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Intercepting real and simulated falling objects: What is the difference?

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ABSTRACT

The use of virtual reality is nowadays common in many studies in the field of human perception and movement control, particularly in interceptive actions. However, the ecological validity of the simulation is often taken for granted without having been formally established. If participants were to perceive the real situation and its virtual equivalent in a different fashion, the generalization of the results obtained in virtual reality to real life would be highly questionable. We tested the ecological validity of virtual reality in this context by comparing the timing of interceptive actions based upon actually falling objects and their simulated counterparts. The results show very limited differences as a function of whether participants were confronted with a real ball or a simulation thereof. And when present, such differences were limited to the first trial only. This result validates the use of virtual reality when studying interceptive actions of accelerated stimuli.

1. Introduction

With ever improving computational technology, an increasing number of studies in the field of human movement perception use simulated motion. This technology has the obvious advantage of allowing precise control of independent variables. In the field of time-to-contact (TTC: the time until the moving object reaches an interception point) estimation, a growing number of experimenters have used simulations for many years (see, e.g., Hecht and Savelsbergh, 2004).

However, a major limitation of simulations is that the stimulus is necessarily impoverished, for purposes of control and convenience. Static features, such as amount of detail, resolution, contrast, and dynamic features can differ dramatically between simulated and actual motion. As far as the dynamic features are concerned, typical displays – be it motion pictures or virtual reality displays – consist of a succession of static images that are presented with a high frequency of 25 Hz or more. Even if the system generates an apparent motion due to the retinal persistence and cognitive processes, there is no actual motion. Instead, a "moving object" actually appears and disappears at different positions at different times. In particular some researchers following the ecological approach to perception (Gibson, 1979) have voiced deep concerns about equating real and snapshot motion (e.g., Michaels and Carello, 1981). Findings based on such snapshot motion may or may not replicate in a real motion context. In this sense, the ecological validity of a catching task may or may not be given if merely snapshot motion displays have been used. In other words, experimental findings based on pixelated sequential displays may not entail the same perceptual and motor processes that real objects do (see De Gelder and Bertelson, 2003). By ecological validity we here mean that the theory that holds for the results found in virtual reality can be generalized to real-world tasks, by allowing the same findings in terms of level of performance, movement parameters and information used. This validity is often taken for granted and may indeed be assumed in many cases. However, in the case of subtle or complex motion, simulation may not be valid, as Zago et al. (2004) stipulated for the interception of free-falling objects. We investigated whether the validity of computer simulation is indeed challenged in the case of falling objects. To this end, we first discuss previous attempts to compare actions in virtual reality with actions in "genuine reality" (Bridgeman, 2009), as relevant for the field of perception–action studies. Then, we explore the potential loss of information when simulating the effects of gravity and finally report an experiment which suggests that our worries may be insubstantial after all.

Previous researches are quite contradictory on the equivalence of results obtained using virtual and genuine realities. On the one

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hand for example, Boer et al. (2000) studied the time-based decision triggers and deceleration control strategies of participants driving the same road in reality or in a driving simulator. The results showed that even if the approach speeds to a stop line and in a curve were similar, these velocities were not reached with the same deceleration profiles: participants initiated their deceleration later in the simulator, and with a higher intensity to compensate for this delay. A similar difference has been found by Dessing et al. (2004) in a catching experiment. Compared to the real control case, catching hand movements in a CAVE were initiated later, leading to substantial differences in other movement parameters, such as aiming direction and velocity peaks. Other authors, however, found comparable results using genuine and virtual reality in such domains as street crossing (Schwebel et al., 2008) or interceptive tasks. Bideau et al. (2003) found no differences both in the level of performance and in movement parameters of a handball goalkeeper when trying to stop balls in genuine reality and for the same throws captured and animated in virtual reality. It is worth noting however, that this experiment only tested one expert goalkeeper and a higher number of participants would be needed to validate this result.

When considering the interception of a free-falling object, it has recently been proposed that the timing of interceptive actions could be based on the use of a priori knowledge of gravitational acceleration. The nature of this a priori knowledge could be tantamount to an internalization of the effect of gravity, and more importantly, it could qualify the well known failure to account for acceleration when merely observing objects on an approach course (Kaiser and Hecht, 1995; Werkhoven et al., 1992). In testing this hypothesis, Zago et al. (2004) obtained mixed results in two experiments with a real and a virtual ball. In the first experiment, in which a real ball had to be intercepted, the ball fell behind a screen while at the same time a virtual ball was projected on the screen, either with or without a 1-g acceleration. The experimental device was programmed in such a way that the virtual and the real ball arrived at the bottom of the screen where the interception could occur. In the second experiment, only a virtual ball was projected and the participants had to press a button when the ball reached the bottom of the screen. Results showed better performances in the first experiment with the real ball falling behind the screen when the simulated ball was falling with a 1-g acceleration compared to a constant velocity fall, but no such difference in the second experiment without a real ball to intercept. Zago et al. (2004) concluded that the simulated objects were perceived as massless and thus not subject to gravity, therefore not activating the internal model of gravity. Hence, only when intercepting a real ball, participants would use a 1-g model, whereas, when acting on a simulated ball participants would lack assumptions on the object's acceleration and thus not engage a 1-g model. Instead they would respond on the basis of first order information corresponding to a constant velocity control mode (e.g., Tresilian, 1991).

These results have important implications that were underlined by Georgopoulos (2004, p. 1455): "if you are trying to intercept a falling object, then you definitely rely on apparently internalized knowledge of gravity; but, when you play games with interception on a video screen using a mouse, you conveniently switch to a strategy based on the assumption of uniform motion, also a very appropriate knowledge in this case!" This could also mean that the ecological validity of most experiments on interceptive actions carried out with virtual set-ups should be questioned.

Indeed, numerous experiments have been conducted using virtual reality in the domain of TTC estimation, for example in interceptive actions (e.g., Gray, 2002, Zaal and Michaels, 2003, Takeichi et al., 2004), pedestrian street crossing (e.g., Seward et al., 2007, Lobjois and Cavallo, 2007), and ball bouncing (e.g., Morice et al., 2007). All these studies have provided new results and new interpretations of human behavior that would become doubtful if the ecological validity of the simulation were challenged.

As this issue has never been directly addressed in interceptive action, we conducted an experiment to find out if there is reason to entertain doubts about the validity of simulation. It is to be noted that the study by Zago et al. (2004) was not designed with this primary goal in mind and that two different tasks (interception vs. button press) were also involved. These two tasks could explain the differences in the results (for a critical analysis of this study, see Baurès et al., 2007 and a reply by Zago et al., 2008). Consequently, we compared the timing of the same interceptive task with a real and a virtual ball. The aim of this experiment was to find a scenario that would most likely produce a difference between a real and a virtual display. We took falling objects to bring out such differences—if they exist at all. According to the above-mentioned evidence, observers might need the richness of a real-world stimulus to access the internal gravitational model they might possess. Two hypotheses were formulated: if observers are able to use an internal model of gravity for interceptions of a real ball, but not for interceptions of a simulated ball, then we should witness different timing and better accuracy with the real ball. In this case, the use of an internal model of gravity vs. the use of a constant velocity strategy based on first order information should be distinguishable on the basis of the pattern of errors in the first trials. The use of an internal model of gravity should result in an immediate accuracy in timing while the use of a constant velocity strategy should result in late errors. Such errors should be found in particular on the first trials before the occurrence of a self-calibration of the system to minimize errors. On the other hand, if the degree of realism is irrelevant for the perception of ball's motion, we should observe no difference in timing between real and simulated objects. In this case, participants should thus exhibit the same pattern of errors, regardless of the ball they intercept. The errors should only depend on the underlying control mode used (internal model of gravity or constant velocity strategy) but not on the display mode.

2. Materials and methods

In the experiment, 32 participants (20 males and 12 females, mean age 24.96 years; SD 3.71 years) were tested. This experiment was conducted in accordance with the Declaration of Helsinki, and all persons gave their informed consent prior to their participation to the experiment. During the test, the participants had to intercept a falling ball with a virtual interceptor when it passed a predefined interception point. The falling ball could be either simulated or real. The real ball had a diameter of 4 cm and a mass of 30 g. It was released by an electromagnet just in front of the white screen. The simulated ball had the same diameter and simulated mass and was projected from the rear onto the white screen. In both conditions, the ball fell from a height of 1.75 m with an initial velocity of 0 m/s. Both simulated and real balls had an initial acceleration of 1 g and were subjected to air resistance corresponding to a value appropriate for the real ball. Both balls reached a velocity of 5.79 m/s at the interception point and had a falling time of 600 ms.

Participants were instructed to hit the ball with a virtual effector by producing a continuous movement without reversing direction. We used a virtual effector to avoid any differences between the virtual and real conditions that would not be the result of the visual simulation. If participants had to perform an interception with the hand, they would have had an impact of the ball with their hand in the real condition only. Such feedback was precisely one of the reasons for the difference observed by Dessing et al. (2004) between intercepting a real and a virtual ball. The virtual effector in our experiment was a black circle (diameter = 5 cm) projected on the screen from behind (Fig. 1). Its starting point was 30 cm to the left

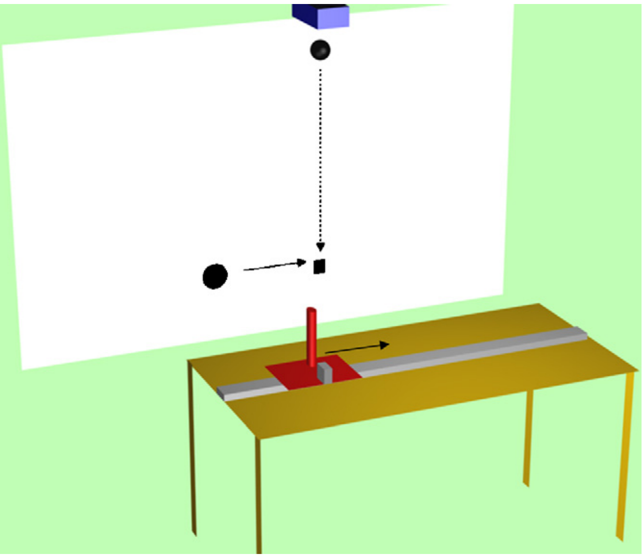

Fig. 1. The experimental setup: when real, the ball was held up and released by an electromagnet in front of the screen. The dashed arrow represents the movement of the ball. The interception point and the effector were represented as symbols on the lower part of the screen. Observers controlled the manipulandum which moved on a straight track whose position was registered by a computer not represented here. Solid arrows represent the movement of the manipulandum and the visible effector yoked to it.

of the interception point, which was indicated by a 1 cm^2 black square. The movement of the virtual effector was controlled by a hand-held manipulandum moving along a track perpendicular to the falling ball. Movements of the hand were recorded with a Flock of Birds electromagnetic tracking system (Model 6DFOB©, Ascension Technologies) at a sampling frequency of 120 Hz. They were immediately fed back to control the motion of the effector on the screen. The lag time (30 ms) remained below perceptual threshold[4].

The trial was initiated by the experimenter pressing a button. After a randomized time interval ranging from 3 to 6 s, the ball was dropped (t0). The timing accuracy was calculated in ms by the difference between the arrival time of the center of the effector and the arrival time of the center of the ball (i.e., 600 ms) at the interception point. A 0 ms value meant that the center of the ball and the center of the interceptor were at the interception point at the same time. A positive value meant that the effector arrived later than the ball while a negative value indicated that the effector was early. A temporal window for a successful interception of the ball was defined according to the temporal margin given by this equation:

$$(D_{\mathrm{eff}} + D_{\mathrm{ball}})/V_{\mathrm{ball}} \quad (1)$$

with D_{eff} the diameter of the effector, D_{ball} the diameter of the ball and V_{ball} the velocity of the ball at the interception point (Tresilian and Lonergan, 2002). Accordingly, verbal feedback ('too late', 'too early' or 'ball intercepted') was given to the participants indicating whether they were within or outside the window of ±8 ms, respectively $[(D_{\mathrm{eff}} + D_{\mathrm{ball}})/V_{\mathrm{ball}} = 16\ \mathrm{ms}]$.

Before the experimental test, the participants had a training session to learn the properties of the effector they used. The training consisted in intercepting a simulated ball moving horizontally from the right to the left towards the interception point. The simulated ball was rear-projected by a video-projector and started always from the same point placed at 1 m to the right of the interception point. It could adopt several velocities. The interception point and the effector had exactly the same position as in the test session. The main goal of this training session was to provide the opportunity to integrate the properties of the virtual effector (i.e., the one-to-one effector ratio and the 30 ms lag). During training participants carried out blocks of five trials each until they performed three trials within ±50 ms. On average, this had been achieved after the fourth block.

After the training session, a first group of participants performed 50 trials with the real ball (real ball group: RB_{group}; $N = 16$) while a second group performed 50 trials with the simulated ball (simulated ball group: SB_{group}; $N = 16$). Five minutes after the end of this first session, half of the participants from the RB_{group} performed a first post-test of 20 trials with a real ball ($RB\text{-}RB_{\mathrm{group}}$) while the other half performed a first post-test of 20 trials with a simulated ball ($RB\text{-}SB_{\mathrm{group}}$). Conversely, half of the participants from the SB_{group} performed a post-test with a real ball ($SB\text{-}RB_{\mathrm{group}}$) while the other half with a simulated ball ($SB\text{-}SB_{\mathrm{group}}$). Lastly, 7 days later, all participants performed a second post-test of 20 trials in the same respective conditions as in the first post-test.

Five dependant variables were analyzed: initiation time corresponded to the time from the release of the ball to the moment where the effector velocity reached 5% of its maximum. Movement time (MT) corresponded to the time between the initiation time and the time at which the hand crossed the interception point. Constant error (CE)[5] corresponded to the difference between the movement time of the hand and the time taken by the ball to reach the interception point. A positive value represents a late error whereas negative value represents an early error. Interception rate corresponded to the percentage of trials in which the error (CE) was within the ±8 ms temporal window. Finally, the initial error corresponded to the error committed at the first trial. The first trial is particularly interesting because it is the only trial for which the kinematics of the ball is still unknown and our representation of the trajectory has not yet been corrected by the presentation of a preceding trial. Any difference of representation of the fall of a real or a simulated ball should be particularly detectable in this first trial.

Position data given by the program were passed through a second-order Butterworth filter with a cutoff frequency of 12 Hz to obtain velocity and acceleration. Then, prior to the analyses, CE data were subjected to outlier tests. We computed average CE per trial. We replaced CEs which did not lie between the average ±2SD of the same ball condition group. A total of 2.85% of all trials were replaced with the average in the real ball group and 2.71% in the simulated ball group.

Initiation time, MT and CE were separately analyzed in a 2 × 50 (ball condition × repetition) ANOVA with ball condition as a between-subjects factor and repetition as within-subjects factor. We also conducted two unpaired Student's *t*-tests between the two groups of ball condition, one on the interception rate transformed into *Z* scores and one on initial error. For all statistical tests, Newmann–Keuls post hoc tests were used for comparison of the means and an alpha level of 0.05 was used to identify significant effects. In the post-test 1 and 2, initiation time, MT and CE were separately analyzed in a 2 × 2 × 20 (ball condition in the test × ball condition in the post-test × repetition) ANOVA with ball condition in the test and ball condition in the post-test as between-subjects factors and repetition as within-subjects factor. We also conducted ANOVAs with ball condition in the test and ball condition in the

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[4] Vogels (2004) showed 45 ms as a minimum delay from which participants are capable of judging a haptic and a visual stimulus are asynchronous, whereas Morice et al. (2008) did not found any degradation of motor performances with delay inferior to 110 ms in a ball bouncing task using the same electromagnetic tracking system.

[5] Absolute error (AE) which corresponds to the mean of the absolute value of the error was also calculated. AE is generally used as an indicator of the general accuracy in response. However, as the analysis with this variable did not provide any additional information to the analysis with CE, we do not present it.

Table 1

Values for the real ball versus simulated ball effect on initiation time, MT, CE, interception rate and initial error in the test (*T*), post-test 1 (PT1) and post-test 2 (PT2).

Test	Post-test	Initiation time (ms)			MT (ms)			CE (ms)			Initial error (ms)		
		T	PT1	PT2	T	PT1	PT2	T	PT1	PT2	T	PT1	PT2
Real ball	Real ball	414	386	387	222	235	233	37	21	20	116	34	22
	Simulated ball		421	417		194	194		15	11		32	29
Simulated ball	Real ball	396	382	387	236	241	238	32	22	25	167	10	37
	Simulated ball		395	378		216	219		7	-2		26	36

post-test as between-subjects factors on interception rate and initial error. Finally, in order to examine the consistency of learning, as well as its retention, we calculated the mean CE on the 5 last trials in the test, and compared it to the 5 first trials of the post-test 1 with paired Student's *t*-tests. The same calculation was made on the 5 last trials of post-test 1 and 5 first trials of post-test 2.

3. Results

Repeated measures ANOVAs on the parameters of movement kinematics showed no difference in initiation time with ball condition (see Table 1), $F(1,30) = 2.04$, $p > 0.05$, but an evolution of initiation time with repetition, $F(49,1470) = 2.13$, $p < 0.05$, the post hoc analysis showing that initiation time in the first trial was later than in all other trials. The ANOVA on MT showed no difference in MT with ball condition, $F(1,30) = 1.41$, $p > 0.05$, and a decrease of MT with repetition, $F(49,1470) = 5.12$, $p < 0.05$. Mainly, post hoc tests showed that the first trial had longer MTs than all the other trials.

When considering performances, results showed no effect of the ball condition on CE, $F(1,30) = 1.09$, $p > 0.05$, but a decrease of CE with repetition $F(49,1470) = 10.13$, $p < 0.05$ and an interaction between ball condition and repetition, $F(49,1470) = 2.17$, $p < 0.05$ (Fig. 2). However, a post hoc test failed to show statistical difference between trials of the similar rank for the two ball conditions (i.e., there was no difference between first trials, no difference between second trials. . .). Finally, unpaired Student's *t*-tests showed no difference in interception rate, $t(30) = 1.50$, $p > 0.05$, but initial error was marginally different according to ball condition, $t(30) = 1.94$, $p = 0.06$ and differed from 0 for both groups, respectively, $t(15) = 6.80$, $p < 0.05$ and $t(15) = 8.32$, $p < 0.05$ for real ball and simulated ball conditions. This indicates that in both conditions, participants initially did not have good timing but improved trough repetition learning. Regardless of this learning process, there was a small trend in favor of the interception of the real ball: participants reacted 12 ms earlier and moved 39 ms faster in the first trial when intercepting the real ball, which corresponds to the difference in initial error of 51 ms.

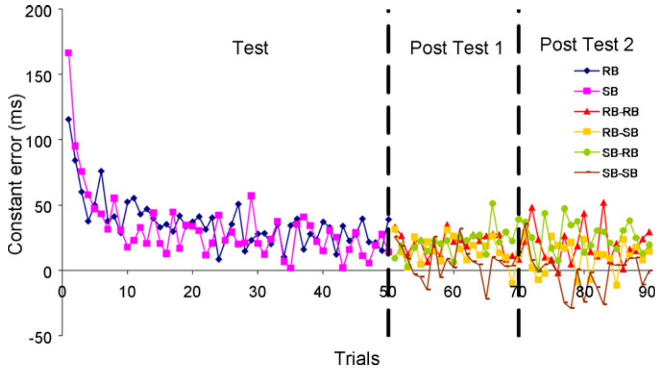

Fig. 2. Evolution of CE for real and simulated balls in the test, post-test 1 and post-test 2.

There was no effect of ball condition in the test or in the post-test performed 5 min after the end of the test, neither on initiation time nor on MT (see Table 1). However, ANOVAs on CE showed an effect of ball condition in the post-test, $F(1,28) = 4.32$, $p < 0.05$, suggesting that errors were larger in the post-test with the real ball, regardless of the ball condition in the test. Analysis of the second post-test, conducted 1 week after the test also failed to show a significant effect of ball condition on initiation time or MT. It also confirmed an effect of ball condition in the post-test, $F(1,28) = 9.38$, $p < 0.05$, one more time showing that errors were more larger when the post-test was using the real ball. An analysis of CE showed an interaction of ball condition in the test and ball condition in the post-test 2, $F(1,28) = 4.58$, $p < 0.05$, indicating that the error in post-test 2 for the VB-VB$_{\text{group}}$ was lower than the error of all the other groups. Moreover, the analysis showed that participants obtained a better interception rate when intercepting the simulated ball in the post-test 2, regardless which ball they intercepted during the test. Finally, the paired Student's *t*-test showed no difference between the performance at the end of the test and the beginning of the post-test 1, $t(31) = 1.13$, $p > 0.05$, nor between the end of post-test 1 and the beginning of post-test 2, $t(31) = 0.02$, $p > 0.05$. This confirmed that the practice in one session was sufficient to permit the same level of performance in the next session and that learning was sustained over time.

4. Discussion

The primary goal of our experiment was to test the ecological validity of simulation, and to determine to which extent such technology can faithfully provide information about human perception and motor performances in genuine reality. To this end, we examined whether the validity of computer simulation is challenged in the case of falling objects. If there is a difference to be reckoned with between the display of real and simulated objects, then it should surface in the interception of a free-falling object. In this case, only when confronted with a real object would participants combine the visual information with additional knowledge about the ball's motion, to the effect that the ball is being accelerated by gravity (see Zago et al., 2004). In simulated and impoverished situations, on the other hand, actors are taken to consider moving objects as having no mass or as not subject to gravity and to use a constant velocity strategy. As a consequence, if the ecological validity of the visual simulation did influence the control mode involved in interceptive timing, significant differences between the two conditions should have appeared in our data: performance should have been of better accuracy and better adaptation in the more natural condition. Our results did not confirm this expectation as none of the analyzed temporal variables yielded appreciable differences between reality and simulation.

It is to be noted that marginal effects were found in either direction. For instance, one analysis found a tendency ($p = 0.06$) for the initial error to be lower when intercepting the real ball compared to the simulated ball. This marginal effect suggests that participants were more likely to expect the falling ball to accelerate when real.

This result could indicate that participants initially have different expectations about the ball's trajectory, which allow a minimization of the initial errors in the real condition. An opposite marginal effect indicated better accuracy for the simulated ball in post-test 2 only. A number of side-results likewise invalidate the hypothesis of the use of two distinct models for actual and simulated acceleration, firstly that initial error in both conditions differed from 0 and secondly that no differences for CE in the test were observed according to ball condition. If participants had used two different control modes depending on the nature of the visual scenes, we should have obtained consistent differences for CE in our experiment. This was not the case. We also did not encounter a significant increase of CE, at least in the initial trials, for participants intercepting the real ball in the test and then the simulated ball in the post-test. The simulation did not seem to upset the visual system even after practice with the real object. On the contrary, we observed a significant decrease of CE for participants intercepting the simulated ball in the test then the real ball in the post-test. Also, we did not find any changes in performance for participants intercepting a different ball between the test and post-test. Our experiment thus provides arguments to claim that computer simulations are valid when studying interceptive actions. They seem to provide equivalent information for the control of hand movements and allow the same level of performance as when intercepting a real ball.

Our results also contradict the conclusions of Zago et al. (2004) about the different strategies that might be used according to the presence or the absence of a real falling ball to be intercepted. It is to be noticed that the primary goal of Zago et al. (2004) was not to compare real and simulated cases, and different motor tasks were used when intercepting the real (interception task) or the virtual ball (coincidence timing task). We suspect that the motor task is critical, as a matter of fact much more critical than the mode of the display upon which it is based. Besides the device used in the first experiment by Zago et al. (2004) some important differences existed between the expected time window that corresponded to the simulated ball and to the real ball falling behind the screen, specifically in the 0-g condition (see Baurès et al., 2007 for more details, but see Zago et al., 2008 for a reply). Consequently, participants could have been mislead by the difference in the time windows. This bias, rather than the involvement of an internal model of gravity, could explain why participants were less accurate in the 0-g condition than in the 1-g condition. Interestingly, in the second experiment carried out by Zago et al. (2004), in which there was no real ball falling behind the screen, and hence no conflict between the expected time window and real time window, the timing accuracy at 0 g was clearly better than at 1 g. As a consequence, the lack of control of time windows in their first experiment might challenge their conclusion regarding the use of two different models according to the nature of the ball to intercept (Baurès et al., 2007). In the present study, we tested a real-object interception against an analogous simulated object interception whilst keeping the task and the involved time windows identical. As no difference was found in these conditions, we suggest that the results obtained by Zago et al. (2004) were more likely due to the use of two different tasks and a lack of control of the time window rather that the presence or the absence of a real ball to intercept.

Note that while our results refute the use of an accurate 1-g model, they do not rule out that observers have some representation of the effect of gravity. The data plead in favor of qualitative implicit physics knowledge about the effect of gravity (Baurès et al., 2007) that could make participants react faster or earlier when the ball is expected to accelerate under the effect of gravity. Such qualitative knowledge would subtract a certain temporal margin from the expected TTC rather than computing the precise kinematics effects of gravity.

However, the effect of qualitative implicit physics knowledge is rather limited. Firstly, it is temporally limited because although we found a tendency for initial error to be different, CEs were clearly similar for both groups from the second trial onward, indicating that participants quickly corrected their expectation on the ball's fall on the basis of the visual information obtained in the first trial. Secondly, intuitive physics knowledge is limited in the information it provides for TTC estimation: even when initial error was better when intercepting the real ball, participants still committed late errors in the first trials for both ball conditions. The error pattern is compatible with the use of first order information and a constant velocity strategy (Tresilian, 1991), which does not give access to the exact TTC when the ball is accelerating (Benguigui et al., 2003), even if an implicit knowledge of the effect of gravity could minimize the error for the initial trial for the real ball. The decrease of the initial error in the first trials could correspond to a process of calibration (Jacobs and Michaels, 2006) allowing for an adjustment of the control law[6] involved in the timing of the interceptive movement, which could take into account the late responses produced on the first trials. Once calibrated, the relationship between information and movement would be learnt and used in future interceptions. This conjecture would explain the good results from the outset in post-test 1 and 2. In the same vein, the minimization of error in the first trial in the real condition could be due to an approximate initial calibration of the movement on the basis of implicit knowledge of the effects of gravity.

Finally, the lack of significant difference when intercepting a real or a virtual ball in our experiment challenges the concerns expressed by the ecological theory on the equivalence of genuine reality and frame-based optic simulation as is virtual reality (e.g., Michaels and Carello, 1981). It indeed appears on the basis of our experiment that an illusion of movement created by the fast succession of discrete and static images is comparable to real motion, leading for the observers to the same perceptual and motor processes. Two observations entail this conclusion. Firstly, participants reached the same performances with the same movement parameters (as soon as the second trial) when intercepting the real or the virtual ball. Secondly, participants kept the same level of performance from the test to the post test, whatever if the ball conditions were the same or not. Hence, the learning realized in one condition also applies for the other. Taken together, these observations refute the concerns expressed earlier on the use of virtual reality and argue for its use in perception–action studies.

It is worth noting however that we used a virtual interception in our task. This was done to avoid an impact of the ball on the hand that would have occurred only when intercepting the real ball. This impact is potentially informative and could be used to calibrate the interceptive movement, as shown in previous studies (Dessing et al., 2004; Zaal and Michaels, 2003). The use of a haptic glove, which would have made it possible to provide tactile feedback to participants, could permit to use real interception and compare the performances in the real and virtual conditions. However, such a glove could probably not recreate the necessary forces that act on the hand when catching a ball, and the equivalence of a real and a virtual interception thanks to this device is still uncertain.

In sum, even if the use of a simulated ball rather than a real ball may lead to subtle changes in movement parameters of the first trial, the results of this experiment have not confirmed the use of a sophisticated internal model of gravity that only comes to bear when intercepting a real ball. The interception of a simulated ball was executed with equal precision and equal error

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[6] A control law links kinematic properties of perceptual flow (e.g., ball's position or velocity) to kinetic properties of movement (e.g., hand's velocity or acceleration) (Warren, 1988).

margins. The data clearly speak against the assumption of two distinct perception–action control modes, one unable to account for acceleration and one able to do so in the face of a real falling object. Instead, people seem to be in the same mode based on first order information when intercepting a real or a simulated falling ball. In this sense, the simulation of free-falling objects is ecologically valid, the affordances[7] of real objects were preserved by their virtual counterparts and the same interceptive actions could be based upon them. Therefore, the results confirm that the use of virtual reality is appropriate to study interceptive actions of accelerated stimuli.

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[7] Affordances are the action's possibilities offered by the environment that afford relevant behavior to the organism (Gibson, 1977).