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The experience of agency in sequence production with altered auditory feedback

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ABSTRACT

When speaking or producing music, people rely in part on auditory feedback – the sounds associated with the performed action. Three experiments investigated the degree to which alterations of auditory feedback (AAF) during music performances influence the experience of agency (i.e., the sense that your actions led to auditory events) and the possible link between agency and the disruptive effect of AAF on production. Participants performed short novel melodies from memory on a keyboard. Auditory feedback during performances was manipulated with respect to its pitch contents and/or its synchrony with actions. Participants rated their experience of agency after each trial. In all experiments, AAF reduced judgments of agency across conditions. Performance was most disrupted (measured by error rates and slowing) when AAF led to an ambiguous experience of agency, suggesting that there may be some causal relationship between agency and disruption. However, analyses revealed that these two effects were probably independent. A control experiment verified that performers can make veridical judgments of agency.

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1. Introduction

In the motion picture *Indiana Jones and the Last Crusade* (1989), there is a scene where Jones must break through a stone floor using a large metal object in the middle of a quiet Venetian library. He slams the object against the floor, producing a very loud sound, at exactly the same moment that a nearby librarian stamps a book. The librarian is perplexed and immediately looks around for another possible cause of the sound, but cannot see Jones or any other possible cause. The librarian stamps the book again, and again the sound from Jones' action coincides with his own. After a third attempt, and a third coincidence of actions, the bewildered librarian puts down his stamp. The librarian's confusion arose because the chance synchronization of his actions with those of Jones caused him to conceptualize the auditory events produced by Jones as being the result of his actions, despite the obvious discrepancy between the expected and experienced auditory event with respect to loudness. The situation was thus ambiguous with respect to the experience of agency.

Several psychological factors probably influenced the librarian's experience of agency – his feeling that he was the cause of the sounds he perceived. Sensorimotor cues (Sato, 2009) – the moving of arm muscles, the abrupt stop when the stamp hit the book, etc. – certainly informed him that he was engaged in the act of stamping. Like most people, he would also have expected a certain sound to result from this act (Levelt, 1989; MacKay, 1987). What he actually heard was not consistent with his expectations, but was similar enough in content and synchrony to produce some feeling of agency. He also made an important inference. Using his knowledge of libraries, stamping, and the visual information about what the people in his immediate vicinity were doing, he determined that there was no other plausible cause of the sound. He thus inferred that he was the most plausible cause. These various cues resulted in a feeling of agency that was not strong enough to completely

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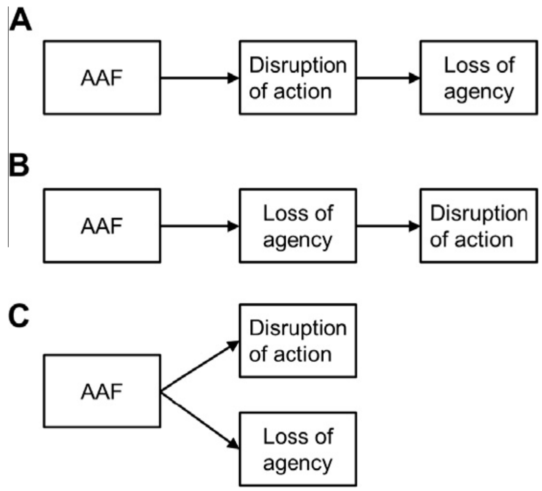

Fig. 1. A conceptual representation of three ways in which AAF might influence agency and performance. (A) The effect of AAF on agency is mediated by its disruptive effect. (B) The disruptive effect of AAF is mediated by the reduced sense of agency. (C) Agency and disruption are two separate effects of AAF.

convince him that he was the cause, but not weak enough to make him believe that he was not the cause. His experience illustrates some of the psychological factors involved in the feeling of agency, and also what might result when the auditory feedback associated with an action is altered.

In this article, we explore a similar kind of agency-related ambiguity that can occur when people produce sequences of actions for which accuracy and timing are critical. Specifically, we examine how alterations of auditory feedback (AAF) influence judgments of agency in piano playing. The AAF paradigm is comparable to the vignette described above, in that the timing and/or contents of auditory events that follow an action may not match what the performer expects to hear. At the same time, during AAF tasks (as in the example above), some cues for agency are usually retained. Many papers have documented the fact that AAF can severely disrupt production, leading to increased errors and slowing of production rate (e.g., Black, 1951; Finney, 1997; Gates, Bradshaw, & Nettleton, 1974; Howell, 2004; Howell, Powell, & Khan, 1983; Lee, 1950, 1951; Pfordresher, 2003, 2005, 2008; Pfordresher & Palmer, 2002, 2006). However, to our knowledge none have addressed how AAF influences the experience of agency, or the way in which agency and disruption may be related. As such, the AAF paradigm offers a new way to investigate the relationship between agency and fluency in production.

Fig. 1 shows three ways to conceptualize the way in which AAF influences agency and performance. Fig. 1A follows from the assumption that the loss of agency (possibly) associated with AAF is mediated by the disruptive effect of AAF on performance. In other words, AAF leads to disruption which in turn leads to a reduced sense of agency. Fig. 1B presents the reverse situation; here the disruptive effect of AAF is mediated by the way in which AAF influences agency. Thus, one might expect that participants who are better able to maintain a sense of agency during AAF may be less prone to disruption than are those who more easily lose the sense of agency. Finally, Fig. 1C shows a framework in which agency and disruption are two separate effects of AAF. This final model follows from recent research suggesting that auditory processes involved in awareness may be distinct from auditory processes used to guide action (cf. Loui, Guenther, Mathys, & Schlaug, 2008).

In the following introduction, we review research on the AAF paradigm and related research on the experience of agency.

1.1. The AAF paradigm

The AAF paradigm is used to explore the importance of auditory information in the production of action sequences. The best known form of AAF is delayed auditory feedback, which usually involves delaying the onset of feedback events by a fixed amount of time (Black, 1951; Lee, 1950, 1951). In most circumstances, delayed auditory feedback, while leading to asynchronies between perception and action, preserves the contents of anticipated feedback events (i.e., the participant hears the event category – pitch class or syllable – that was expected to follow the action). A related form of AAF that we use here, in which the feedback delay is not constant, is referred to as *asynchronous* auditory feedback. Other kinds of AAF change the contents of auditory feedback while maintaining synchrony. Such alterations have primarily been used in musical tasks involving an electric keyboard and include manipulations such as randomizing pitch (Finney, 1997; Pfordresher, 2005), shifting pitches such that participants hear events intended for other serial positions (Pfordresher, 2003, 2005, 2008; Pfordresher & Palmer, 2006), or presenting variations of produced melodies (Pfordresher, 2008).

Different kinds of AAF can lead to disruption that varies in type and magnitude. With respect to type, alterations of feedback synchrony disrupt the production of timing but do not disrupt accuracy; conversely, alterations of pitch can disrupt accuracy in production but do not disrupt timing (Pfordresher, 2003). With respect to magnitude, the disruptive effect of asynchronous AAF depends on when it occurs during the inter-onset interval (IOI) – the time between the beginning of each performed event – and is closely related to movement patterns (Pfordresher & Benitez, 2007; Pfordresher & Dalla Bella, 2011). The magnitude of disruption for alterations of pitch functions differently. The most disruptive alterations result from AAF manipulations that lead to an intermediate level of similarity between the produced sequence and the sequence of auditory feedback events. For instance, randomized pitch feedback – a sequence of notes uncorrelated with performance – does not disrupt production (Finney, 1997; Pfordresher, 2005). By contrast, *serial shifts* of feedback pitch are disruptive (Pfordresher, 2003, 2005; Pfordresher & Palmer, 2006; for a similar alteration in speech see Müller, Aschersleben, Esser, & Müsseler, 2000); these events result in the participant hearing feedback associated with an event from a fixed serial distance in the past or future (e.g., always hearing the next note in the sequence) and thus maintain the sequential organization of produced and perceived sequences while altering their serial relationships. Finally, combining alterations of synchrony and contents, paradoxically, reduces disruption relative to the presentation of each type of alteration alone (Pfordresher, 2003).

This raises the question: Does the term "feedback" truly apply to the effects of AAF? This terminology arose during the 1950s, when the disruptive effects of AAF were first discovered. At that time, cybernetic theories of feedback control were popular and as such the term "feedback" in the context of AAF constitutes an explicit theoretical stance. However, subsequent research sheds doubt on whether such a theoretical framework applies. The most convincing evidence against the "feedback" concept was produced by Howell and colleagues who demonstrated that transforming the speech signal to a non-speech signal (e.g., a square-wave tone) did not diminish the effect of AAF (Howell & Archer, 1984; Howell et al., 1983). Under such circumstances, the "feedback" signal clearly does not match the intended outcomes of action with respect to content, and thus is highly unlikely to function as an error signal. It has also been pointed out that time lags involved in the loop linking actions and feedback are too long in latency for feedback control to guide speech (Borden, 1979). Such observations have led to the idea that AAF disruption is not specific to interpreting auditory information as feedback, but is instead representative of a broader class of sensorimotor interactions in which auditory information can interfere with action planning (such as playing in a musical ensemble, Howell et al., 1983).

The experiments reported here tested a corollary of this idea: If AAF does not represent feedback, does one experience AAF as if it were produced by an external agent? We addressed these questions by comparing the effects of AAF on sequence production with its effects on the experience of agency, which in this context is the performer's subjective experience that he or she has caused the auditory events that follow his or her actions.

1.2. Agency and motor behavior

To date, research concerning the role of agency in sensorimotor interactions has focused on tasks that involve discrete responses rather than action sequences. Based on these kinds of results, Wegner (2002) has argued for three inferential cues that enhance the experience of agency: priority, consistency, and exclusivity. These cues deal with the relationship between the intended and actual outcomes of an action. Priority requires that the intention occur before the actual consequences. Consistency requires that the intention be consistent with the actual consequences. Exclusivity requires no plausible alternative cause of the events other than the intended action. If these conditions are met, the theory states that a person will feel a strong sense of agency, even in cases when they did not in fact cause the event in question to occur (Wegner, Fuller, & Sparrow, 2003; Wegner, Sparrow, & Winerman, 2004; Wegner & Wheatley, 1999). At first glance, it may seem implausible to relate intentions to motor behaviors like music performance. However, Pacherie (2008) has recently introduced a theory of action phenomenology that suggests three hierarchical levels of intentionality, the lowest of which involves selection and control of motor actions. This framework thus brings up the issue of what sort of cues may regulate the link from intention to outcome at the level of motor behaviors.

With respect to motor actions, a critical source of information for relating intentions to feedback is sensorimotor cues – the physical sensations associated with an action. Sensorimotor cues are thought to involve the use of forward models in production (Kawato, 1999; Wolpert, Miall, & Kawato, 1998). A forward model is formed by the anticipated perceived outcomes of a motor action, as is also assumed by the older ideomotor theory (James, 1890). The performer is thought to generate a forward model while planning a motor action. The forward model generates an efference (sensorimotor) copy – the anticipated perceptual outcome of an action – and uses it to inhibit matching sensory input. A popular example is the fact that one cannot tickle oneself, due to inhibition of the anticipated tactile input (Blakemore, Wolpert, & Frith, 2000). However, when there is a mismatch between the anticipated (efference copy) and actual outcomes of an action, input is enhanced and the perceiver experiences the input as an unanticipated event not controlled by himself or herself. This efference account differs somewhat from Wegner's inferential account because it relies on the online monitoring of sensorimotor cues more than on post-hoc inferences about thoughts and outcomes.

The hypothesis that efference cues and forward models play a strong role in the experience of agency has been advanced by studies that not only manipulate priority, consistency, and exclusivity, but also the sensorimotor cues associated with an action. For example, Sato (2009) found that participants obviously experienced stronger feelings of agency when they pressed a button and experienced the resulting consequences than when the experimenter pressed the button. However, even in the latter case, with sensorimotor cues absent, participants felt a stronger sense of self-agency when the consequences of the experimenter's action matched the participant's expectations. This suggests that both motor predictions and the conditions Wegner (2002) describes affect agency to some degree (Knoblich & Sebanz, 2005).

At least one study has explored the relationship between agency and AAF-like conditions. Sato and Yasuda (2005) had participants learn to associate a specific pitch event with a single button press. During trials, participants would press a button once and report their judgment of agency regarding the tone that followed this action. Similar to the AAF manipulations described above, auditory events could vary with respect to pitch (i.e., they could differ from the associated event) or synchrony. Both alterations decreased judgments of agency, and increases in asynchrony led to monotonic decreases in agency. However, one cannot determine the relationship between the disruptive effect of AAF and its influence on agency in tasks requiring a single discrete response, because there is very little (if any) disruption.

Other studies have looked at the role of efference cues for agency in sequential actions under conditions where the performer may or may not be in control of auditory information (Knoblich & Repp, 2009; Repp & Knoblich, 2007). These authors adopted a modification of the pseudo-synchronization paradigm (Fraisse, 1971); in different phases of each trial, participants tapped along with a series of tones that was generated independently of actions (i.e., a metronome) or constituted feedback from one's actions. Participants were able to discriminate these two phases to some extent, and further analyses suggested that discrepancies between the timing of actions and sounds played a critical role in these identifications. Thus, sensorimotor cues – synchronization of actions and sound – influenced agency. Similar discrepancies may occur in AAF tasks; however, unlike the paradigm of Repp and Knoblich, the AAF paradigm allows one also to address dependencies of sequence production on auditory events.

1.3. The current study

The experiments reported here differ from past studies in that the effect of AAF on agency was investigated during the production of complex action sequences: melodies performed on a keyboard. In sequence production, prior thoughts and expectations occur rapidly and are constantly changing. Consistency between expected and experienced events can vary in several ways (e.g., in pitch, synchrony, or some combination of the two). Mismatches between expected and experienced events are often partial. A given sequence may contain both mismatches caused by AAF *and* instances where expectations match perception because the participant adjusts expectations to match the AAF condition. Any one of these factors or any combination might prevent AAF from producing coherent and reliable feelings of agency.

In the experiments reported here we sought to answer three important questions. First, we tested whether AAF manipulations affect the experience of agency in sequence production. We predicted that agency would be reduced by increasingly disparate AAF manipulations. However, we did not know whether this reduction would be related monotonically to AAF, as it is in single-response tasks, or whether interactions between pitch and synchrony manipulations would emerge in the more complex sequence-production environment. In particular, we were interested in whether certain AAF conditions that do not disrupt production, such as the presentation of randomly selected pitches, likewise fail to influence the experience of agency.

Second, we tested the degree to which the experience of agency is related to the disruptive effect of AAF in sequence-production tasks by comparing agency ratings to production disruption. To further explore this second question we experimentally manipulated the condition of exclusivity in Experiment 2 by having participants perform alone or with an accompanist – a confederate (J.J.C.) who played the same melody as the participant on a separate keyboard. Although Experiments 1 and 2 only ever used a single melodic pattern that originated from the participants' own performance, the presence of an accompanist created a plausible violation of exclusivity designed to reduce agency. If inferential cues for agency modulate the disruptive effects of AAF, then the belief in a plausible alternative source of feedback ought to change the pattern of disruptive effects in a significant way.

Third, we asked in Experiment 3 whether participants could actually tell whether events were self-caused or not. This was accomplished by asking participants to apply the rating scale used in Experiments 1 and 2 to streams of altered auditory feedback that either originated from the participant or from a recording of a previous participant.

If agency and performance are associated in a causal chain (as in the frameworks outlined in Fig. 1A and B one might expect a monotonic relationship between agency and performance across feedback conditions. However, it is also possible that effects of AAF on agency and performance follow different functions, which is closer to the scenario shown in Fig. 1C. Moreover, we adopt statistical and experimental procedures to try to determine whether the effect of AAF on disruption is mediated by its effect on agency, or vice versa.

2. Experiment 1

Participants performed short, previously unfamiliar melodies from memory on a keyboard while listening to auditory feedback over headphones. On certain trials, auditory feedback could be altered with respect to its pitch contents and/or its synchronization with keypresses. After experiencing normal or altered feedback on a trial, participants rated their experience of agency for that trial. We predicted that agency would vary with respect to the relatedness between produced actions and the auditory feedback sequence.

2.1. Method

2.1.1. Participants

Eighteen students (12 women, 6 men) from the University at Buffalo participated in exchange for course credit for an introductory psychology class. The mean age of participants was 20 years. Seventeen participants reported being right-handed, one was left-handed. Participants averaged 0.88 years of piano training, and 4.29 years of total training on any instrument. Only one participant had more than 1 year of piano training (13 years experience during childhood; the participant's results and verbal reports did not significantly differ from the others). None of the participants reported having absolute pitch, nor did they report any hearing problems.

2.1.2. Materials

Each participant played one of two possible melodies for the entire session. Each melody comprised eight events and was created so that mapping between fingers and piano keys was invariant. Melodies were generated by sampling from five possible pitch classes, corresponding to the white piano keys C4–G4. One melody began on C4 and initially ascended, whereas the other began on G4 and initially descended. One was characterized by an alternating up–down melodic contour (fingering 5–3–4–2–1–3–2–4); the other was created to have a smooth contour with fewer changes (fingering 1–2–3–5–4–3–2–3). Melodies were isochronous and were performed with the right hand. These were displayed as a row of numbers corresponding to the fingers with which the participant pressed sequential piano keys. Above each number in this row was a drawing of a hand with the to-be-moved finger highlighted (see Pfordresher (2005), for full details). On the keyboard, the numbers 1–5 were arranged in a row above the corresponding piano keys, with arrows pointing from the number down to the requisite piano key.

2.1.3. Conditions and design

Experiment 1 included four pitch alteration conditions crossed with four synchrony alteration conditions. For each type of alteration, pitch or synchrony, AAF conditions were chosen to reflect a continuum of relatedness between the produced sequence and the sequence of feedback events.

Pitch alteration conditions were: normal feedback, pitch lags of 1, pitch lags of 7, and random pitches. Pitch lags were manipulated such that when a participant pressed a key, they heard the pitch corresponding to the note they had played N notes earlier in the sequence. Lags were accomplished by storing previous notes in a buffer and, with each key press, providing a feedback pitch corresponding to the pitch stored at the corresponding position in the buffer. Because the melody was eight notes long, in an error-free performance a pitch lag of 7 would present the participant with the pitch corresponding to the immediately forthcoming note in the sequence (similar to the +1 "prelay" condition of Pfordresher & Palmer, 2006). Note that lags 1 and 7 maintain sequence structure and thus maintain similarity between perception and action with respect to structure, while the random pitch condition creates dissimilarity with respect to the current note and the sequence structure. Lags 1 and 7 were designed to be very similar, with each condition presenting a note that is one position away from the current key press; the only difference is that lags of 1 present past notes while lags of 7 present notes that are about to be played.

Synchrony conditions included a baseline synchronous feedback condition and three asynchronous feedback conditions. Two of the asynchronous feedback conditions involved delayed feedback by a fixed percentage of the mean of the two most recent IOIs, 25% or 75%. The third asynchronous condition was termed a random delay condition and involved presenting a delay drawn randomly from 100, 200, 300, or 400 ms after each keypress. Note that for 400 ms IOIs, 25% = 100 ms while 75% = 300 ms. As with pitch alterations, these alterations of synchrony were considered to represent different degrees of relatedness between the produced sequence and feedback sequence with respect to synchronization, with the random condition representing maximal dissimilarity.

Eight repetitions of each trial type, organized into eight blocks with different random orders of the 16 conditions, resulted in 128 experimental trials.

2.1.4. Apparatus

Participants performed the melodies on an M-AUDIO Keystation 49e unweighted-key piano set on a keyboard stand at a comfortable level (32 in. above the ground for all participants). Presentation of auditory feedback and metronome pulses as well as MIDI data acquisition were implemented using the FTAP software program (Finney, 2001) on a Linux operating system with millisecond resolution. Participants heard auditory feedback and metronome pulses over SONY MDR-7506 headphones at a comfortable listening level. The piano timbre originated from Program #1 (Standard Concert Piano 1) of a Roland RD-700 digital piano.

2.1.5. Procedure

At the beginning of each session, participants were acquainted with the hand position and piano keys that would be used in the experiment, and with the music notation described earlier. Participants practiced the melody with normal feedback until they could perform it from memory without errors for at least three successive repetitions. Music notation was then removed for the rest of the experiment. No participant appeared to forget the melody and no participant asked to see the notation again (though they were permitted to do so).

Participants were instructed to play what they had learned regardless of the feedback they heard, and also to pay attention to whether they felt like the feedback was the result of their own actions or not. They were told, "Sometimes what you hear might feel like it was produced by you, and sometimes it might not. After each trial, make the [agency] rating based on how strongly you feel like the sound was produced or not produced by you."

Participants then performed a practice trial with normal feedback (no pitch lags and no delay) to acquaint them with the metronome and trial structure, followed by a practice trial with random pitches and random delays to acquaint them with the nature of altered feedback. Each trial was preceded by four metronome clicks, separated by 400 ms. No metronome clicks occurred during performance. After the clicks, participants played the melody three times (without pausing) at a self-selected moderate tempo. Participants were instructed to play with consistent timing. Though participants were not explicitly instructed to follow the metronome tempo most appeared to do so (mean IOI across participants and trials = 407 ms, SD = 75 ms). During the first repetition of the melody, which comprised the first eight key presses, no auditory feedback was given. This was done to seed the buffer with pitches (see above). During the next two repetitions, which comprised the rest of the trial, auditory feedback corresponded to the trial condition. For all altered pitch conditions, each keystroke triggered a feedback pitch selected from the buffer.

After each trial, participants heard a MIDI tone that cued them to make their agency rating. The piano keyboard was labeled "ME" on the far right end, and "NOT ME" on the far left. Participants made the rating by pressing any of the 29 white keys present on the electronic keyboard, corresponding to how strongly they felt that they had (or had not) produced the feedback they heard. No auditory feedback was generated from this response.

At the end of the experiment, participants were asked, "What made you rate what you heard as coming from you or not from you?" Participants were not explicitly asked about pitch alterations, synchrony alterations, or about whether they used inference or sensorimotor cues so that they were free to report any factors that went into their rating.

2.1.6. Data analysis

Effects of melody were not analyzed because both melodies produced similar results.

Agency ratings were transformed into a percentage. Transformation was accomplished by numbering the white keys sequentially, then using the formula: Key pressed/total number of keys. This was performed in order to adjust for the variable spacing of white keys on the keyboard. Due to an error in the program that coded responses, agency ratings were not collected on certain randomly selected trials (17% in Experiment 1 and 20% in Experiment 2). However, because each combination of participant and condition yielded eight trials that we pooled across, these missing data did not complicate our analyses of variance. In analyzing measures of performance, we removed all trials that lacked an agency rating. Only 1 participant, in Experiment 1, needed to be eliminated from analysis due to having too many of these errors.

We focused on two performance measures: Mean IOI and key-press error rates. Means of produced IOIs measure performance tempo and are larger (slower) when AAF disrupts performance timing. We removed IOIs that were outliers (±3 standard deviations from the mean IOI within a trial), and IOIs associated with errors and tempo drift. Key-press errors were detected with software that compared the produced sequence of pitches with the sequence that would result from an accurate performance (Large, 1993; Palmer & van de Sande, 1993, 1995).

Debriefing responses were coded based on whether participants reported using pitch cues ("the sound of the note", "how high or low it was", etc.) or delay cues ("the timing of the notes", "how fast or slow it was", etc.), and whether they seemed to be using inference cues (pitch or delay factors with reference to other possible causes, such as the experimenter or a computer) or sensorimotor cues (pitch and delay factors without reference to possible other causes).

2.2. Results

2.2.1. Agency ratings

We first consider the way in which AAF influenced the experience of agency. Fig. 2A shows the effect of pitch and synchrony conditions on ratings of agency. Agency ratings were analyzed with a 4 (pitch alteration: normal, lag 1, lag 7, random) $\times$ 4 (synchrony alteration: normal, 25%, 75%, random) within-participants analysis of variance (ANOVA). This analysis yielded a significant main effect of pitch alteration, $F(3,48) = 12.61$, $p < .001$, $\eta_p^2 = .39$, a significant main effect of synchrony alterations, $F(3,48) = 37.46$, $p < .001$, $\eta_p^2 = .70$ and a significant pitch x synchrony interaction, $F(9,144) = 2.68$, $p < .01$, $\eta_p^2 = .14$. A Tukey's HSD test on the main effect of pitch alteration ($\alpha = 0.05$) revealed a reliable difference between the normal and random pitch conditions, whereas other contrasts were not reliable. A similar post-hoc analysis on the main effect of synchrony alteration revealed that 75% and random delays were rated lower than either normal or 25% delays. The two-way interaction reflected two subtler outcomes. First, as perception/action similarity diminished according to one experimental factor, differences across the levels of the alternate factor decreased. For instance, when auditory feedback was synchronous, the range of responses to different pitch alterations spanned 40%, whereas when the synchronization of auditory feedback was (quasi) random, differences as a function of pitch alteration only spanned 22%. A second trend leading to the interaction is the fact that one of the 16 cells, the cell associated with a 75% time alteration combined with a lag 7 pitch alteration, led to higher agency than would be predicted by the other data points. This deviation may not simply represent sampling error, but rather may reflect the fact that this condition in most cases would lead the feedback pitch associated with the current key press to fall just slightly before the produced onset (see discussion of the lag 7 condition in Section 2.1.3).

One potential question that emerges in analyzing agency ratings is the extent to which performance deviations within a trial may have influenced agency ratings. As demonstrated by Repp and Knoblich (2007), aberrations in production can act as cues regarding whether perceptual events are or are not feedback. In that study, the authors were primarily interested in timing. We considered pitch errors to be more problematic here because hearing an error as part of a lagged pitch sequence could strongly indicate to the participant that he or she in fact produced the pitch sequence. Note that such responses would run contrary to our instructions, which were to respond to the subjective experience of agency rather than awareness of actual agency. Nevertheless, we re-analyzed agency ratings after removing any trials with errors. Error rates were low (see below) and so doing this did not reduce power appreciably. The same pattern of results emerged, which suggests that

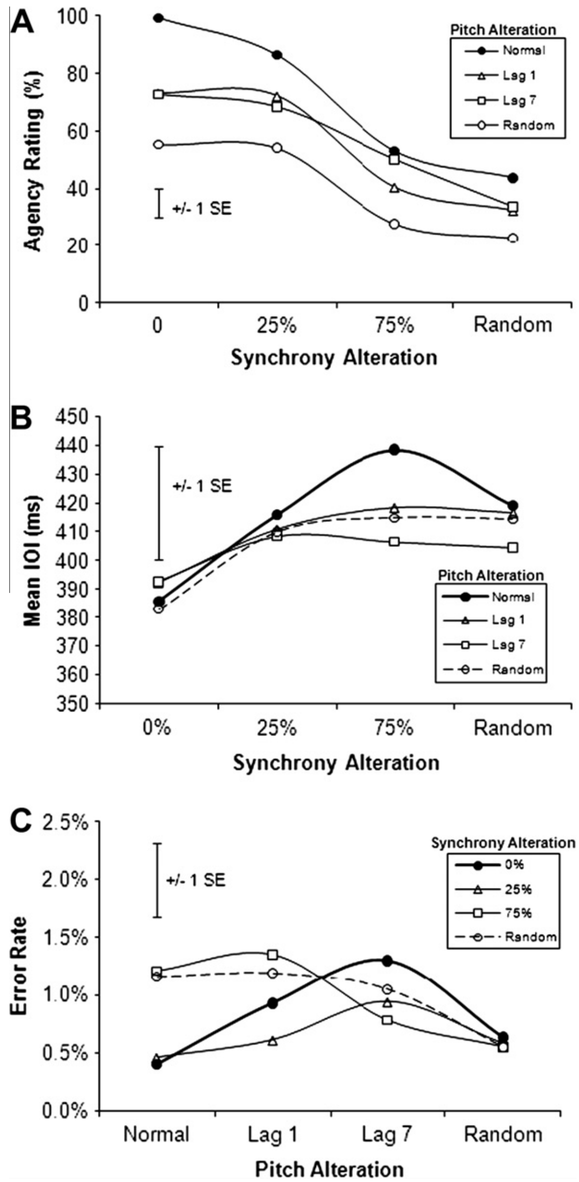

Fig. 2. (A) The effect of feedback synchrony and feedback pitch on ratings of agency in Experiment 1. (B) The effect of feedback synchrony on mean IOI. (C) The proportion of trials with any error. Error bars indicate ±1 SE, averaged across all 16 conditions in each plot. Panel (C) plots pitch alteration on the abscissa and uses synchrony alteration as the parameter because pitch alterations typically influence errors more so than do synchrony alterations.

participants were in fact responding to their subjective experience, as instructed. In support of this observation, we correlated cell means for trials with and without errors; the patterns of results were highly similar, $r^2 = .98$.

We also examined participant's accounts of their own ratings. All participants reported using sensorimotor cues to judge agency; no participant reported making any inference. Nine participants reported using pitch, 1 synchrony, 3 pitch and synchrony, and 5 reported using "sounds" and could not be more specific.

2.2.2. Disruption by AAF

We now turn to analyses concerning how altered feedback disrupted performance. We analyzed slowing of performance and pitch errors, to determine whether the conditions that caused lower agency ratings also caused these types of disruption. Fig. 2B shows the effect of different AAF conditions on mean IOIs. In the past, mean IOIs have been found primarily to be influenced by the synchrony of AAF and not alterations of pitch. The ANOVA yielded a main effect of synchrony, $F(3,48) = 6.08$, $p < .01$, $\eta_p^2 = .28$, a marginal main effect of pitch ($p = .09$, $\eta_p^2 = .13$), and a significant synchrony x pitch interaction, $F(9,144) = 1.98$, $p < .05$, $\eta_p^2 = .11$. With respect to the main effect of synchrony, the 75% and random delays caused significantly longer IOIs than synchronous feedback, whereas the 25% delay did not influence production significantly.

However, this difference was only apparent in the conditions with normal feedback pitches, which led to the significant interaction.

We also analyzed production errors resulting from AAF. In short trials like the ones used here, pitch errors were expected to be rare (cf. Pfordresher & Kulpa, 2011) and results bore this out, though we did find patterns of results like those seen in related research. Errors were measured using the mean proportion of incorrectly sequenced events on each trial. Mean error rates across conditions are shown in Fig. 2C. Note that this plot shows pitch, rather than synchrony, on the abscissa based on the fact that pitch alterations rather than synchrony alterations typically influence accuracy (Pfordresher, 2003, 2005). The two-way ANOVA on these measures did not yield any main effect or interactions. However, it can be seen that the lag-7 pitch condition increased errors considerably when feedback was synchronous.

2.2.3. Tests of mediation

Finally, we tested whether the experience of agency mediated the disruptive effect of AAF, or vice versa, according to the logic illustrated in Fig. 1. Given the complexity of the design, with multiple AAF manipulations and measures of disruption, which may share nonlinear relationships, traditional tests of mediation based on linear regression (e.g., Baron & Kenny, 1986) are difficult. Thus we adopted a slightly different procedure from standard mediation tests, which nonetheless applies the same logic. A critical test of mediation, according to Baron and Kenney, is whether the control of a mediator variable in a regression equation changes the relationship between a predictor and an outcome variable. In the context of the current experiment, mediation would cause the relationship between the combined AAF conditions and some outcome measure (e.g., ratings of agency) to change when we partial out a different effect of AAF (e.g., its effect on timing). To that end, we used detrending analyses as a way of addressing mediation.

We conducted four basic analyses that involved detrending. This was done by linearly regressing the dependent variable on the covariate and then adding the resulting residuals to the mean score across all conditions, leaving only variability due to the experimental manipulation and error variance. The first two analyses involved removing variability associated with each measure of disruption (error rates, mean IOIs) from ratings of agency, and then evaluating the effect of AAF conditions on the detrended agency data. Two further analyses resulted from removing variability associated with agency ratings from each measure of disruption. We also conducted four further analyses that removed variability associated with quadratic relationships, but the results of these analyses did not differ from those based on linear regression and so we will not report them here.

The removal of variability associated with disruption did not alter the effect of AAF on ratings of agency. In each case levels of significance did not change. Furthermore, regressions of the agency ratings for the 16 original feedback conditions on the same conditions after detrending suggested highly significant relationships ($r^2 > .99$ in both cases), with slopes close to one (.95 in each case). A similar result emerged when analyzing the effects of AAF on mean IOIs after removing variability associated with agency ratings. Significant main effects of each factor (but no interaction) remained, and the original data were strongly related to the detrended data ($r^2 = .92$, slope = .88). Error rates, which were not significantly influenced by AAF, retained non-significant effects after detrending and were significantly correlated with the original data ($r^2 = .72$, slope = .78).

2.3. Discussion

The results of Experiment 1 demonstrate the joint effects of AAF manipulations on performance fluency and on judgments of agency. Judgments of agency were clearly affected by altered pitch contents and synchrony of auditory feedback. Participants made higher judgments of agency during normal feedback than they did during pitch and synchrony alterations. Increased disparity between expected and actual consequences resulted in lower feelings of agency, and this effect was attenuated when conditions were combined (when the relationship between the sequence of actions and the feedback sequence was weaker). This suggests that, like single-response tasks, weaker feelings of agency result when expectations are violated by altered feedback in sequence production tasks.

The design of Experiment 1 was similar to the study by Sato and Yasuda (2005), mentioned earlier, in that both studies address the way in which perception/action disparities in both synchronization and expected feedback pitch can influence agency. As with that study, we found that both factors influence agency. However, our data are unlike those of Sato and Yasuda, in that we found both factors to interact, whereas they (in Experiment 1) reported independent effects. As noted earlier, our data suggest that deviations of one type (e.g. randomized pitch feedback) can attenuate the effect that alterations of a different type (synchronization) have on agency. This difference may be related to differences across tasks, in that we incorporated a sequence production task whereas Sato and Yasuda used a single-response task. However, further investigations are needed to explore this possibility; in particular, some data from Sato and Yasuda are more suggestive of interactive effects like those we report (e.g., their Experiment 2).

A more important difference between the present study and earlier work is that the use of a sequence production task allowed us to address the relationship between agency and disruption (i.e., errors and/or longer IOIs). Our data clearly suggest that decreases in agency do not lead to linearly scaled increases in disruption. Furthermore, detrending analyses (based on linear and nonlinear fits) suggested that the effect of AAF on judgments of agency is largely independent of its effect on performance and vice versa. Thus, the data support the framework suggested in Fig. 1C, which characterizes experience of agency and disruption as two independent effects of AAF. This finding suggest that the disruption that results from AAF is probably not caused by a reduced sense of agency.

3. Experiment 2

The purpose of Experiment 2 was to determine the effect of exclusivity on agency ratings and disruption. Specifically, an inference cue was added by having the experimenter (J.J.C.) appear to be a possible source of the feedback. On half the trials the experimenter acted as a kind of accompanist. He would perform the same melody as the participant in synchrony with the participant; however, only one feedback melody was heard, so the source of the feedback was ambiguous to the participant. If exclusivity effects agency in sequence production tasks, then a plausible alternative explanation for the sounds participants heard should lead to weaker effects of pitch and synchrony manipulations on agency (based on the logic of Wegner (2002)). Furthermore, if this inferential aspect of agency is causally related to disruption, then the manipulation should in turn influence the way in which AAF disrupts production. On the other hand, attributing disruptive content to another performer may enhance disruption. For instance, past research suggests disruption of timing when performers hear additional auditory events (possibly heard as if coming from another performer) between key presses (Loehr & Palmer, 2009).

3.1. Method

3.1.1. Participants

Eighteen students (11 women, 7 men) from the University at Buffalo participated in exchange for course credit for an introductory psychology class. The mean age of participants was 19 years. Participants averaged 2.44 years of piano training, and 4.50 years of total training on any instrument. Three participants had more than 6 years of piano training. None of the participants reported having absolute pitch, nor did they report any hearing problems. All participants reported being right-handed.

3.1.2. Materials, conditions, design, and apparatus

Auditory feedback conditions and melodies in Experiment 2 were identical to Experiment 1. However, in order to simplify the description of the results, we focus only on those conditions that led to the most reliable patterns of disruption in earlier research (e.g., Pfordresher, 2003) and in Experiment 1. These are conditions in which AAF of one type is combined with normal feedback on the other AAF dimension; the other conditions produced results similar to Experiment 1 (see Appendix for details). This partitioning of conditions led to a design involving two separate within-participants ANOVAs, one in which the independent variable is synchrony (normal, 25% delay, 75% delay, random delay) for conditions with normal pitch contents, and one in which the independent variable is manipulation of pitch (normal, lag 1 serial shift, lag 7 serial shift, random pitch) for conditions with synchronous auditory feedback. In addition, all trials were conducted with or without the confederate playing the role of accompanist. This variation led to the factor exclusivity, which was crossed with the AAF factor for each ANOVA.

3.1.3. Procedure

Participants were trained exactly as they were in Experiment 1, and the task of physically playing the melody while hearing the feedback and physically making the ratings was the same in both experiments.

The incorporation of the accompanist in Experiment 2, which was used to manipulate exclusivity, is illustrated in Fig. 3. Participants were seated so that they were facing a keyboard of the same model as the one they would play in the experiment. A music stand was placed between the two keyboards, on which the music notation was placed during training (as in Experiment 1) and was empty during the rest of the experiment. The stand was solid, black, and opaque. It was 13.5 in. high by 18.5 in. wide. It was tilted so that the participant could never see the experimenter's hand, but the experimenter could see the hand of the participant so that he could ensure that the participant was doing the task correctly. The experimenter's arm and body movements were visible to the participant. He varied the speed of his playing and body movements to correspond to the feedback, so that he would seem to be a plausible source of the feedback (e.g., his arm would move with each instance of feedback, whereas a participant's arm would move with each performed action). To further increase this plausibility, his eyes were focused on the participant's finger movements during trials, so that it appeared he was fully aware of what they were playing and could play the melody as similarly to them as he chose. The experimenter's keyboard was turned on, and an output cord ran from the keyboard toward the same location as the output cord of the participant's keyboard. Both cords ran to the back of the Roland-700, the inputs of which could not be seen by the participant (the experimenter's cord was in fact plugged into an unused jack on the Roland-700).

For half of the trials in Experiment 2 (quasi-randomly distributed) participants were in a situation similar to that of participants in Experiment 1. Like participants in Experiment 1, they were facing a keyboard and the experimenter (J.J.C.) was turned away from them looking at a computer screen. The only difference between this situation and Experiment 1 was that the experimenter's keyboard was present. The room was set up exactly as it was in Fig. 3 except that when the experimenter was not playing he was rotated 90° counterclockwise so that he was facing the table. During these trials, all procedures from Experiment 1 were followed.

For the other half of trials, the experimenter appeared to be an "accompanist" and all of the procedures described above were followed. Each condition was repeated 4 times when the participant was playing alone and 4 times when the

Fig. 3. An example of an experimental session during trials in which the accompanist appeared to be a possible source of feedback (duet trials) in Experiment 2. The music notation on the music stand was removed after training but the stand itself was not removed. Solo performance trials used essentially the same setup, except that the experimenter faced a computer that was oriented 90° counter-clockwise from the keyboard.

experimenter appeared to be playing. Thus, there were 4 trials × 16 types of feedback × 2 conditions, resulting in 128 total trials ordered quasi-randomly.

After participants had memorized the melody and could play it without errors, and just before they were acquainted with altered feedback, they were told, "sometimes I might be playing and sometimes I might not be playing. You might sometimes hear what you are producing and might sometimes hear what I [the experimenter] am producing." They were then acquainted with altered feedback, and instructed to make ratings using the instructions from Experiment 1 (without further reference to the possibility of the sounds being produced by the experimenter).

Participants were aware of the experimenter's presence and whether or not he appeared to be playing, but rarely looked away from their keyboard and did not appear to be highly focused on his movements. This suggests that, when he appeared to be playing, they accepted him as a possible source of what they heard and were not trying to use visual evidence to disprove that possibility.

At the end of the experiment, participants were debriefed exactly as in Experiment 1. After this debriefing, in order to determine whether the agency manipulation was successful, participants were asked two additional yes/no questions: "Did you think there were times when you could definitely tell that you were making the sounds?" and "Did you think there were times when you could definitely tell that I [the experimenter] was making the sounds?" More extensive comments were obtained from pilot test subjects, including some experienced in this type of AAF paradigm, in order to ensure that J.J.C. was in fact acting in a way that suggested he was a plausible source of feedback.

3.1.4. Data analysis

Agency ratings, performance timing, and performance errors were analyzed exactly as in Experiment 1. As mentioned earlier, analyses focused on subsets of the data that are typically associated with greatest disruption. As such, two orthogonal repeated measures ANOVA designs were used. One ANOVA incorporated the variables feedback synchrony (four levels) and exclusivity (solo versus duet performance). The other ANOVA incorporated the variables feedback pitch (four levels) and exclusivity. We report both ANOVA designs for analyses of agency ratings. For measures of disruption we focus on designs associated with AAF manipulations known to disrupt that measure of performance. Thus, we analyzed mean IOI using the ANOVA design based on feedback synchrony and we analyzed error rates using the ANOVA design based on feedback pitch (see Appendix for all means).

3.2. Results

3.2.1. Agency ratings

The ANOVA that evaluated the effect of feedback synchrony on agency ratings yielded a significant main effect of feedback, $F(3,51) = 46.62$, $p < .01$, $\eta_p^2 = .73$, but only a marginal effect of exclusivity ($p = .07$, $\eta_p^2 = .19$) and no interaction ($p > .70$, $\eta_p^2 = .03$). Fig. 4A shows that feedback synchrony, as in Experiment 1, was associated with reduced ratings of agency that were lower for long (75%) and unpredictable random asynchronies than for the 25% asynchrony. Exclusivity had only a modest effect on agency ratings, though the effect was in the predicted direction.

Likewise, an ANOVA that evaluated the effect of feedback pitch on agency yielded only a main effect of feedback, $F(3,51) = 29.36$, $p < .01$, $\eta_p^2 = .63$, but no other effects ($p > .30$ in each case, η_p^2 for the main effect of exclusivity = .07, for the interaction = 0.01). Fig. 4B illustrates that the pattern of results was again similar to that in Experiment 1, with lowest agency ratings being found when participants experienced randomly selected pitches and intermediate ratings for the lag 1 and lag 7 conditions.

Results from agency ratings may be taken to indicate that participants did not "believe" our manipulation of exclusivity. However, reports from the debriefing phase of our experiment strongly suggest otherwise. Only one participant correctly inferred that all of the feedback in the experiment was triggered by their own actions. By contrast, 13 participants reported thinking that there were definitely times when the sounds were created by the experimenter, a significant margin according to a binomial sign test ($p = .033$). Of the remaining 5, who did not think there were times that the experimenter definitely created auditory feedback, but did not correctly label all feedback as their own, only two participants reported thinking that there was no difference in the production of sounds during trials in which the experimenter was playing versus when the experimenter was not playing. All participants reported thinking that there were definitely times when they produced auditory feedback. Thus, the manipulation of agency in Experiment 2 was successful in influencing the phenomenological experience of participants overall. However, the current experiment did not gauge the level at which participants attended to the experimenter. This leaves open the possibility that, while they definitely did attend to him and did believe that he sometimes created the sounds they heard, their attention might not have been sufficient enough to produce a change in their behavior. This could be because the manipulation was not strong enough, or because inferences about exclusivity do not have a strong influence on agency in sequence production.

With respect to the specific cues participants reported using to determine agency, 6 participants reported using sensorimotor cues; 12 reported making an inference (all 12 referenced the experimenter), suggesting that the agency manipulation successfully focused participants on inference strategies. Nine participants reported using pitch only, 5 reported using synchrony, and 4 pitch and synchrony.

3.2.2. Disruption by AAF

We now turn to analyses concerning how the feedback and manipulation of exclusivity influenced performance. The ANOVA on mean IOI yielded a significant main effect of synchrony alteration, $F(3,51) = 7.42$, $p < .01$, $\eta_p^2 = .30$, a marginal main

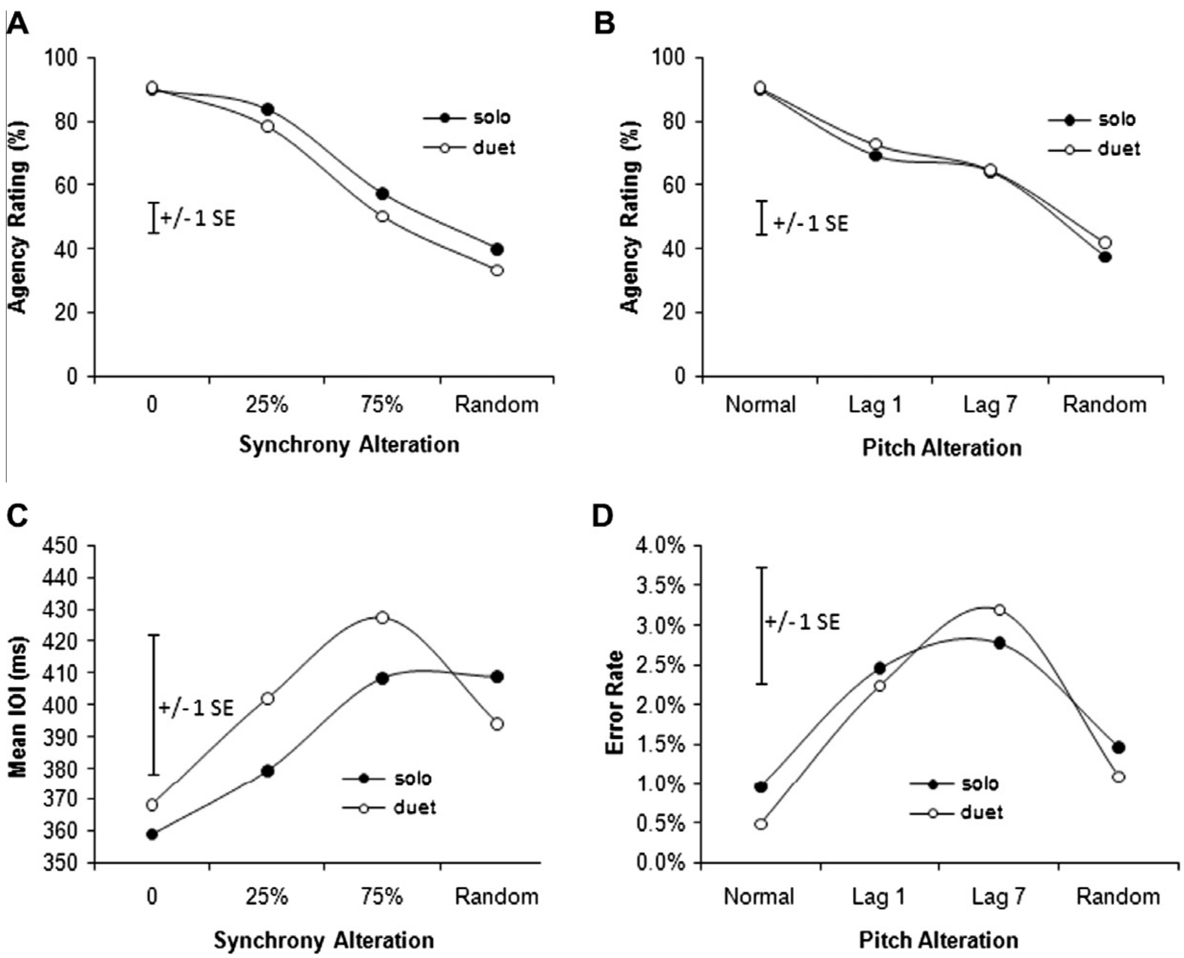

Fig. 4. (A) The effect of feedback synchrony on agency ratings in the solo and duet conditions of Experiment 2. (B) The effect of serial shift on agency ratings in the solo and duet conditions of Experiment 2. (C) The effect of feedback synchrony on performance speed (mean IOI) with normal pitch only. (D) The effect of serial shift on error rates with synchronous feedback only. For all graphs, error bars indicate ±1 SE.

effect of exclusivity ($p = .08$, $\eta_p^2 = .17$), and no interaction ($p > .18$, $\eta_p^2 = .09$). As can be seen in Fig. 4C, the results replicate the effect of feedback synchrony for the normal feedback pitch condition of Experiment 1, with greatest slowing for the 75% delay condition and intermediate degrees of slowing for the 25% and random delay conditions. Participants overall performed somewhat more slowly in duet than in solo conditions, though this effect did not reach significance.

We also measured the effect of AAF on accuracy in production, using mean error rate per trial. Unlike Experiment 1, in which the effect of error rates on AAF was unreliable, the ANOVA on error rates yielded a significant main effect of feedback pitch, $F(3,51) = 3.39$, $p < .05$, $\eta_p^2 = .19$, but no main effect of exclusivity and no interaction ($p > .80$, $\eta_p^2 < .01$ for each). As can be seen in Fig. 4D, pitch alterations led to increased errors for the lag 1 and lag 7 feedback conditions, but not for the random pitch condition, with no apparent modulating effect of exclusivity.

3.2.3. Tests of mediation

We employed the same strategy as used in Experiment 1 to test whether the effect of AAF on agency was mediated by its disruptive effect on performance, or vice versa. Detrending was based on relationships computed across all trials, including those not incorporated in the analyses reported here (see *Data Analysis*). As in Experiment 1, significance levels did not change after detrending, and detrended data were well predicted by the original data in every case ($r^2 > .91$ in every case). One slight difference from Experiment 1 was that slopes formed by the relationship between original and detrended data for agency ratings were greater than 1 (slope = 1.65 when error rates were partialed out, 1.66 when IOIs were partialed out), suggesting that the removal of variance associated with disruption enhanced the effect of AAF on the judgments of agency. This is the opposite effect of what is typically expected from mediation.

3.2.4. Relating disruption and agency

Finally, we assess the nature of the relationship between the judgments of agency during AAF and the disruptive effect of AAF. Though tests of mediation suggest that these outcomes are independent of each other, it still of interest to determine what experience of agency accompanies more or less disruptive AAF conditions. We plot this relationship in Fig. 5, separately for disruption of timing (Fig. 5A) and accuracy (Fig. 5B). Data from Experiment 2 are plotted along with comparable conditions from Experiment 1.

Two themes emerge from the data presented in Fig. 5. First, the relationship between agency and disruption is non-linear, with greatest disruption being associated with conditions yielding an intermediate experience of agency. The tendency for maximal disruption for intermediate agency was tested by fitting a second-order polynomial function to the 12 means shown in each panel of Fig. 5 (for Fig. 5A, $r^2 = .69$, $p < .01$ for Fig. 5B, $r^2 = .44$, $p < .05$). The second theme is that the overall context of Experiment 2 led participants to favor speed over accuracy, more so than in Experiment 1. This was particularly apparent when comparing the "solo" performances across Experiments, which were slower and less errorful in Experiment 1 than in Experiment 2. Despite differing with respect to the speed/accuracy tradeoff, possibly due to social facilitation (Zajonc, 1965) or other processing demands, the pattern of disruption (relative to normal feedback) was very similar across experiments as were the overall judgments of agency.

3.2.5. Effect of piano experience

Participants in Experiment 2 had significantly more piano training than those in Experiment 1, $t(34) = 2.1$, $p < .05$, but there was no difference in overall musical training. To determine whether the manipulation in Experiment 2 had any effect independent of piano training, we analyzed the data from 7 participants from each group that had no piano training. Using all of the analyses described above, we found the same characteristic pattern of results. We also ran three separate ANCOVAs on agency ratings, mean IOIs, and errors, using musical training as a covariate and found that it did not contribute to our results. Thus, we do not believe that piano training had any effect on agency ratings or performance disruptions in these experiments. However, this does not rule out the possibility that more experienced musicians might produce different results.

3.3. Discussion

In Experiment 2, we introduced a paradigm in which participants either performed alone or while facing the experimenter, and were told that during the latter type of trial auditory feedback could result from the experimenter's actions. This manipulation was used to test the role of inferences in the influence of AAF on self-agency. Specifically, we wondered whether the non-exclusive inferred cause of feedback during "duet" performance would reduce the experience of agency, and possibly reduce the influence of AAF on agency. Most participants reported believing our manipulation, which suggested that the manipulation did successfully influence inferences about the experiment. However, this new manipulation had no effect on the disruptive effect of AAF or the influence of AAF on judgments of agency.

At the same time, results of Experiment 2 replicated the critical findings from Experiment 1, and further analyses pooled across experiments suggested that the relationship between AAF's influence on agency and its influence on disruption is non-linear, with the most disruptive conditions being associated with an intermediate (ambiguous) experience of self-agency. Moreover, the fact that the manipulation of exclusivity did not lead to reliable effects on agency or on the disruptive effect of AAF has important theoretical implications. Namely, the influence of AAF on agency seems to be based primarily on

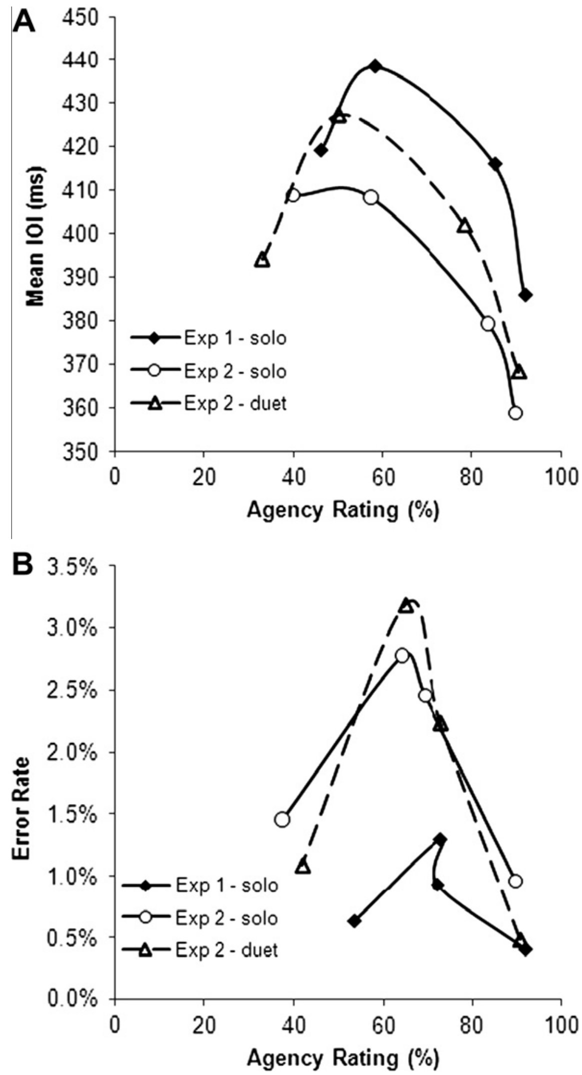

Fig. 5. The relationship between agency and disruption across both Experiments. In each panel, the data from Experiment 2 is separated for trials with and without the accompanist. (A) The relationship between ratings of agency and mean IOI for alterations of feedback synchrony. Each series shows, from left to right data from random, 25%, 75%, and normal synchrony conditions in which feedback pitch was normal. (B) The relationship between ratings of agency and the percent of trials with any error. Each series shows, from left to right, data from random, lag 7, lag 1, and normal conditions in which feedback synchrony was normal.

sensorimotor cues, related to mismatches between the anticipated and actual outcomes of actions with respect to time and pitch content. AAF manipulations influence sensorimotor cues, whereas the exclusivity manipulation was designed to alter inferences (which appeared to be successful, based on participants' responses during debriefing). Of course, inferences about priority and consistency are usually coupled with corresponding sensorimotor cues, while inferences about exclusivity are not. Inferences about exclusivity apparently have a less strong effect on performance and agency, at least in this experimental situation. Still, it is possible that different manipulations of exclusivity might affect agency.

4. Experiment 3

Experiments 1 and 2 provided strong evidence that manipulations of auditory feedback decrease subjective judgments of agency. However, Experiments 1 and 2 did not contain any conditions designed to determine whether participants could make veridical agency judgments in situations where auditory feedback truly does not reflect one's own actions. We thus conducted a control experiment testing participants' feeling of agency when presented with their own performance versus someone else's performance.

Participants performed short, unfamiliar melodies from memory silently on a keyboard (i.e., without auditory feedback). Following their silent performance, participants heard the feedback from the performance they just completed or a recording of an unaltered feedback trial from Experiment 1. Participants were then asked to judge whether the melody they heard was the result of their own actions, using the same agency scale described in the other two experiments. We predicted that participants would have some ability to recognize their own performances (Flach, Knoblich, & Prinz, 2004; Repp & Knoblich, 2004) and would therefore report greater feelings of past agency when they heard feedback from their own performance than when they heard feedback from someone else's performance.

4.1. Method

4.1.1. Participants

Twelve students (four women, eight men) from the University at Buffalo participated in exchange for course credit for an introductory psychology class. The mean age of participants was 20 years. Participants averaged 1.08 years of piano experience, and 5.67 years of total experience on any instrument. Only one participant had more than 6 years of piano experience (12 years reported), the rest reported 1 year or less experience. None of the participants reported having absolute pitch, nor did they report any hearing problems. All participants reported being right-handed. None had participated in Experiment 1 or 2.

4.1.2. Conditions and design

Experiment 3 featured two critical feedback conditions: "self," in which the participant heard their own performance, and "other," in which the participant heard a previously recorded performance of a participant in Experiment 1. Participants performed "self" and "other" trials in a pseudorandom order. Each participant performed two melodies (those described above) during different halves of the experiment, and each participant was assigned to one of two trial order conditions that varied whether the first trial was a "self" trial or an "other" trial. Ten repetitions of both feedback conditions were performed in one of two random orders for each of two melody blocks, resulting in 40 experimental trials.

During "other" trials we aimed to have the participant hear unaltered feedback from Experiment 1 that closely matched the mean IOI (M) and coefficient of variation of IOI (CV) of the participant's current performance. On each "other" trial we selected the performance from Experiment 1 that minimized the vector distance to the participant's current performance using the following Euclidean distance metric:

$$d = \left| \sqrt{M_p^2 + CV_p^2} - \sqrt{M_q^2 + CV_q^2} \right|$$

where d is the vector distance between trials, the subscript p refers to the present trial and the subscript q refers to a trial from Experiment 1. We chose to minimize differences in tempo and timing variability in this way in order to test whether self-agency cues can be based on subtle nuances of performance rather than large-scale global characteristics like overall tempo and timing variability. For similar reasons, trials from Experiment 1 containing pitch errors were not used.

4.1.3. Materials and apparatus

All were identical to Experiment 1.

4.1.4. Procedure

Participants were trained for the first of their two melodies exactly as in Experiment 1. On each experimental trial, participants began playing at the conclusion of four metronome clicks and stopped playing after the end of three repetitions of the melody. No auditory feedback was presented during this performance. Immediately following the performance, participants heard a recording either of the performance they just produced ("self" trials), or of the most closely matching performance of the same melody from normal feedback trials in Experiment 1, using the algorithm described above ("other" trials). The participant then heard the tone that cued them to make their agency rating, which they generated as in Experiments 1 and 2. The participant took a break after 20 of these trials, and training began for the second melody before performing 20 more experimental trials with the second melody.

4.2. Results and discussion

Agency ratings were significantly higher during self (M = 73%, SE = 2%) than other (M = 44%, SE = 3%) trials, $t(11) = 8.51$, $p < .01$. The finding that participants experience greater feelings of agency when listening to their own performances versus performances closely matched on salient factors such as tempo and tempo variability indicates that participants can in fact tell the difference between their own performances and someone else's. These results rule out a possible concern about whether results in Experiment 2 could have resulted from confusions about the self-report measure we used. They also suggest that the rating system may still capture the effect of inferential and sensorimotor cues when applied to offline judgments of past performance.

The matching procedure we applied made it possible to have the same "other" performance selected on multiple trials for a given participant, which could make the task of identifying "other" performances easier. On average, 65% of "other" trials were unique for each participant, and among files that repeated the mean number of repetitions was 3.35. We addressed a potential effect of trial repetitions by selecting only the first repetition of each "other trial" for a participant in a follow-up

ANOVA. The results of this ANOVA yielded the same results. We also ran a follow-up ANOVA using only trials in which participants made no errors and found the same results.

5. General discussion

In two experiments, participants performed short melodies from memory on an electronic keyboard while experiencing altered auditory feedback (AAF). Alterations could take the form of asynchronies between key presses and sounds and/or alterations of feedback pitch. Participants rated their experience of agency after each trial. Disruptive effects of AAF on production replicated findings from previous research: Alterations of pitch increased errors, alterations of synchrony slowed production, and combined alterations led to reductions in disruption. This was the first time to our knowledge that agency had been investigated in a sequence production task. Agency ratings decreased (indicating lower experiences of agency) as AAF manipulations caused the sequence of feedback events to become less similar to the planned sequence of actions. Furthermore, the relationship between agency and disruption was non-linear and suggested that maximally disruptive AAF may be found when the relationship between perception and action is ambiguous with respect to agency, and vice versa. Finally, manipulations that influenced participants' inferences about agency (via the presence or absence of an accompanist) did not influence the effect of AAF on judgments of agency, or its disruptive effect on production. These results have important implications both for the relationship between perception and action in sequence production and for the generalizability of existing theories of agency.

5.1. Implications for theories of agency

There is currently no evidence for a link between altered auditory feedback in sequence production tasks and the experience of agency, although several results from single-response tasks suggest that this link may exist. Two basic theoretical approaches to agency were described by Sato (2009). One approach focuses on sensorimotor prediction and assumes that agency is determined in part on the generation of an efference copy by motor commands (e.g., Wolpert, Doya, & Kawato, 2003). According to this view, the performer matches actual perceptual feedback to the efference copy generated by the sensorimotor system, and agency is reduced when there is a mismatch. A second approach, exemplified by Wegner (2002), focuses on the way in which agency results from our conscious intentions and the inferences we form based on those intentions about the results of our actions. Agency according to this approach can be inferred by our assumptions about the context in which we perform a series of actions, independently of sensorimotor matches. Wegner (2002) described three conditions that would lead a person to infer that they caused an action: Priority, consistency, and exclusivity, as described in Section 1.

The experiments here were not designed to distinguish these contributions to agency directly. However, the present results seem to support the view that AAF manipulations influence agency via motor prediction and are impervious to the influence of some kinds of inferences. Our manipulation in Experiment 2 influenced overall judgments of agency but did not modulate the influence of AAF either on agency or on performance. AAF may selectively influence motor prediction (via the efference copy mechanism), whereas our manipulation of exclusivity selectively influenced the experience of agency via inference. In this respect our data are similar to those of Repp and Knoblich (2007, cf. Knoblich & Repp, 2009), who assessed the contribution of bias and sensitivity in a synchronization task where the control over sound events would change at one point in the trial (first described by Fraisse and Voillaume (1971/2009)). For part of the trial sounds were generated by a metronome and during the other part the sounds were auditory feedback from the participant's taps. Overall these authors found that sensorimotor cues (the degree of asynchrony between actions and sound) determined judgments of agency, but an independent source of bias (toward interpreting sounds as being self-generated) also influenced participants' responses. Flach (2005) similarly found some evidence to suggest that agency was independent from performance adjustments made in response to perturbations.

Although our data suggest a stronger role for sensorimotor cues than inferential on the effect of AAF, the principles of priority and consistency as proposed by Wegner (2002) are nonetheless useful in interpreting the effects of AAF on agency. Specifically, altered feedback violates consistency by creating a situation in which the prior thought (the expected auditory consequence) is similar, but not identical, to the actual auditory consequence. Beyond this, randomized feedback conditions additionally influence consistency, in that the link between perception and action was less reliable in that condition than others. Our data suggest that both priority and consistency influence judgments of agency, though we found a stronger influence of consistency in alterations of feedback pitch than in alterations of feedback synchrony. We suspect that this difference reflects the fact that alterations of feedback synchrony are variable in that AAF onsets are influenced by variability in produced timing. Thus, the timing of randomly selected delays may not be much more variable than the timing of delays of a predetermined length, particularly given our earlier observation that the delays used in the random condition were within the range of delay lengths used in the other asynchronous feedback conditions. By contrast, the difference between randomized pitch feedback and serially shifted feedback is more salient.

5.2. Implications for the effect of AAF

The disruptive effects of AAF on production in Experiments 1 and 2 replicate what has been found in previous research; the novel contribution of the current research has to do with the relationship between agency ratings and disruption. The relationship between agency and disruption was non-linear. Results suggest that greatest disruption is found for AAF conditions leading to an intermediate (ambiguous) experience of agency. The relationship between disruption and agency for manipulations of feedback synchrony was more consistent with the view that disruption reaches an asymptotic level after which decreases in agency are not associated with increases in disruption.

These patterns of results, on their own, do not speak to whether there is any kind of causal relationship between agency and disruption. The manipulation of exclusivity in Experiment 2 was designed in part to address this issue by reducing inferential aspects of agency more in some conditions than others. The fact that this manipulation did not influence disruption from AAF suggests that disruption is at least not a direct result of the inferential aspects of agency. Furthermore, analyses designed to test for possible mediating relationships of agency on the disruptive effect of AAF, or vice versa, suggested that no mediating relationships exist. Thus we suspect that the experience of agency and disruption are joint effects of AAF on two distinct components of the perception–action loop. In line with other recent research (Berkowska & Dalla Bella, 2009; Loui et al., 2008; Repp, 2000, 2001), we suggest that the effect of AAF on performance may reflect one stream within the auditory system that regulates sensorimotor coordination, whereas the effect of AAF on agency may reflect a different stream responsible for the interface between perception and awareness.

Beyond this, a critical point concerning the effects of AAF can be addressed by the present results. Namely, to what extent does AAF truly function as "feedback" in a feedback-control system? For various reasons, some have questioned the very term "feedback" in paradigms like ours, as discussed earlier (e.g., Howell, 2004). Similarly, the current data suggest that "feedback" may be an ill-fitting term for the current paradigm, in that disruptive conditions are also those in which the participant is uncertain about whether his or her actions in fact led to feedback events. Thus, although the term "auditory feedback" is useful as a reference due to familiarity and brevity (cf. "altered recurrent auditory information", proposed by Howell (2004)), the term may not in fact be accurate in the context of AAF paradigms. Moreover, there is a possibility that AAF is disruptive in part because it is not interpreted as feedback but instead is treated by the system like an external source of interference (cf. Flach, 2005).

In conclusion, we have provided evidence that AAF can influence one's experience of agency, but that the influence of AAF on agency is distinct from the influence of AAF on production. We suspect that disruption may be associated with the experience of uncertainty regarding self-agency, but that the two are not causally related. That is, AAF yields correlated effects on both action planning and metacognition that are related to distinct underlying processes.

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Appendix A

See Tables A1–A3.

Table A1
Judgments of agency for all synchrony (vertical) and pitch (horizontal) conditions in Experiments 1 and 2 and their respective means. Judgments use the 100-point scale described in the text.

	Normal	Lag l	Lag 7	Random	MEANS
Experiment 1					
0%	91.89	72.15	72.81	53.36	72.55
25%	85.15	70.74	68.23	48.92	68.26
75%	58.16	43.77	54.18	30.84	46.74
Random	46.12	35.56	35.90	25.84	35.85
MEANS	70.33	55.55	57.78	39.74	
Experiment 2 (solo)					
0%	89.76	69.21	64.17	37.44	65.15
25%	83.71	61.47	65.17	36.38	61.69
75%	57.25	45.90	49.85	21.83	43.71
Random	39.95	33.66	36.05	15.87	31.38
MEANS	67.67	52.56	53.81	27.88	
Experiment 2 (duet)					
0%	90.63	72.88	64.82	41.91	67.56
25%	78.55	65.63	61.19	39.50	61.22
75%	50.18	47.50	49.88	15.10	40.66
Random	33.16	32.47	36.61	15.79	29.51
MEANS	63.13	54.62	53.13	28.07	

Table A2
Mean IOI for all synchrony (vertical) and pitch (horizontal) conditions in Experiments 1 and 2 and their respective means. IOIs are in milliseconds and calculated as described in the text.

	Normal	Lag l	Lag 7	Random	MEANS
Experiment 1					
0%	385.84	392.32	392.72	382.97	388.46
25%	415.95	410.79	408.57	409.92	411.31
75%	438.40	418.25	406.53	414.82	419.5
Random	419.12	416.46	404.51	414.33	413.6
MEANS	414.83	409.45	403.08	405.51	
Experiment 2 (solo)					
0%	358.83	388.08	369.69	354.83	367.86
25%	379.23	405.55	385.02	386.50	389.07
75%	408.31	435.92	422.86	412.93	420.01
Random	408.76	424.40	413.44	407.00	413.4
MEANS	388.78	413.49	397.75	390.32	
Experiment 2 (duet)					
0%	368.22	372.16	384.58	371.70	374.17
25%	401.93	400.01	388.80	388.62	394.84
75%	427.26	437.54	432.81	398.62	424.06
Random	394.08	389.56	405.52	393.40	395.64
MEANS	397.87	399.82	402.93	388.09	

Table A3
Error rates for all synchrony (vertical) and pitch (horizontal) conditions in Experiments 1 and 2 and their respective means. Error rates are percentages calculated as described in the text.

	Normal	Lag l	Lag 7	Random	MEANS
Experiment 1					
0%	0.4	0.9	1.2	0.7	0.8
25%	0.6	0.6	1.0	0.7	0.7
75%	1.3	1.3	0.9	0.7	1.1
Random	1.0	0.9	1.1	0.4	0.9
MEANS	0.8	0.9	1.0	0.6	
Experiment 2 (solo)					
0%	1.0	2.5	2.8	1.5	1.9
25%	1.7	3.2	3.3	2.5	2.7
75%	1.3	2.8	1.5	2.3	2.0
Random	2.4	1.2	1.3	1.7	1.6
MEANS	1.6	2.4	2.2	2.0	
Experiment 2 (duet)					
0%	0.5	2.2	3.2	1.1	1.8
25%	1.3	2.0	1.1	1.8	1.5
75%	2.0	2.2	1.9	1.0	1.8
Random	1.7	3.3	1.2	1.1	1.8
MEANS	1.4	2.5	1.8	1.2	

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