were used to test the statistical significance of the difference

and to compute 95% confidence bounds. In all analyses we assume two tailed tests with a Type I error rate of .05. This initial study was part of a larger research program to understand stress, affect, and social processes in couples (22), and was conducted without a-priori power analyses for the purpose of the IELD effect. The syntax and all the data used for Study 1 analyses are available at https://osf.io/8w2du/.

Study 2: IELD and Timing in Preparing for Premed Exams.

Participants and Design. Participants were pre-med students (N = 246, M_{age} = 20.03 years, SD = 2.08, 70.5% female) recruited for a 2-week diary study and received compensation for their time, up to $50 and several chances to win $250 lotteries. Participants who filled out at least one diary (*n* = 228) were included in the analyses. Prior to the diary portion of the study all participants completed a background questionnaire. They were then randomly assigned to 1 of 7 groups, each of which had a different start date ranging from eight to two days before the exam. Participants were sent links to the online survey the day before they were to begin filling out the survey. Group 1 began the diary on Day 8 before the exam and served as a comparison for the other six conditions. For this reason we allocated more participants to this group (using an 8 to 3 ratio relative to the other six groups) to increase power for these contrasts. Participants in Group 1 were also asked to complete 15 diaries instead of 14. The remaining 6 groups filled out diaries over 14 days and were randomized to different starting days, ranging from 7 to 2 days before the exam. Each day participants completed two diaries, one within an hour of waking, and one within an hour of going to bed in the evening. On average participants in the sample completed 13.61 morning

diaries (*SD* = 1.02) and 13.25 evening diaries (*SD* = 1.23). Participants did not differ in terms of their demographic characteristics across groups.

Measures. Study 2 included measures of mood as assessed in Study 1, an extended list of physical symptoms, and study time. The mood scales showed satisfactory reliability, with an average Cronbach's alpha of .80 for anxious mood and .71 for vigor, and average R_{Change} of .82 and .69 for anxiety and vigor, respectively. To assess physical symptoms, each evening participants indicated whether or not they experienced any of eight different physical symptoms (nausea/upset stomach, sore throat, insomnia, constipation/diarrhea, headache, back/muscle ache, rash/irritation, runny nose/congestion), coded as "1" if present and "0" if not. Responses were summed to create an index of the total number of physical symptoms experienced on a given day. To assess study time, participants reported the number of hours they spent studying in the past 24 hours in the evening diary.

Data Analysis. We estimated the IELD effect by comparing the reports on the first day of Groups 2 through 7 to the average of the reports of groups that started the diary earlier. For example, the first report of Group 2 was compared to the reports on the same day in Group 1, and the first report of Group 3 was compared to the average of the reports on that day from Groups 1 and 2. We used the MIXED procedure of SAS to fit a general linear model that adjusted for average level of each day before the exam and adjusted for repeated measures by specifying a Toeplitz structure with six bands on the residual correlation matrix. A-priori power analysis for this diary study was based on Study 1 and a smaller study of repeatedly assessed depression by Sharp and Gilbert (17) with an average effect size of d = .23. Using SAS PROC MIXED simulations to

detect first day effects vs. days distant from the exam, we calculated an intended sample size of 260 participants with an estimated standard error of 0.08 and 82% power to detect an effect of at least d=.23. The syntax and all the data used for Study 2 analyses are available at https://osf.io/jtdb8.

Study 3: IELD in a Panel Study of College Experiences.

Participants and Design. Undergraduates and their roommates were recruited from two urban private universities to participate in a longitudinal survey of "college life" over 8 months, n = 870, 742 (85.3%) of them recruited as roommate pairs, M_{age} = 18.9 (SD = 1.5), 77% female. Participants were randomly assigned to start the survey in October, December or February. Roommate pairs were stratified in the random assignment so that the starting months were uncorrelated and balanced. All participants were asked to complete February and April assessments. The order of the self vs. roommate interview sections was randomized.

Of 870 persons recruited, 800 participants (Self report n=455; roommate report n=345) filled out at least one follow-up survey and were included in the analyses. Participants were asked to complete up to four bi-monthly assessments describing their own or their roommate's (if enrolled in the study) psychological and physical health. For each pair of roommates, one target individual described herself/himself, and their roommate described the target individual. Participants received $10 per survey completed, for up to $50 (background questionnaire and four bi-monthly surveys) and had a chance to win one of five $250 lotteries across the course of the study.

Measures. As in Study 1, we investigate IELD in mood and physical symptoms, and included overall distress. Mood was assessed as described in Study 1. The

average scores for anxiety and vigor were relatively low (anxiety *M*: 0.78 ; vigor *M*: 1.24). The reliability of anxiety and vigor was adequate, *R* = 0.77 and 0.77, respectively. To assess physical symptoms, participants were asked to indicate the frequency of 14 physical symptoms over the past 6 weeks on a scale ranging from 0 (never) to 3 (nearly every day). The physical symptoms included headaches, asthma, cold/flu, nausea, and insomnia. The responses to the 14 items were averaged, with higher scores indicating more health symptoms. The average scores for physical symptoms was relatively low (*M* = 0.43). The reliability of physical symptoms was also adequate, *R* = 0.85. We assessed distress with 10 items from a short distress scale (15) (example item for reporting about self: “How often did you feel so nervous that nothing could calm you down?”; about roommate: ”How often did your roommate feel so nervous that nothing could calm him/her down?”). Participants rated how they or their roommate had been feeling over the past 6 weeks on a scale ranging from 0 (none of the time) to 4 (all of the time). The responses to the 10 items were averaged. The average scores for distress for participants reporting about themselves and those reporting about their roommate across experimental groups were relatively low (self: *M* = 0.92, roommate: *M* = 0.72). The reliability for self and roommate reports were *R* = 0.90 in both groups.

Data Analysis. We estimated the IELD effects by comparing the initial reports of participants who started the panel survey in December to those starting in October, and those starting in February with those who started in October or December. This was done with a multilevel model that included all available data; participants starting in October, December and February could contribute respectively four, three or two waves of data. The model adjusted for months near final exams (December and April) and a

random effect for the participant's average level of the outcome. The IELD effect itself was associated with a dummy variable that marked which survey point was first. The syntax and data for the analyses are available in supplemental material. We assumed smaller effect sizes in a panel study of the usual college experience with longer intervals than for a diary study of acute stress. We planned to recruit 810 participants, 270 per cell in a 3-group (start date October, December, February) experimental design to detect at least a small effect (*d* = .1) for the contrast between first and second interviews with 81% power. The syntax and all the data used for Study 3 analyses are available at https://osf.io/pdnma.

Study 4: IELD and Conversational Norms.

Participants and Design. Participants (N=141, Age: M = 18.8, SD = 1.4, 73.9% female) were recruited for a two-week diary study before a major exam with the same methods as in Study 2. Participants received compensation for their time up to $30. All participants were enrolled to complete two weeks of a daily diary starting 10 days before the exam (Day 1-10). After consent, they were randomly assigned to one of two conditions. In Condition 1, participants completed the 2-week diary as the "*Exam Preparation Study*." Condition 2 was designed to induce students to participate in two seemingly separate, consecutive 1-week diary studies run by two different research groups, in Week 1 as the "*Daily Health Study*," then in Week 2 as "*Exam Preparation Study*," each with a different study purpose, research group, online survey platform, and survey layout. The 141 students who agreed to participate were randomly assigned to Condition 1 and 2 (60 and 81 participants) at a ratio of 3 to 4 to compensate for expected drop-out in the 2-study condition. Of the 60 assigned to Condition 1, 53

followed the link and provided usable data over the two weeks. Of the 81 persons assigned to Condition 2, 43 agreed to enroll in the *Daily Health Study*. One week later, these participants were invited to start the *Exam Preparation Study*. All of those who enrolled in the *Daily Health Study*, agreed to start the *Exam Preparation Study* in Week 2. We call these 43 persons Condition 2a. Of the 38 participants who declined to participate in the *Daily Health Study* in Week 1, 58% (n=22) enrolled in the one-week version of the Exam Preparation Study. We call these Condition 2b. Because self-selection undermined the initial randomization, we checked whether the three groups (Conditions 1, 2a and 2b) differed in their age, gender, grade point average or major. None of these comparisons revealed statistically significant differences.

Regardless of condition, participants completed identical items for the two weeks (with additional health items in the *Daily Health Study*). After the 2-week diary period, all participants completed a background questionnaire that assessed in addition to demographic information the extent to which participants in Condition 2 were aware that the *Daily Health Study* and the *Exam Preparation Study* were actually identical diary surveys being run by one research team. Nine out of the 43 persons (21%) in Condition 2a suspected that the investigators of the *Exam Preparation Study* were the same as in the *Daily Health Study*. Eliminating these nine persons did not change the results.

Measures. Measures in Study 4 included mood, physical symptoms, and study time, as in Study 2. To make the Daily Health Study in Condition 2 more credible, we included more questions about health in Study 4 than in Studies 1-3. Participants answered a series of items that assessed the presence of a number of health issues over the course of the previous day, asking “How often during the past 24 hours have

you experienced the following health problems?" During week 1, participants in each condition responded to an extended checklist that included additional items assessing more differentiated headache symptoms (e.g., tension headache, migraine, sinus headache). However, the physical symptoms variable used in this analysis is based on the same eight items used in Study 2.

Data Analysis. To test the hypothesis that starting a new study would induce an IELD effect, we compared the level of responses on day 8 of group 1 (the two-week Exam Prep Study group) to the day 8 responses of group 2a (the group that completed seven days of the health study, and started a one week Exam Prep Study on day 8). The mean difference was tested with an independent sample *t test.* To estimate the within group presence of the IELD effect in group 1, we compared day 8 to day 1 and tested the difference with a paired *t test*. To test the IELD effect of group 2b, which started a one-week long Exam Prep Study without having completed the one week health study, we compared their first day of reports to day 8 of group 1 using an independent sample *t test.* Following guidance by Cohen (15), a sample size of 140 participants was calculated to detect a medium effect size of d = .5 comparing the two experimental groups (One-study condition vs. two-study condition). The syntax and all the data used for Study 4 analyses are available at https://osf.io/y2r7s.

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Table 1: Estimates of IELD Effects for Examinees and Partners Based on Comparison of Diary Days (Within Person) and of Diary and Panel Groups (Between Person) in Study 1.

	Within Person Comparisons of Day 1 & 8				Panel vs. Diary Conditions at Day 22				Panel vs. Diary Conditions at Day 35				Panel vs. Diary Conditions at Day 44			
	Effect Size		LB	UB	Effect Size		LB	UB	Effect Size		LB	UB	Effect Size		LB	UB
Examinee																
Anxious Mood AM	0.47	***	0.36	0.58	0.82	***	0.52	1.12	0.31		-0.28	0.89	0.84	***	0.41	1.27
Anxious Mood PM	0.26	***	0.15	0.37	0.35	*	0.05	0.66	-0.20		-0.79	0.40	0.58	**	0.14	1.01
Vigorous Mood AM	0.09		-0.03	0.21	0.07		-0.20	0.34	0.44	+	-0.01	0.89	-0.17		-0.60	0.26
Vigorous Mood PM	0.10	+	-0.01	0.21	0.01		-0.25	0.27	0.06		-0.40	0.52	-0.20		-0.63	0.23
Physical Symptoms PM	0.39	***	0.25	0.54	0.80	***	0.53	1.08	0.79	***	0.21	1.37	0.88	***	0.48	1.27
Study Time PM	-0.02		-0.13	0.09	0.01		-0.26	0.28	-0.47	+	-1.01	0.08	0.26		-0.24	0.76
Partner																
Anxious Mood AM	0.47	***	0.33	0.61	0.96	***	0.68	1.25	1.09	***	0.57	1.61	0.49	*	0.07	0.91
Anxious Mood PM	0.14	*	0.00	0.29	0.64	***	0.34	0.94	0.64	*	0.05	1.22	0.55	**	0.14	0.97
Vigorous Mood AM	0.10		-0.03	0.23	0.20		-0.09	0.49	0.24		-0.21	0.68	-0.03		-0.45	0.40
Vigorous Mood PM	0.18	*	0.04	0.31	0.11		-0.18	0.40	0.24		-0.21	0.69	-0.35		-0.77	0.08
Physical Symptom PM	0.50	***	0.33	0.66	0.92	***	0.60	1.24	0.60	**	0.15	1.05	0.64	***	0.25	1.03

Notes: Effect sizes are in Cohen's *d* metric. Significance levels: * *P<.05*; ** *P<.01;* *** *P<.001.* LB and UB are lower and upper bounds of 95% confidence intervals. Degrees of freedom for one sample t tests of within-person comparisons were on degrees of freedom that ranged from 271 to 288. Degrees of freedom for two sample t-tests of between-group comparisons ranged from 312 to 332 (Day 22 comparison) 237 to 278 (Day 35 comparison), and 258 to 263 (Day 33 condition).

Table 2: Study 2 IELD Effect Estimates for Diary Self-reports of College Students Prior to Science Examination

	Effect		LB	UB
Anxiety AM	0.56	***	0.38	0.75
Anxiety PM	0.10		-0.10	0.30
Vigor AM	0.11		-0.10	0.33
Vigor PM	0.38	**	0.15	0.60
Physical symptoms PM	0.34	***	0.18	0.50
Study time PM	0.43	***	0.24	0.63

Notes: Effect sizes are in Cohen's *d* metric and were estimated in a generalized linear model that compared first reports to the average of reports of participants whose report was not first. The model adjusted for position of day relative to exam and for correlated residuals. Significance levels: ** *P<.01;* *** *P<.001.* LB and UB are lower and upper bounds of 95% confidence intervals. Degrees of freedom for tests were 223.

Table 3: Study 3 IELD Effect Estimates for College Students' Self-reports and for Reports on Roommates

	Effect size		LB	UB
Participant self report				
Anxious mood (current)	0.16	***	0.05	0.27
Vigorous mood (current)	0.15	**	0.05	0.26
Physical symptoms (six weeks)	0.18	***	0.11	0.26
K10 Mental distress (six weeks)	0.29	***	0.21	0.37
Participant Report on Roommate				
K10 Mental distress (six weeks)	0.13	***	0.03	0.23

Notes: Effect sizes are in Cohen's *d* metric and were estimated in a linear mixed model that compared first reports to the average of reports of participants whose report was not first. The model adjusted for early/late in semester and random intercepts. Significance levels: ** *P<.01;* *** *P<.001.* LB and UB are lower and upper bounds of 95% confidence intervals. The participant results were based on N=455 and the roommate results were based on N=345.

Table 4: Results from Study 4: One versus Two Study Design

	Between: Group 2a vs 1			Within Group 1: Day 8 vs Day 1				Between: Group 2b vs 1			
	Effect	LB	UB	Effect		LB	UB	Effect		LB	UB
Anxiety PM	-0.17	-0.60	0.27	0.17		-0.16	0.50	0.56	*	0.00	1.12
Vigor PM	-0.11	-0.53	0.32	-0.09		-0.35	0.16	-0.02		-0.55	0.51
Physical symptoms PM	-0.04	-0.41	0.33	0.77	***	0.45	1.09	0.92	***	0.45	1.39
Study time PM	0.00	-0.45	0.45					-0.21		-0.77	0.34

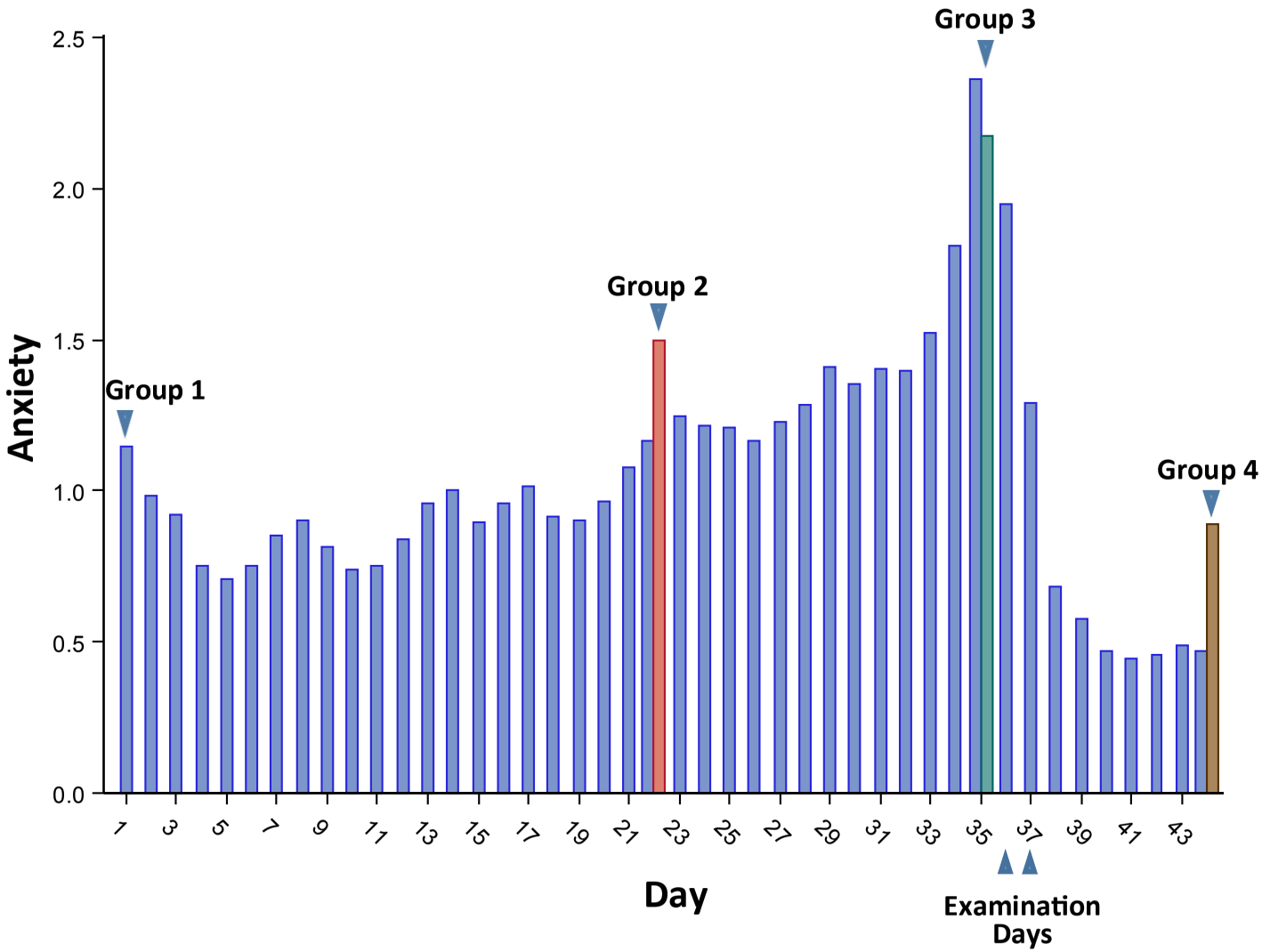
Notes: Effect sizes are in Cohen's *d* metric. Significance levels: ** *P<.01;* *** *P<.001.* LB and UB are lower and upper bounds of 95% confidence intervals. Group 1 completed a single Exam preparation study for 14 days. Group 2a completed one week of Health study and one week of Exam preparation study. Group 2b only completed 7 days of Exam preparation study. Comparisons of Group 2a and 1 were on 87 *df* for symptoms and 83 for study time. Within group t tests were on 46 *df*. Comparisons of Group 2b and 1 were on 68 *df* for symptoms and 66 for study time.

Figure 1

Evening anxiety in Study 1 over 44 days around 2-day state bar exam (Day 36-37) for examinees in 4 experimental conditions with different assessment start dates.

Figure 2

Anxiety and symptoms in Studies 2 and 4 over the days of the study in the experimental conditions with different assessment start dates.


Group 3
Group 2
Group 1
Group 4
Anxiety
2.5
2.0
1.5
1.0
0.5
0.0
1
3
5
7
9
11
13
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23
25
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29
31
33
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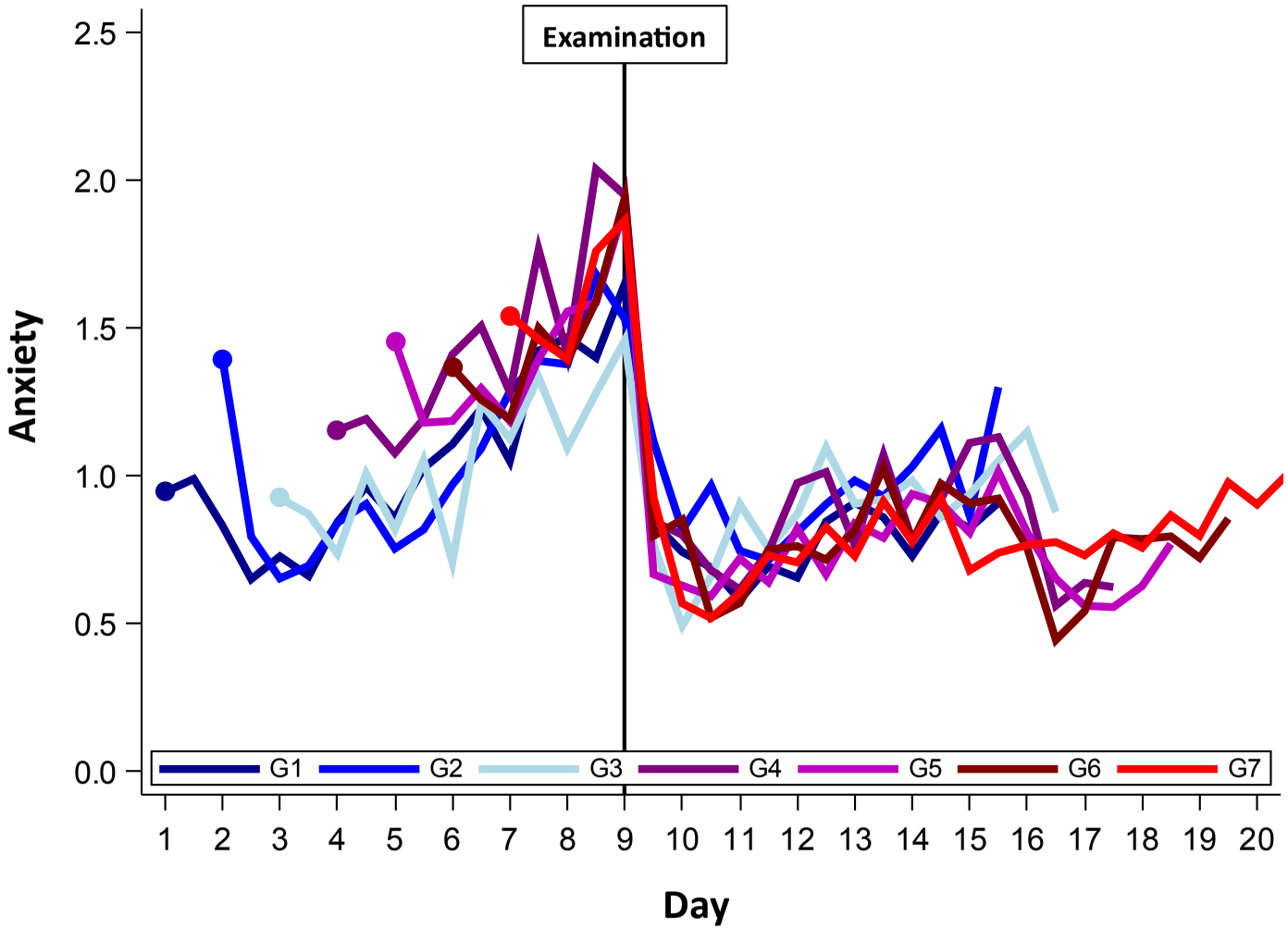
37
39
41
43
Day
Examination
Days

Study 2


Examination
Anxiety
2.5
2.0
1.5
1.0
0.5
0.0
G1
G2
G3
G4
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G6
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1
2
3
4
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11
12
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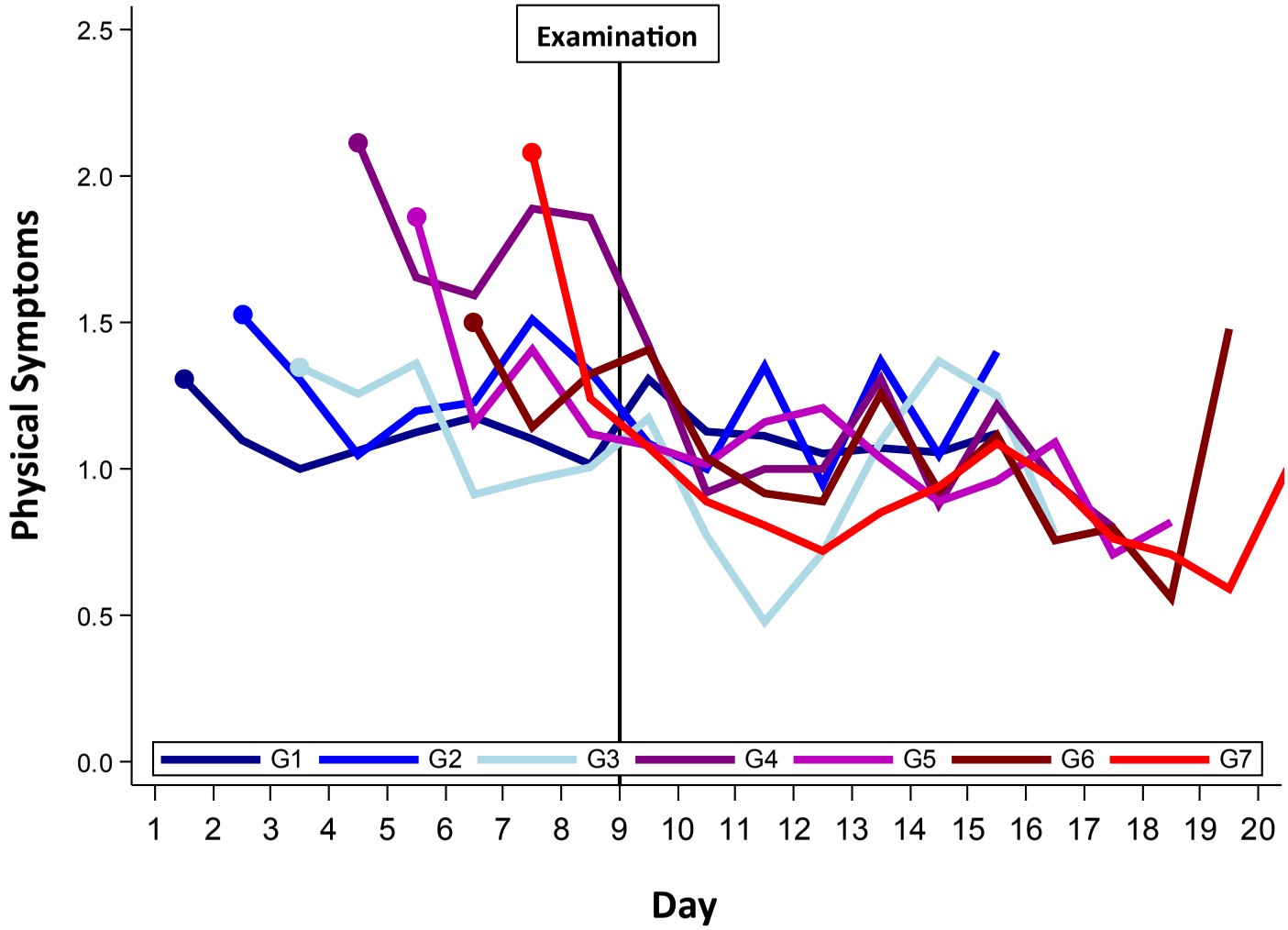
20
Day


Examination
Physical Symptoms
2.5
2.0
1.5
1.0
0.5
0.0
G1
G2
G3
G4
G5
G6
G7
1
2
3
4
5
6
7
8
9
10
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14
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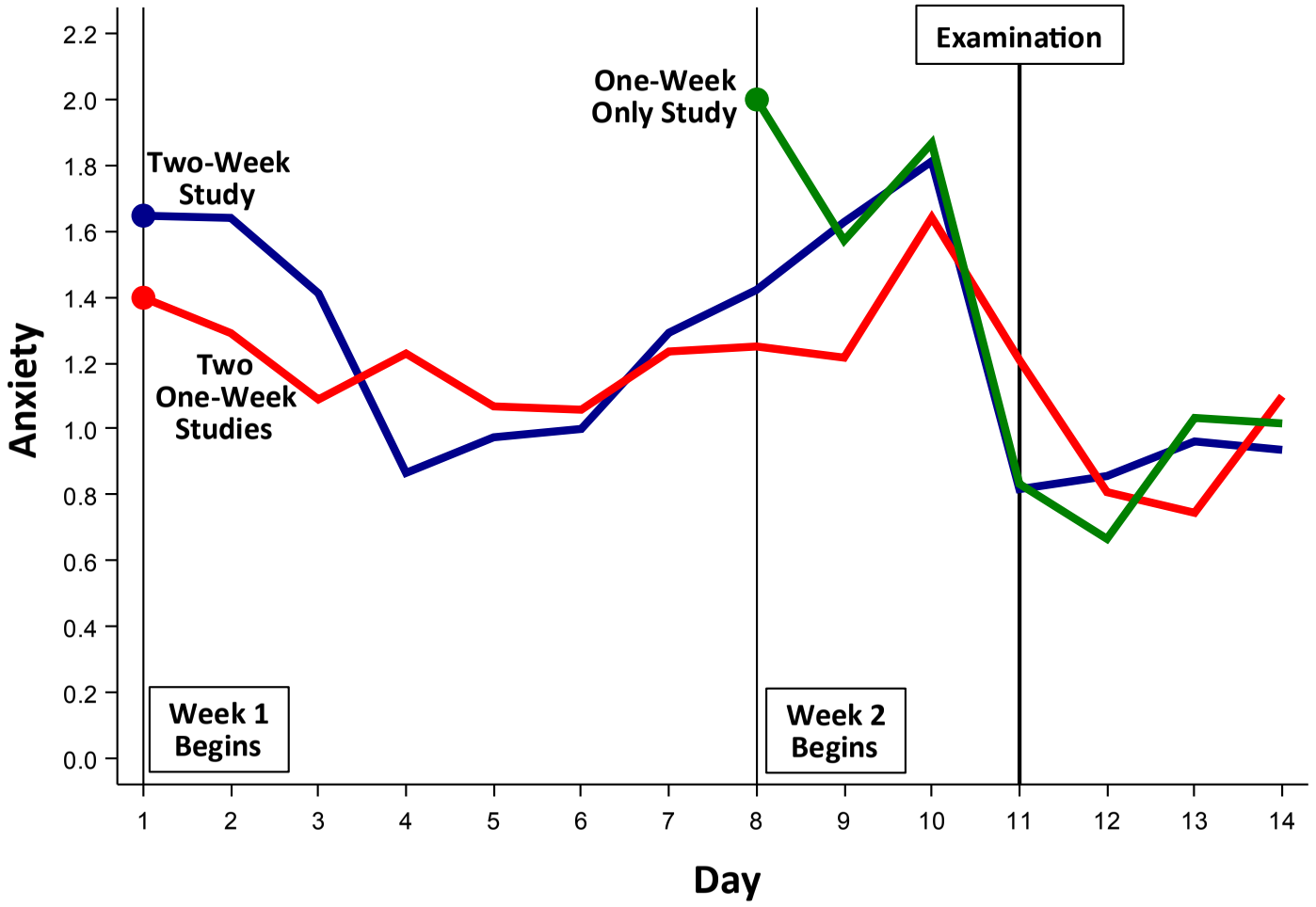
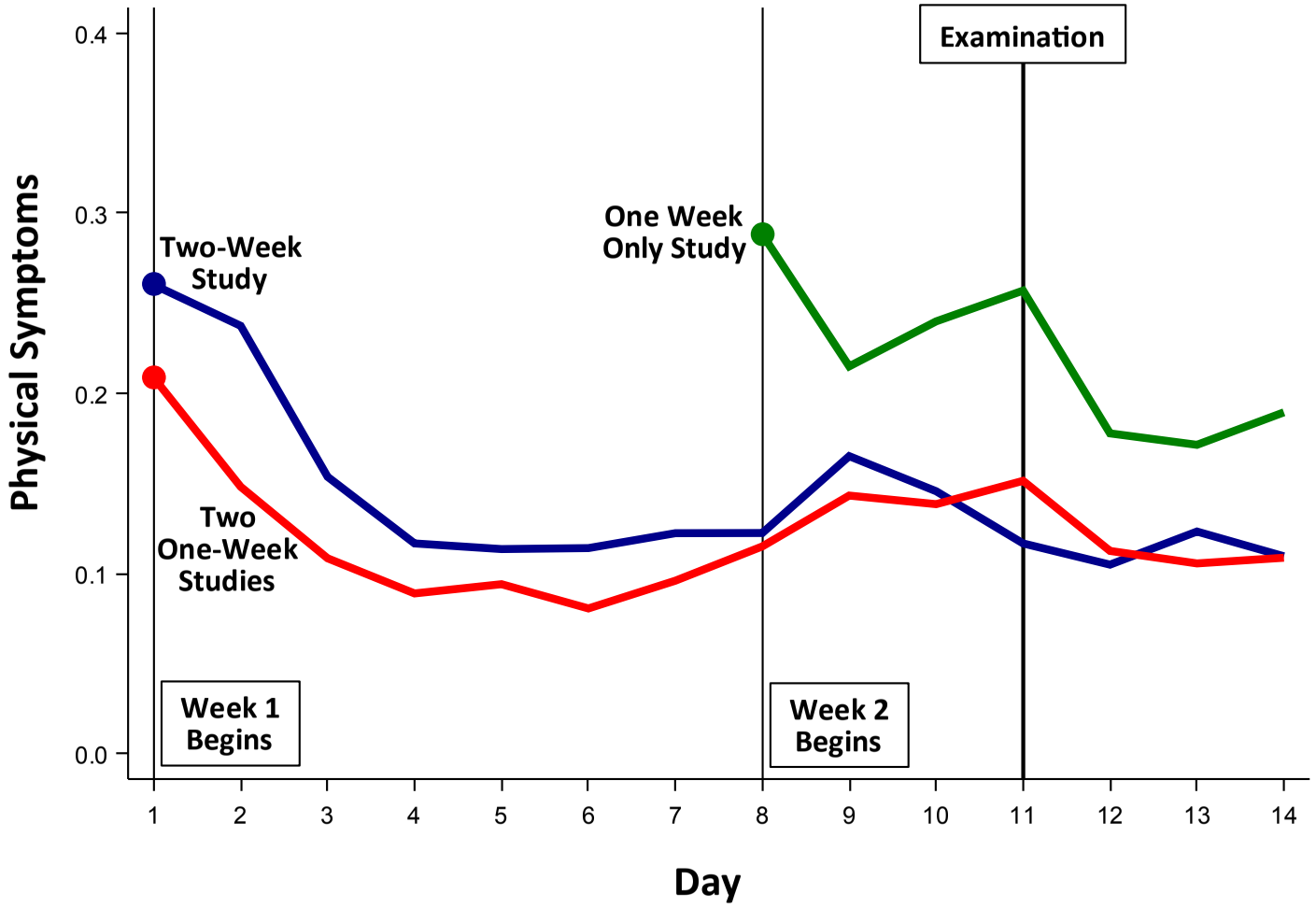
Day

Study 4


Examination
One-Week
Only Study
Two-Week
Study
Two
One-Week
Studies
Anxiety
2.2
2.0
1.8
1.6
1.4
1.2
1.0
0.8
0.6
0.4
0.2
0.0
Week 1
Begins
Week 2
Begins
1
2
3
4
5
6
7
8
9
10
11
12
13
14
Day


Physical Symptoms
0.4
0.3
0.2
0.1
0.0
Examination
One Week
Only Study
Two-Week
Study
Two
One-Week
Studies
Week 1
Begins
Week 2
Begins
1
2
3
4
5
6
7
8
9
10
11
12
13
14
Day