

Individual locomotor bias drives counterclockwise motion in pedestrian crowds

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This file contains all reviewer reports in order by version, followed by all author rebuttals in order by version.

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

This study systematically investigates the spontaneous counterclockwise (CCW) rotational preference in human walking through five well-designed experiments across diverse settings (Spain, Japan), populations (adults, teenagers, young children), and various conditions (confined circular arenas, open schoolyards, different crowd densities, groups with specific turning preferences, etc.). The core thesis posits that this CCW preference is universal, rooted in inherent individual locomotor tendencies rather than being primarily driven by collective interactions, boundary effects, or specific socio-cultural factors as traditionally assumed. The findings pose a significant challenge to existing pedestrian dynamics theories, particularly concerning the mechanisms of emergent collective behavior, and offer new perspectives for optimizing crowd management strategies. A key strength is the ingenious serial experimental design, which systematically eliminates various confounding factors (e.g., boundary effects, cultural avoidance habits, learned behaviors, physiological lateralities like handedness/footedness/eye dominance, and social norms via questionnaires), providing robust support for the central conclusions. Overall, the manuscript presents a promising idea, and the experimental design is generally sound. However, the logical flow and clarity of presentation need to be further refined.

Detailed Comments are as below,

1. While the study covers different cultures and age groups, 'universality' is a strong claim. It is recommended to discuss the applicable boundaries of this 'universality' more deeply in the discussion section. For instance, in scenarios such as extreme crowding, strong goal-driven movement (e.g., emergency evacuation), or under strong external guidance (e.g., specific signage), would this intrinsic CCW preference still be significant or dominant?
2. The current experiments primarily focus on 'free roaming' or situations without clear external goals. It would be beneficial to briefly discuss how this preference might manifest or be modulated by other factors in more complex daily walking tasks (e.g., shopping, commuting).
3. When discussing CCW motion, the circle antipode experiment type of study, especially the vortex phenomena observed during its progression, often comes to mind. Due to its high central symmetry, it is highly conducive to CCW research and should represent a very important set of experimental data. The authors could find an open-source dataset at the following link: <https://github.com/rickyspy/circle-antipode-experiment>. Perhaps they could explore whether this type of experiment supports the CCW phenomenon or consider it as a reference.
4. The average collective polarization parameter M (excluding young children) is mostly around 0.2-0.25, indicating a statistically significant but not overwhelming preference. The distribution of individual polarization parameters m_i (as in Figs. 7, 8c) shows that there are some individuals who consistently walk CCW ($m_i \approx 1$), some who consistently walk CW ($m_i \approx -1$), and many whose behavior is variable or involves straight-line walking. The discussion section could more meticulously analyze how this diversity in individual behavior patterns aggregates to form the observed collective M value.
5. The young children group exhibits a very strong CCW preference ($M > 0.7$, PDF(M) peak close to 1). The paper mentions this might be related to imitative behavior. Could this be explored further: Is it that young children have a stronger innate CCW preference, or are they more susceptible to the influence of a few CCW pioneers, leading to strong conformity/imitative

behavior? More speculation or literature support in this part would be beneficial.

6. The paper successfully rules out several common laterality factors (handedness, footedness, eye dominance). While acknowledging that the 'biological roots are unclear,' could the authors slightly expand on possible directions or hypotheses? For example, are there preliminary studies or theories regarding the vestibular system, cerebellar functional asymmetry, or some inherent bias in neural circuits related to spatial navigation? Even posing some open questions could better inspire future research.

7. The results in Figure 5 are very interesting: if a social norm exists, it surprisingly tends towards CW motion, which is contrary to the observed CCW behavior. It is recommended that the authors provide a more in-depth interpretation of this phenomenon. Does this imply a discrepancy between people's 'expectations' (empirical and normative) and their 'unconscious or spontaneous behavior'? Does this discrepancy itself reflect some interesting cognitive or cultural phenomenon?

8. Figure 1e shows the time-averaged collective polarization parameter M under different experimental conditions, with error bars representing the standard error of the mean. It is suggested to provide the specific M values, standard deviations, and sample sizes (e.g., number of repetitions or duration of analysis) for each experimental condition in the supplementary materials. This would help readers more accurately assess differences between conditions.

9. The paper mentions that young children show a strong CCW preference during 'free running' in a eurhythmics activity. To further rule out potential guiding factors (e.g., whether the teacher unconsciously guided them in a CCW direction, or if there were asymmetrical visual stimuli in the activity area), it is advisable to provide a more specific description of the 'freedom' in 'free running' and the teacher's role in the Methods section (if data allow).

10. For a confined circular arena, the center point is clear. However, for a large open area like a schoolyard, how is the center point selected? If the overall center of mass of the crowd drifts significantly during the experiment, would this affect the calculation and interpretation of M values? A brief explanation in the Methods section is recommended.

11. In the 'Individual Motion' experiment, some subgroups (e.g., left-handed $n=14$, left-footed $n=18$) have relatively small sample sizes. Although statistical tests found no significant differences, the statistical power to detect a small, yet real, effect is low with small samples. It is suggested to prudently mention this when discussing these 'no significant difference' results, i.e., the possibility of a weak effect cannot be entirely ruled out.

12. The discussion section mentions the potential applications of the findings in urban planning and crowd management. Providing 1-2 more specific examples or directions would be more illuminating. For instance, how can this CCW preference be considered and utilized when designing museum flow paths, stadium entry/exit routes, or crowd guidance in large open spaces like public squares?

13. The description of the right-eye patch experiment subjects and comparison groups in the Methods section of 'Individual Motion' and in the labels of Figure 8e could be clearer. For example, in Figure 8e, 'R-Eye Patch' is divided into 'No' and 'Yes.' The text mentions, 'restricting the analysis to participants with right-eye dominance revealed no meaningful difference in m_i between those who wore a patch and those who did not.' Does this imply that both the 'No' and 'Yes' groups consist of right-eye dominant individuals? If so, it's advisable to clarify this in the figure caption or Methods to avoid readers misinterpreting it as a comparison between wearing and not wearing an eye patch among all individuals.

Reviewer #2

(Remarks to the Author)

This is a well written paper which concludes that counterclockwise bias is universal and individual rather than coming from collective dynamics. The paper makes a clear case to justify this hypothesis, making use of a range of well-thought and executed experimental studies. In particular, the individual motion experiment is very relevant to back up the hypothesis, so I congratulate the authors for coming up with this idea.

I recommend this paper for publication after addressing the following comments, which mainly aim at providing more information on the assumptions adopted, clarifying the study limitations, adding relevant information on experimental studies to increase their repeatability, and detailing further ideas for future studies which could be a follow up of the paper.

Thanks,
Enrico Ronchi

1) The paper clearly motivates the scientific hypothesis of individual counterclockwise motion, but it is not able to explain the reason for such behaviour, which means it can open up for several future studies in this area. I would suggest the authors to further expand on what research directions could be taken to understand why such behaviour is observed. Technology like eye tracking for instance could be helpful. In addition, VR experiments have been more and more adopted in recent time in the pedestrian dynamics field and could be recommended here as a future direction to investigate what type of sensory stimuli is dominantly affecting the counterclockwise motion. For instance, it seems that the experimental studies presented here mostly focused on visual stimuli, while it could be interesting to investigate the impact of other types of sensory stimuli in isolation (e.g. auditory or even haptic) or combined to investigate to which extent this is a purely visually-based turning decision. Sensory stimuli could be fairly easily isolated in a VR experimental setup for instance.

- 2) The findings are robust and based on a wide range of experiments. Nevertheless, it is important to acknowledge that human population is ageing in most countries and crowds include a significant number of people with functional limitations (e.g. mobility or other impairments). Therefore, the generalisability of the findings should be discussed in light of older groups and people with functional limitations. This is particularly important in case of crowd motion, where heterogeneity of the crowd could (in some instances) impact the whole movement. Your sample characterisation could be further expanded to discuss the impact of age (i.e. not just children vs adult) and functional limitations.
- 3) The paper includes an interesting discussion on the CCW tendency vs the dimensions of the space people are in. This could be further expanded, as it may not be related just to presence of physical boundaries, but also to the (perceived) distance to such boundaries (i.e. if they are visible or not and how they are perceived).
- 4) Regarding the experiments in a nursery school, the assumption of young children being less affected by social influence and in general, treating them as a comparison source with adult behaviour may need further discussion. For instance, there are experimental studies (<https://doi.org/10.1016/j.firesaf.2023.103797>) reporting fairly different movement of pre-school children in case of narrow spaces compared to adults. Such experimental work shows that preschool children prefer squeezing through narrow spaces and form bottlenecks rather than navigate obstacles in such scenarios.
- 5) When referring to normative social influence in emergency scenarios, it would be relevant to refer and discuss the classical work by Latané and Darley
- 6) Regarding the methodological section, it would be good to report further details on sample size choices (was this based on power analysis or based on related previous experiments?)
- 7) Please discuss further the findings of social norm elicitation based on inferential statistics.
- 8) In general, I suggest to mild down some statements (e.g. Line 289), as the generalisation of the CCW motion finding can be further discussed (and investigated in the future) considering the walking task and conditions. For instance, it would be interesting to further investigate if this motion depends on the time available for the turning decision (i.e. if it is prominent only when the turning is “automatic” rather than a conscious well-thought decision, i.e. to force people in an experimental to think for longer time before they decide where to turn). This also relates to the need to further discussing the time intervals selected (and observed) for taking turning decision in the experimental studies, i.e. if this CCW motion is an inner individual feature, does the turning asymmetry occur or not when people have more time (e.g. either due to lower speeds or time at decision points) to decide the direction to choose? Is there a decision time after which CW and CCW become symmetrical again? Those are all interesting questions that are worth discussing

Minor comments

- I suggest reducing the number of acronyms to the bare minimum as they may negatively affect readability
- In some instances, language should be more formal (e.g. expressions like “a little bit” or similar should be changed to more formal quantification).
- Line 188 (should “patent” be “present”?)

Reviewer #3

(Remarks to the Author)

The article presents a comprehensive investigation of an intriguing phenomenon in human locomotion: spontaneous walking counterclockwise. As an aside, I am wondering how the British publisher feels about using the word ‘counterclockwise’, instead of ‘anticlockwise’, but I leave it to the copyeditor. The contribution is novel and interesting, and I do not have reservations about the rigour of the experiments, or their analysis. I am also aware that the subject has been discussed in the collective motion community for some time, and attracted a lot of interest during conferences. Thus, I am strongly in favour of publication in Nature Communications, but I also have a few suggestions how the work could be better contextualized.

1. The introduction starts by discussing lane formation. I am not sure if this is helpful for the following reasons: (a) intentional bidirectional motion is significantly different than free roaming, so I am not sure that the symmetry breaking mechanisms are related at all; (b) some definitive statements lack empirical confirmation, e.g. there is no citation to support the statement that “In most countries, lanes tend to form to the right” (line 54), or a citation to support the hypothesis that lanes form because “individuals tend to follow others who move in the same direction” (line 34). In summary, I would recommend more care in describing the literature on lane formation and relating it to the present study.

2. Given how important the rotation parameter is to the analysis, I suggest moving equation (1) to the main body of the manuscript. Related to this, I also have a fundamental question. The rotation is always defined with respect to the centre of the arena, but what happens if the arena gets bigger and bigger, or less and less circular? Do we expect to get one large vortex or is it possible that the crowd will break into several smaller vertices rotating around local centres? Maybe when we cannot perceive the centre, the CCW motion ceases? I understand that answering these questions would require separate experiments, beyond the scope of this study, but I think it would be important to address this matter in the discussion. In other words, the authors have chosen to measure global rotation about the centre of the arena, which is more specific than just CCW motion. Apart from global CCW roaming, pedestrians could also execute various local CCW motion that could be detected by looking at individual acceleration vectors. I was wondering if the authors could elaborate on this choice.

3. For understandable reasons, the article is written for a physics-minded reader, interested in active matter and complex systems. I am a member of this community myself, and I find it very interesting. However, I imagine the article would also be of interest to biologists interested in human motor control. If I understand correctly, our best hypothesis is that the tendency to move CCW is physiological in nature. I was wondering therefore, how we could attract the attention of the biological

community to this article. Nature Communication as an interdisciplinary journal is definitely a good venue. Maybe it would also be worth consulting this finding with a colleague in biomechanics? Maybe the authors could also cite some more biological literature? Unfortunately, I cannot recommend anything myself, but I found an old article titled "Spiral movement in man" By AA Schaeffer interesting. Again, it is all going beyond the scope of this article, but I think it would be worth making sure that this article makes impact worthy of its potential.

4. At times, the language of the article lapses into colloquialisms. Words such as 'bluish' (line 144) or 'nice' (line 264) should be avoided.

Reviewer #4

(Remarks to the Author)

I read this paper with interest and I think that it is very good research. Specifically, the studies are well conducted and the analyses sophisticated and well-reported. My questions therefore focus less on the research and results themselves, and more on the assumptions underpinning the work, the reasoning behind the contributions made and the meaning of some of the findings.

Regarding the assumptions, the paper opens by mentioning a very limited set of reasons why self-organised structures form. Also in the discussion it is said "emergent collective behaviours in pedestrian dynamics have been attributed **solely** to local interactions and social coordination". I believe that this list is too short. Moreover, the multiple reasons that exist do not exclude one another. Birds can make beautiful swarms or murmurations because they follow very basic rules when in the air, because they are sociable and migratory animals, because they seek to mate, because dusk sets in and they look for a suitable place to roost and because they have evolutionarily encountered predators with certain properties they want to evade (e.g., able to fly in particular ways, rather than run): none of these explanations are mutually exclusive; indeed for some species all play a role. The current paper presents explanations in an either/or manner and I find this not so persuasive. And as the long list of factors involved in such phenomena in animals suggests, I believe that the entire spectrum of reasons for these phenomena for human behavior is even broader. The authors ignore factors that obviously play a role. For example, the physiology of the human body must be a factor. Because the present paper claims to have excluded the most obvious explanations of the phenomenon, it is important to demonstrate that the explanations mentioned exhaust the universe of possible/most likely explanations.

Regarding the first contribution, it is said. "First, we provide robust experimental evidence showing that the CCW bias is a universal characteristic of human locomotion, observable even in isolated trials. Our findings demonstrate that the symmetry-breaking phenomenon arises from innate individual predispositions, while ruling out the most obvious explanations—such as handedness, footedness, and eye dominance—thus leaving the precise origins open for further investigation." I have a number of observations. First, there is a tension between claiming the phenomenon arises from innate predispositions and stating that its origins are unknown and require further investigation. Indeed, I read the paper as an excellent set of studies that attempted to rule out some obvious explanations. But from a series of null effects (it's not handedness, it's not eye dominance, etc.) I believe that one can at best (if the studies are flawless, which is always a tricky assumption) infer that it may not be these things that are causing the effect. So my impression is that the true contribution of the paper lies in ruling out some explanations, leaving us with an intriguing mystery for future research.

Regarding the second contribution, it is said: "Our findings suggest that nontrivial group behaviors can originate from basic individual locomotor predispositions". Although this claim is slightly contradicted by the point above (a series of null effects can not logically prove that the effect is due to basic individual locomotor predispositions) I am also wondering how this is new. Maybe I am missing the point, but is this not what researchers in pedestrian dynamics have shown many times over, and is this not also what biologists have shown when modelling animal collective behaviour?

Finally, I believe that the study showing these are individual tendencies is very informative. If the study were presented first, then all other studies are as expected. So what this leads me to suggest is that the contribution of the paper could also be described as showing that whilst some have thought the CCW preference was a collective phenomenon, it is not.

A final point is that throughout the paper, I was trying to find indicators of the magnitude of effects and for the magnitude of variability. What does it mean that $M \sim .2$? And what are the implications of the fact that M is not 1.0, for the use of the work "universal" in this paper?

Specific questions/comments.

You write "his task allowed us to categorize participants as left-turners (LT) or right-turners (RT) based on the direction of their turn." There seems to be an assumption that the execution of a single task is sufficient to characterize individuals as LT or RT. What is the basis of this assumption? What is the reliability of the test? What does that mean for the subsequent interpretation of the results, which hinge on the assumption that the groups are composed of 100% RT, etc.?

I think the study with children rests on an unfounded assumption that culture only begins to affect human beings when they enter primary school. The very fact that children start using language from age 1, shows otherwise. I also note that the source to corroborate this claim is a book about the development of morality, not about the acquisition of social motor skills.

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

The manuscript is excellent and I have no further major concerns.

I have only one minor suggestion for the authors to consider regarding the title. The term "Universal" is a very strong claim. While the main text and discussion do an excellent job of rigorously defining the scope and boundary conditions of this preference, the title itself might give readers an overly absolute first impression and could attract unnecessary debate.

You might consider a slightly more nuanced term to pre-empt potential debate. For example, "An Intrinsic..." or "A Widespread Counterclockwise Bias..." could also capture the significance of your findings effectively.

This is merely a suggestion for your final consideration. Congratulations on an outstanding study.

Reviewer #2

(Remarks to the Author)

I thank the authors for addressing my comments, I think the manuscript has improved and is now ready for publication.

Reviewer #3

(Remarks to the Author)

The authors have addressed all of my comments professionally and accurately. I am happy to recommend the article for publication.

Reviewer #4

(Remarks to the Author)

I thank the authors for their responsiveness and the revisions made. While I continue to support the publication of this paper, I have one remaining, fundamental objection regarding the claim of "universality" that must be addressed.

1. Core Issue: The Misleading Claim of "Universality"

My primary concern is the repeated claim that the research "demonstrates" a "universal" CCW or CW bias. This terminology is both empirically unsupported and conceptually misleading, a point also raised by other reviewers.

The authors have added a paragraph to clarify their use of "universal," but this clarification to me confirms the term's inappropriateness. They state that universality "should not be interpreted as an absolute law" and that the bias only emerges "when competing influences are minimal." This directly contradicts the standard meaning of the word and the strong inference it creates for readers.

I am concerned this is not the right term to use on three levels:

Empirical: The data itself argues against a strong universality claim. With an effect size of $M \sim 0.2$, a significant portion of participants do not exhibit a CCW bias. A universal trait (e.g., having lungs) would show a near-perfect distribution, which is not the case here. The data supports a statistical tendency, not a universal law.

Logical: It is logically impossible to prove universality across all human contexts based on only five studies. At best, the findings suggest a "robust tendency observed across the limited cultures and contexts tested so far." Claiming universality based on this sample is an overreach.

Terminological: The word "bias" is used ambiguously throughout the paper, referring to individual, between-individual, and collective levels. To claim "universal counterclockwise bias" without specifying which level is meant adds significant confusion.

Suggested solution: I advise to replace "universal" with more precise and defensible language throughout the manuscript, particularly in the abstract and conclusions. If it were about handedness, one would not write "demonstrating that handedness bias is universal" but simply "there are more right-handers". IN the same way, I would advise a rephrasing that is more straightforward, close to the evidence at hand and understandable without a dictionary, such as: "the CCW bias occurs because individuals make CCW turns more often". If authors want to write something about how it could be universal across other cultures and settings than the 5 settings now studied, they will have to use more speculative language (e.g., "this could be").

2. Specific points from previous reviews not addressed (related to point 1)

On Effect Size ($M \sim 0.2$): My original question was about the implications of $M < 1.0$ for the universality claim. The authors' response explained what $M \sim 0.2$ means statistically but did not engage with its conceptual implication: that the data show a deviation from universality, not a confirmation of it.

On the "Series of Null Effects": The revised text now correctly explains the findings as a series of consistent results. However, consistency across five studies is evidence of robustness, not universality. The leap to "universality" remains unjustified.

3. Minor Point: strengthening the "Social Norm Elicitation" section

The new paragraph on social norms (page 5) is a step in the right direction, but can be improved.

Citation of Latané & Darley: While placing the work in a social science context is valuable, the citation to Latané and Darley's bystander intervention work is tangential, as it does not directly address movement choices. Furthermore, the bystander effect itself has been nuanced by recent meta-analyses (e.g., Fischer et al., 2011; Levine & Manning, 2015). I recommend citing literature more directly relevant to movement and social norms, such as the work of Anna Sieben (on shared cultural understandings of the situation, social conventions and emergent dynamics in crowds) and Anne Templeton (on shared identity and movement).

Now, from the perspective of that literature, the new paragraph (lines 223-234) can be improved further. I believe that what the authors are saying (when they use the word "intuition", which I would avoid because it brings in yet another literature) fits with a distinction between different kinds of norms. I think that what the authors are trying to say here is better explained through the lens of explicit vs. implicit norms.

Explicit norms: known, codified rules (e.g., "drive on the right"). The questionnaire correctly shows no evidence for an explicit norm governing turning direction. Implicit norms or conventions: automated, situation-specific behavioral conventions people may not be consciously aware of (e.g., subtle traffic interactions). The data suggests that if an implicit norm for CCW turning exists, participants are not conscious of it.

This framework provides a clearer, more theoretical grounding for the authors' findings on participant awareness. It ties the current findings to Sieben's and Templeton's way of thinking (and their focus not on explicit norms, but on conventions and implicit norms). It also avoids making a complex and I think problematic distinction between personal and social norms (the personal norms are social too, if they stem from social injunctions or law? The social norms remain a bit hazy for me). In sum, for reasons my advice: stick more closely to the work that is directly relevant to pedestrian dynamics.

Version 2:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

The authors have addressed all of my comments, and I'd like to recommend the article.

Reviewer #4

(Remarks to the Author)

My thanks to the authors for their responsiveness and patience with what are, to some extent, semantic issues. They can easily crop up in multidisciplinary discussions: in my field the term "universal" is a big claim to make... But this is now resolved really well. Thanks for the careful adjustments made: the title and abstract and discussion are phrased in exactly the right way from the perspective of my disciplinary background. I also believe that the paper is now integrated really well with the social science side of the pedestrian dynamics literature, as far as I know it. I thank the authors for all that: this is a great paper and an exemplary piece of careful research that really advances our understanding of the origin of this intriguing phenomenon. I hope to see it in print soon.

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Authors' Response to Reviewers' Comments

Dear Editor,

Thank you very much for handling our manuscript and for seeking referees with such an exceptional level of professionalism. Their reports reflect a remarkable interest in our work and a particularly careful reading of it. Indeed, they provide numerous constructive comments and suggestions, most of which we have carefully implemented. Some of these changes led to improvements in the main manuscript, while others were incorporated into the Supplementary Material.

In response to the referees' and your own valuable recommendations, we have introduced several important modifications: (i) the title has been slightly revised to be more specific and better reflect the scope of the study; (ii) the conclusion of the statistical analysis has been rephrased for greater clarity and accuracy; (iii) the Ethical Considerations of the experiment involving teenagers have been explicitly included; (iv) we now provide a working link granting full access to the anonymized data of all our experiments; and (v) the reporting summary has been updated, with irrelevant fields removed to streamline the information.

As a result of these and other additions, the Discussion section may now appear somewhat lengthier, as it integrates the many ideas and potential directions for future work suggested by the referees.

In any case, after all the improvements made and given the interest raised by our work among the referees, we are very confident that the manuscript is now ready for publication in Nature Communications.

Below, we have included a point-by-point reply to all remarks made by the Reviewers. For clarity, we quote Reviewers' Comments in **blue** font, our replies in **black**, and the modified text in the manuscript in **red**.

Yours sincerely,

The authors.

REVIEWER #1 (REMARKS TO THE AUTHOR):

This study systematically investigates the spontaneous counterclockwise (CCW) rotational preference in human walking through five well-designed experiments across diverse settings (Spain, Japan), populations (adults, teenagers, young children), and various conditions (confined circular arenas, open schoolyards, different crowd densities, groups with specific turning preferences, etc.).

The core thesis posits that this CCW preference is universal, rooted in inherent individual locomotor tendencies rather than being primarily driven by collective interactions, boundary effects, or specific socio-cultural factors as traditionally assumed. The findings pose a significant challenge to existing pedestrian dynamics theories, particularly concerning the mechanisms of emergent collective behavior, and offer new perspectives for optimizing crowd management strategies.

A key strength is the ingenious serial experimental design, which systematically eliminates various confounding factors (e.g., boundary effects, cultural avoidance habits, learned behaviors, physiological lateralities like handedness/footedness/eye dominance, and social norms via questionnaires), providing robust support for the central conclusions. Overall, the manuscript presents a promising idea, and the experimental design is generally sound. However, the logical flow and clarity of presentation need to be further refined.

We thank the referee for acknowledging the breadth of our experimental campaign. We consider these tests in diverse contexts as an essential point to demonstrate the generality of the behaviour discovered. Moreover, we are grateful for the recognition that our results pose a significant challenge to existing pedestrian dynamics theories on collective behaviour. We are convinced that this perspective could inspire new theoretical models and practical strategies for crowd management.

Finally, we also thank the comments on the necessity of refining the logical flow and clarity of presentation. We have worked hard on this, and we believe that the manuscript is now much better, more precise, and easier to follow than in its previous version.

In what follows, we provide a point-by-point response detailing the changes we made in the manuscript.

1. While the study covers different cultures and age groups, ‘universality’ is a strong claim. It is recommended to discuss the applicable boundaries of this ‘universality’

more deeply in the discussion section. For instance, in scenarios such as extreme crowding, strong goal-driven movement (e.g., emergency evacuation), or under strong external guidance (e.g., specific signage), would this intrinsic CCW preference still be significant or dominant?

We appreciate this thoughtful comment. The referee is totally right. The CCW bias shown in our manuscript is always in relatively low density conditions and in scenarios where pedestrians are roaming. As it will be seen below, and thanks to the referee's comment, in the new version of the paper we demonstrate the presence of this bias also in an independent open dataset of antipodal experiments. Therefore, the CCW behaviour can extend to other scenarios where there is a goal-driven movement, but we do not believe that this would be the case in emergency evacuations or strong external guidance.

As a consequence of this comment, we have modified the title of the manuscript, added a couple of words in the abstract and included a paragraph in the Discussion section that reads as:

At the same time, the notion of universality should not be interpreted as an absolute law. Our experiments reveal a robust underlying bias that tends to emerge when competing influences are minimal. In more complex scenarios—such as emergency evacuations, dense crowds, or environments shaped by signage or architectural constraints—other mechanisms may attenuate or completely eliminate the CCW tendency. For example, a recent study showed the emergence of chiral motion within an extremely dense crowd; however, the symmetry does not appear to be broken, as the size and number of CW and CCW regions is proved to be similar [9]. In more diluted scenarios, one way to test the applicability limits of CCW motion is by considering tasks with strong symmetry and explicit goals. A first step in this direction is the analysis of antipodal (circle-antipode) experiments, where participants start from opposite points and converge toward a common centre before dispersing [37]. Despite the imposed goal structure and high central symmetry, preliminary analysis of such experiments reveals that CCW bias is still present (see Supplementary Fig. 3). This finding suggests that the CCW preference is more resilient than anticipated, even under structured conditions. Moreover, our study participants were healthy children, teenagers,

and young adults, leaving open the question of whether the same tendency holds in older adults or individuals with functional limitations, whose mobility, balance, and perception may differ substantially. Exploring these types of groups could not only test the limits of CCW motion but also provide valuable insights of its biomechanical origin.

2. The current experiments primarily focus on ‘free roaming’ or situations without clear external goals. It would be beneficial to briefly discuss how this preference might manifest or be modulated by other factors in more complex daily walking tasks (e.g., shopping, commuting).

In line with the previous point, we totally agree with the referee that our experiments focused on free-roaming situations. As clarified in our response to Comment 1, in the new version of the manuscript (see paragraph introduced above) we emphasise that the CCW bias is a robust predisposition rather than an absolute rule, and its expression can be attenuated in more complex daily walking tasks. We expect further research in this direction as stated in the previous to last sentence of the manuscript.

3. When discussing CCW motion, the circle antipode experiment type of study, especially the vortex phenomena observed during its progression, often comes to mind. Due to its high central symmetry, it is highly conducive to CCW research and should represent a very important set of experimental data. The authors could find an open-source dataset at the following link: <https://github.com/rickyspy/circle-antipode-experiment>. Perhaps they could explore whether this type of experiment supports the CCW phenomenon or consider it as a reference.

We thank the referee for directing us to these interesting experiments. In response, we analysed the open-source dataset and added Supplementary Figure 3 (included also here as Fig. 1), which reports the average collective polarization $\overline{M}$ for each experimental condition, following the same representation as in Fig. 1 of the main manuscript. Remarkably, even under this highly symmetric configuration, where all pedestrians pursue well-defined and opposing goals, a consistent CCW bias was observed. This additional analysis reinforces the robustness of our conclusions by showing that the effect extends beyond the free-roaming scenarios investigated in our experiments. As indicated in point 1, we have also incorporated this new analysis into the Discussion

section, where it is presented as further evidence that the bias is resilient even in structured and goal-driven contexts.

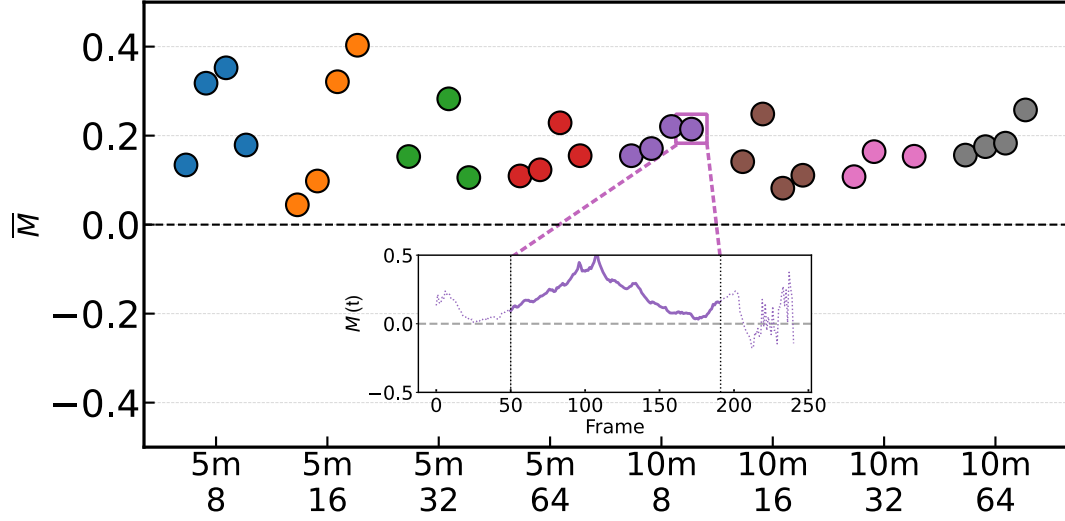


FIG. 1. **Antipodal Experiments.** Time-averaged collective polarization ($\overline{M}$) across the different experimental conditions reported for the antipodal experiments in [7]. Each value of $\overline{M}$ is obtained from the time series of the collective polarization $M(t)$, excluding the first and last 50 frames (2 s each). The inset shows a representative example of such a time series, with the solid line indicating the data segment used for the computation of $\overline{M}$.

4. The average collective polarization parameter M (excluding young children) is mostly around 0.2–0.25, indicating a statistically significant but not overwhelming preference. The distribution of individual polarization parameters m_i (as in Figs. 7, 8c) shows that there are some individuals who consistently walk CCW ($m_i \approx 1$), some who consistently walk CW ($m_i \approx -1$), and many whose behavior is variable or involves straight-line walking. The discussion section could more meticulously analyze how this diversity in individual behavior patterns aggregates to form the observed collective M value.

Thank you for this constructive suggestion. Indeed, the fact that the average collective polarization M remains around 0.2–0.25 reflects the **heterogeneity of individual behaviours**. As shown in Figs. 7 and 8c, the distribution of m_i reveals three recurrent groups: some participants walk almost exclusively CCW ($m_i \approx 1$), a smaller fraction walk CW ($m_i \approx -1$), and many display values close to zero, either because

they alternate between CCW and CW or because they often perform radial/straight displacements.

Formally, M is just the mean of all m_i , so any imbalance in these proportions explains the observed values. For example, if 40% of participants rotate almost entirely CCW ($m_i \approx 1$), 20% rotate CW ($m_i \approx -1$), and the remaining 40% have mixed trajectories but averaging around $m_i \approx 0$, the expected mean is around

$$M = 0.4(1) + 0.2(-1) + 0.4(0) = 0.2,$$

which falls squarely in the observed range. This kind of composition—dominated by a CCW peak, a weaker CW peak, and a large neutral mass—is exactly what we see in the empirical distributions.

Indeed, the idea of constructing synthetic distributions (Fig. 8d) from randomly choosing values of m_i from experiments with pedestrian walking alone, was to demonstrate the origin of both the 0.2–0.25 value of M as well as the increasing narrowing of the distribution as the number of pedestrians chosen (or participating in the experiments) increases.

In the new version of the manuscript, the beginning of the last paragraph before the Discussion has been rewritten to clarify this point, which obviously, was not clear in the previous version of. Thank you again for this.

Going a step further, we try to connect the individual motion results with the values of collective polarization (M mostly around 0.2 in Fig. 1 as well as their distributions), where we observed a clear narrowing of the peak with the crowd size (Figs. 2 and 4). To this end, in Fig. 8d we synthetically build the PDFs of M by taking the actual values of m from the individual pedestrians moving alone (as shown in Fig. 8b) in a random manner. In particular,...

5. The young children group exhibits a very strong CCW preference ($M > 0.7$, PDF(M) peak close to 1). The paper mentions this might be related to imitative behavior. Could this be explored further: Is it that young children have a stronger innate CCW preference, or are they more susceptible to the influence of a few CCW pioneers, leading to strong conformity/imitative behavior? More speculation or literature support in this part would be beneficial.

We thank the referee for this pertinent observation. Following the suggestion we have introduced more literature and a couple of sentences in the main text to justify the imitative behaviour of children. Indeed, it is known that preschoolers are highly susceptible to majority influence [9], preferentially adopt behaviours from non-dissenting peers [10], and tend to over-imitate others' actions even when causally irrelevant [11]. Moreover, coordinated musical or rhythmic activity (such as that used in the nursery experiment) is known to enhance prosocial alignment in early childhood [12]. We believe these works provide enough theoretical grounding for the strong conformity effects observed in this age group. The new text in the manuscript reads as follows:

...children tend to imitate their peers and end up walking in the same direction which, of course, is the CCW one. This is consistent with prior research showing that young children are highly sensitive to peer consensus, prone to over-imitation, and tend to align more strongly with others during rhythmic or musical group activities [24-27].

6. The paper successfully rules out several common laterality factors (handedness, footedness, eye dominance). While acknowledging that the 'biological roots are unclear,' could the authors slightly expand on possible directions or hypotheses? For example, are there preliminary studies or theories regarding the vestibular system, cerebellar functional asymmetry, or some inherent bias in neural circuits related to spatial navigation? Even posing some open questions could better inspire future research.

We thank the referee for raising this important point. After reviewing the relevant literature, we found that spontaneous turning behaviours in humans (particularly under conditions without visual cues) have been attributed not to constant biomechanical or neural asymmetries, but rather to drift in the subjective sense of straight-ahead, likely caused by accumulating sensorimotor noise [5]. Although Souman et al. found no consistent lateral bias across participants, they did show that a subset of individuals exhibit systematic circling behaviour even in the absence of external cues, and that the vestibular system likely plays a key role in this drift. We have incorporated a brief paragraph in the Discussion section to highlight this possible link and encourage future research connecting sensorimotor asymmetries and navigational biases in visually rich environments.

... unravelling their biological or neurological grounds. Interestingly, preliminary stud-

ies of blindfolded walking show that some individuals spontaneously circle in one direction due to accumulating drift in their subjective straight-ahead, possibly linked to vestibular noise [34]. While our experiments include full visual feedback, such findings suggest that subtle sensorimotor asymmetries might also contribute to the CCW bias observed here.

7. The results in Figure 5 are very interesting: if a social norm exists, it surprisingly tends towards CW motion, which is contrary to the observed CCW behavior. It is recommended that the authors provide a more in-depth interpretation of this phenomenon. Does this imply a discrepancy between people’s ‘expectations’ (empirical and normative) and their ‘unconscious or spontaneous behavior’? Does this discrepancy itself reflect some interesting cognitive or cultural phenomenon?

We thank the Reviewer for bringing this interesting point to our attention. In many other contexts of social interaction, people have personal norms that tell them how to behave; typically coming from moral, ethical, or legal regulations. Those personal norms can then be in contrast with the social norms in place, and it may be the case that people follow one type of norm over the other when they do not agree. In the context of interest here, it is not very plausible that people has a personal norm as to how to move around, so we agree with the Reviewer that the role of the personal norm comes from some kind of unconscious or intuitive behaviour. But then, if such predisposition is unconscious, it is very likely that people never reflected about it and therefore it conditions neither their expectations nor their behavior, something that a personal norm in the usual sense would certainly do. Hence, we do agree with the Reviewer’s interpretation and we have included a discussion of this interesting cognitive issue in the corresponding section.

The above result has interesting cognitive implications. In most social interaction contexts, people have personal norms that tell them how to behave in those situations, typically coming from moral, ethical, or legal regulations. Those personal norms can then be in contrast with the social norms in place, and it may be the case that people follow one type of norm over the other when they do not agree. Instead, in our experiment there is no reason to expect that people have such kind of personal norm in place, as the interaction involves no moral or legal issues. One could then conclude

that they have some kind of intuition, possibly unconscious, about what to do in this context, but this intuition has not reached the point in which people reflect on it and gives rise to expectations about others. It might then be the case that many subjects' answer in the survey comes from some cultural influence that does not align with that intuition or with their actual behavioural bias, leading to the observed discrepancy between expectations and behaviour, an issue that may merit further specific research.

8. Figure 1e shows the time-averaged collective polarization parameter M under different experimental conditions, with error bars representing the standard error of the mean. It is suggested to provide the specific M values, standard deviations, and sample sizes (e.g., number of repetitions or duration of analysis) for each experimental condition in the supplementary materials. This would help readers more accurately assess differences between conditions.

We have added in the Supplementary Table 1 the mean and standard deviation of M for each experimental condition, along with the number of repetitions. We hope this additional information will be useful to readers.

9. The paper mentions that young children show a strong CCW preference during 'free running' in a eurhythmics activity. To further rule out potential guiding factors (e.g., whether the teacher unconsciously guided them in a CCW direction, or if there were asymmetrical visual stimuli in the activity area), it is advisable to provide a more specific description of the 'freedom' in 'free running' and the teacher's role in the Methods section (if data allow).

As described in Ichikawa et al. (2021), during the "free running" activity, *"the instructor did not give explicit instructions, such as whom to run with or where to run"*, making this phase the one with the highest degree of freedom in the eurhythmics session. The instructor simply played the piano without interacting with the children. We have clarified this point in the Methods section of the manuscript to better convey the absence of guiding cues or asymmetries in the environment.

...while the instructor plays the piano (see Fig. 1c). As described in the original work, no instructions were given regarding the direction, purpose, or manner of running. The environment was symmetric, and the teacher remained passive, neither guiding

nor encouraging particular behaviours.

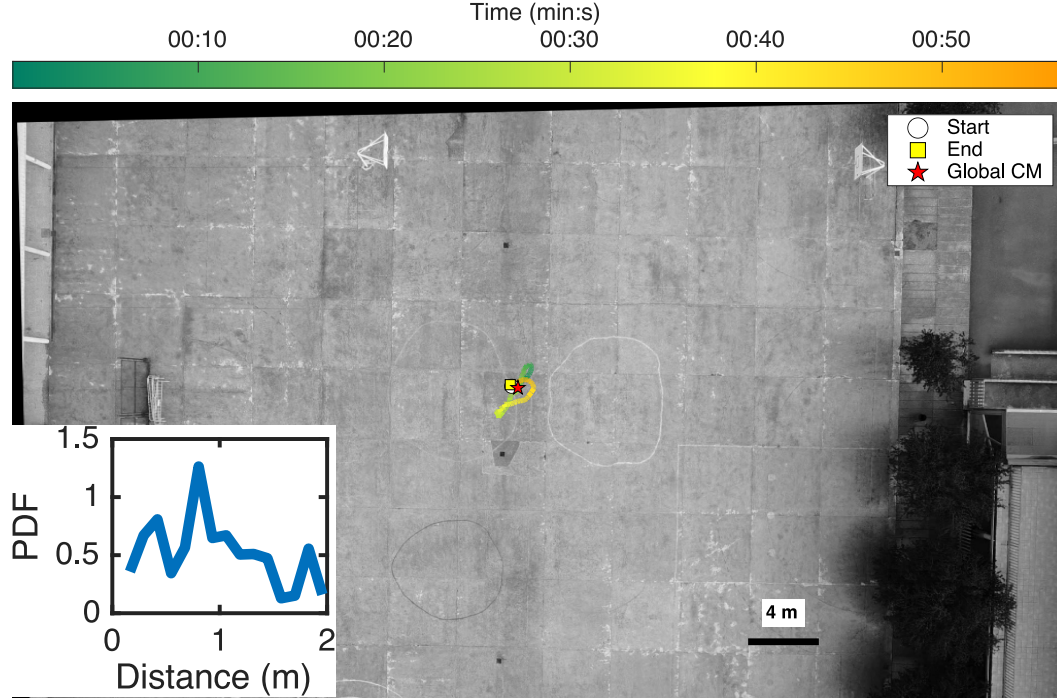


FIG. 2. **Center of Mass Time Evolution.** Center of Mass Time Evolution. Trajectory of the centre of mass (CM) for the experiment with 107 teenagers in a Spanish schoolyard. The CM at each time frame is computed as the mean position of all participants. Initial and final CM positions are indicated by white and yellow markers, respectively. The Global CM, obtained as the time average of the CM trajectory and used to calculate the individual m_i values, is shown in red. The panel at the bottom left reports the distribution of distances between the instantaneous CM and the Global CM.

10. For a confined circular arena, the center point is clear. However, for a large open area like a schoolyard, how is the center point selected? If the overall center of mass of the crowd drifts significantly during the experiment, would this affect the calculation and interpretation of M values? A brief explanation in the Methods section is recommended.

We thank the referee for this helpful observation. In the non-confined schoolyard experiments, where there is no fixed geometrical centre, we defined the origin as the global centre of mass (CM) of the crowd (i.e., the average CM over the entire duration of the experiment). To confirm that this choice is appropriate and does not intro-

duce significant bias, we have included Supplementary Figure 1 (included also here as Fig. 2), which shows both the trajectory of the instantaneous CM over time and the distribution of distances between the instantaneous CM and the global CM (averaged over the whole experiment). The average deviation is less than 1 meter, which is negligible compared to the dimensions of the schoolyard (approximately $50 \times 60 \text{ m}^2$). This confirms that the global CM serves as a stable and meaningful reference point for the analysis. A brief explanation and the figure have been added in the Methods section (Metrics and Nomenclature).

In experiments conducted in confined circular arenas, the reference point is naturally taken as the geometrical centre of the arena. For the schoolyard experiments, where no fixed central point exists, the reference centre used for computing polarization was the global centre of mass of the crowd, averaged over the entire sequence. The stability of this choice was confirmed by analysing the position of the instantaneous CM, which remained within 1 m of the global CM throughout the trial (see Supplementary Figure 1).

11. In the ‘Individual Motion’ experiment, some subgroups (e.g., left-handed $n=14$, left-footed $n=18$) have relatively small sample sizes. Although statistical tests found no significant differences, the statistical power to detect a small, yet real, effect is low with small samples. It is suggested to prudently mention this when discussing these ‘no significant difference’ results, i.e., the possibility of a weak effect cannot be entirely ruled out.

We thank the referee for this thoughtful observation. We fully agree that the subgroup sizes in the ‘Individual Motion’ experiment are relatively small, and therefore the statistical power to detect subtle effects is limited. While our analyses did not reveal any significant differences in polarization across laterality traits, we acknowledge that weak effects (if present) could go undetected in such small samples. We have added a sentence to the Results section to explicitly note this limitation and interpret the “no difference” findings with appropriate caution.

While these results indicate a consistent CCW bias regardless of these factors, we note that the sample sizes of certain subgroups—particularly left-handed ($n=14$) and left-footed ($n=18$) individuals—were relatively small, limiting statistical power. Therefore,

although the trend is robust, we cannot entirely exclude the possibility of weak or subtle effects.

12. The discussion section mentions the potential applications of the findings in urban planning and crowd management. Providing 1-2 more specific examples or directions would be more illuminating. For instance, how can this CCW preference be considered and utilized when designing museum flow paths, stadium entry/exit routes, or crowd guidance in large open spaces like public squares?

We thank the referee for this suggestion. We have now added two concrete examples related to the design of circulation paths in museums and the placement of entry/exit points in large open spaces to clarify potential applications. This has been incorporated into the Discussion.

...shopping centres. For instance, circulation paths in museums or exhibitions could be designed to follow the natural CCW bias to improve flow efficiency, while entry/exit points in large open spaces—such as plazas or stadium forecourts—might be more effective when aligned with this prevailing directional tendency.

13. The description of the right-eye patch experiment subjects and comparison groups in the Methods section of ‘Individual Motion’ and in the labels of Figure 8e could be clearer. For example, in Figure 8e, ‘R-Eye Patch’ is divided into ‘No’ and ‘Yes.’ The text mentions, ‘restricting the analysis to participants with right-eye dominance revealed no meaningful difference in m_i between those who wore a patch and those who did not.’ Does this imply that both the ‘No’ and ‘Yes’ groups consist of right-eye dominant individuals? If so, it’s advisable to clarify this in the figure caption or Methods to avoid readers misinterpreting it as a comparison between wearing and not wearing an eye patch among all individuals.

The referee is right. The explanation was very confusing. To clarify: in Fig. 8e the “Right-Eye Patch” comparison simply contrasts participants who did not wear a patch with those who did. The “Yes” group (patched) included individuals with both left- and right-eye dominance; it was not restricted to right-eye dominant participants. The purpose of the patch was to evaluate whether artificially constraining vision through the right eye could have an effect on the rotational bias

We have improved the explanations given in different sections of the manuscript in (Methods, Results, and Fig. 8e caption) to avoid any possible misunderstanding.

(Results) Likewise, the comparison between participants with and without a right-eye patch (the latter including both left- and right-eye dominant individuals) did not yield a statistically significant difference in m_i :

(Methods) Independently, to examine how reducing the visual field affects movement patterns, 49 participants wore a patch over their right eye. The patched group included both left- and right-eye dominant individuals, and was compared against the rest of the participants who walked without a patch

(Fig. 8e caption) The ‘Yes’ group corresponds to participants wearing a patch, including both left- and right-eye dominant individuals.

Thank you again for all the constructive comments and suggestions made, which have undoubtedly helped to improve the manuscript.

REVIEWER #2 (REMARKS TO THE AUTHOR):

This is a well written paper which concludes that counterclockwise bias is universal and individual rather than coming from collective dynamics. The paper makes a clear case to justify this hypothesis, making use of a range of well-thought and executed experimental studies. In particular, the individual motion experiment is very relevant to back up the hypothesis, so I congratulate the authors for coming up with this idea.

We sincerely thank the referee for the positive and encouraging assessment. We are particularly grateful for the appreciation of the individual motion experiment, which we also consider a key element to support the main hypothesis of the paper.

I recommend this paper for publication after addressing the following comments, which mainly aim at providing more information on the assumptions adopted, clarifying the study limitations, adding relevant information on experimental studies to increase their repeatability, and detailing further ideas for future studies which could be a follow up of the paper.

We thank the referee once again for the thoughtful and constructive feedback. Following these suggestions, we have implemented several changes in the manuscript that, in our view, have helped clarify key points and improved the overall quality of the manuscript. Our detailed response to each comment is provided below.

1. The paper clearly motivates the scientific hypothesis of individual counterclockwise motion, but it is not able to explain the reason for such behaviour, which means it can open up for several future studies in this area. I would suggest the authors to further expand on what research directions could be taken to understand why such behaviour is observed. Technology like eye tracking for instance could be helpful. In addition, VR experiments have been more and more adopted in recent time in the pedestrian dynamics field and could be recommended here as a future direction to investigate what type of sensory stimuli is dominantly affecting the counterclockwise motion. For instance, it seems that the experimental studies presented here mostly focused on visual stimuli, while it could be interesting to investigate the impact of other types of sensory stimuli in isolation (e.g. auditory or even haptic) or combined to investigate to which extent this is a purely visually-based turning decision. Sensory stimuli could be fairly easily isolated in a VR experimental setup for instance.

We very much appreciate this suggestion. In fact, it is quite a coincidence, because we

had already started developing ideas for VR experiments precisely along these lines. As the referee points out, VR offers tremendous potential to disentangle the role of different sensory inputs (visual, auditory, or haptic) under controlled conditions, something extremely difficult to achieve in real-world setups. Given the robustness of the CCW bias observed here, we believe VR will be an invaluable tool to probe why this asymmetry emerges, and it is reassuring to see that the referee independently highlighted the same direction we are currently pursuing.

To reflect this in the manuscript, we have added a sentence in the Discussion section outlining this future research direction:

In this context, virtual reality experiments provide a particularly powerful framework for studying this problem, as they allow sensory inputs to be isolated and systematically manipulated under controlled conditions [35, 36].

Other research directions - suggested by referee 2 or the other three - have been introduced in the fourth paragraph of the discussion section.

2. The findings are robust and based on a wide range of experiments. Nevertheless, it is important to acknowledge that human population is ageing in most countries and crowds include a significant number of people with functional limitations (e.g. mobility or other impairments). Therefore, the generalisability of the findings should be discussed in light of older groups and people with functional limitations. This is particularly important in case of crowd motion, where heterogeneity of the crowd could (in some instances) impact the whole movement. Your sample characterisation could be further expanded to discuss the impact of age (i.e. not just children vs adult) and functional limitations.

We thank the referee for raising this very relevant point. We agree that age-related factors and functional limitations are crucial aspects when considering the generalisability of our findings. Our current experiments primarily involved young adults, teenagers, and children, so the role of older adults or people with impairments remains an open question. To make this scope explicit, we have added a couple of sentences in the discussion section and a clarification in the Methods section when describing the experiments.

Discussion: Moreover, our study participants were healthy children, teenagers, and young adults, leaving open the question of whether the same tendency holds in older adults or individuals with functional limitations, whose mobility, balance, and perception may differ substantially. Exploring these types of groups could not only test the limits of CCW motion but also provide valuable insights of its biomechanical origin.

Methods: Across all experiments, participants were healthy children, teenagers, or young adults, ensuring consistency across conditions while leaving the exploration of older groups or people with functional limitations for future research.

3. The paper includes an interesting discussion on the CCW tendency vs the dimensions of the space people are in. This could be further expanded, as it may not be related just to presence of physical boundaries, but also to the (perceived) distance to such boundaries (i.e. if they are visible or not and how they are perceived).

This is an interesting point. Fig. 3 seems to reflect the existence of some patterns that perhaps merit further investigation. Nevertheless, the fact that the CCW motion persists in the ‘free of boundaries’ schoolyard made us jump to test other hypothesis. Of course, it could be argued that in this schoolyard scenario some sort of psychological boundaries still exist (as the children will never go outside the schoolyard). In any case, we believe it is fair to conclude that the CCW motion emerges even when the effect of the boundaries is minimized.

In any case we agree that perceptual effects are important when moving, and we have already tried to gain some initial intuition in this direction through the individual experiments with the eye patch, where the visual field was reduced. In the future, VR would provide a much more powerful framework to systematically explore how perceived distance or visibility of boundaries affects the CCW tendency.

Aiming a clarification of this perception issue, in the text we have added a sentence after the description of the density patterns (Confined Random Motion in Spain):

The density fields (first row, Fig. 3) show a rather homogeneous spatial distribution, yet some faint circular patterns can be perceived. These suggest that the position of the boundaries (or pedestrians’ perception of them) affects the motion within the arena.

4. Regarding the experiments in a nursery school, the assumption of young children being less affected by social influence and in general, treating them as a comparison source with adult behaviour may need further discussion. For instance, there are experimental studies (<https://doi.org/10.1016/j.firesaf.2023.103797>) reporting fairly different movement of pre-school children in case of narrow spaces compared to adults. Such experimental work shows that preschool children prefer squeezing through narrow spaces and form bottlenecks rather than navigate obstacles in such scenarios.

Thank you for this thoughtful point and for the helpful pointer to Najmanová & Ronchi (2023). Our intention in using the nursery-school data was not to argue that children are free from social influence, but rather that they are **less exposed to acquired adult conventions** (e.g., signage norms or learned circulation habits) that might bias directionality. The cited work also highlights that children exhibit distinct emergent movement patterns, which is highly relevant to our rationale, as our aim was precisely to analyse a context markedly different from the adult scenarios previously tested.

Aiming to clarify this in the text, we have rewrote the reasons why we included the nursery school experiments and added a reference to the cited work:

To explore this option, we analysed experiments implemented in a nursery school, as previous research has demonstrated that young children differ from adults in their emergent movement patterns [23], and it can reasonably be assumed that they are less influenced by acquired adult conventions (e.g. signage norms or circulation habits).

5. When referring to normative social influence in emergency scenarios, it would be relevant to refer and discuss the classical work by Latané and Darley.

Thank you for this. Although establishing connections of our work with emergency scenarios is not easy, we agree on the importance of this reference as a pioneering work referring to normative social influence. We have added the reference at the beginning of the section “Social Norm Elicitation” to give context to it.

6. Regarding the methodological section, it would be good to report further details on sample size choices (was this based on power analysis or based on related previous experiments?)

Thanks for this important question. The sample sizes in our study were based on previous experiments where the CCW motion was identified for the first time – and incorrectly attributed to pedestrian-wall interactions [6]. Based on the number of participants used in that work, we sought to keep a similar scale while controlling additional variables. The experiments are presented in historical order, reflecting this progression: starting from setups with comparable sample sizes to the original study, and then gradually changing the number of pedestrians (or repetitions) as new hypotheses were tested. In all cases, our guiding rationale was to ensure reliable data through multiple repetitions of the same experimental conditions, so that the statistical analyses remained robust.

We have added a clarification in the Methods section to reflect these points:

The experiments are presented in chronological order, reflecting the historical development of the project: initial studies adopted participant numbers consistent with previous related work [16], while subsequent campaigns progressively adapted the sample sizes (both in terms of group size and number of repetitions) to address new hypotheses.

7. Please discuss further the findings of social norm elicitation based on inferential statistics.

We thank the Reviewer for this suggestion but unfortunately, the survey to obtain the expectations was conducted without collecting any other data, i.e., we only asked about expectations and actions as described in the text. Therefore, we cannot carry out any further analysis based on inferential statistics as far as regressions are concerned. On the other hand, in terms of possible hypotheses testing, what we are presenting here is just the number of people who answered the questions in one way or another, and hence we do not have any specific hypotheses to test. Once again, this lack of hypotheses also comes from the data availability, limited only to the survey answers.

8. In general, I suggest to mild down some statements (e.g. Line 289), as the generalisation of the CCW motion finding can be further discussed (and investigated in the future) considering the walking task and conditions. For instance, it would be interesting to further investigate if this motion depends on the time available for the turning

decision (i.e. if it is prominent only when the turning is “automatic” rather than a conscious well-thought decision, i.e. to force people in an experimental to think for longer time before they decide where to turn). This also relates to the need to further discussing the time intervals selected (and observed) for taking turning decision in the experimental studies, i.e. if this CCW motion is an inner individual feature, does the turning asymmetry occur or not when people have more time (e.g. either due to lower speeds or time at decision points) to decide the direction to choose? Is there a decision time after which CW and CCW become symmetrical again? Those are all interesting questions that are worth discussing

We thank the Reviewer for these constructive suggestions, which align with Reviewer 1’s observation that some of our statements should be expressed with greater caution.

In response, substantive modifications have been introduced in the title of the manuscript and in the section discussing the limitations of our study. In the latter case, we now explicitly acknowledge that the CCW bias may not necessarily manifest under all circumstances (for instance, in high-density conditions, emergency evacuations, or goal-oriented tasks) or in populations not represented in our experiments, such as older adults or individuals with functional limitations. A new paragraph has been incorporated in the Discussion section to reflect these ideas:

At the same time, the notion of universality should not be interpreted as an absolute law. Our experiments reveal a robust underlying bias that tends to emerge when competing influences are minimal. In more complex scenarios—such as emergency evacuations, dense crowds, or environments shaped by signage or architectural constraints—other mechanisms may attenuate or completely eliminate the CCW tendency. For example, a recent study showed the emergence of chiral motion within an extremely dense crowd; however, the symmetry does not appear to be broken, as the size and number of CW and CCW regions is proved to be similar [9]. In more diluted scenarios, one way to test the applicability limits of CCW motion is by considering tasks with strong symmetry and explicit goals. A first step in this direction is the analysis of antipodal (circle-antipode) experiments, where participants start from opposite points and converge toward a common centre before dispersing [37]. Despite the imposed goal structure and high central symmetry, preliminary analysis of such exper-

iments reveals that CCW bias is still present (see Supplementary Fig. 3). This finding suggests that the CCW preference is more resilient than anticipated, even under structured conditions. Moreover, our study participants were healthy children, teenagers, and young adults, leaving open the question of whether the same tendency holds in older adults or individuals with functional limitations, whose mobility, balance, and perception may differ substantially. Exploring these types of groups could not only test the limits of CCW motion but also provide valuable insights of its biomechanical origin.

Regarding the specific question of whether turning asymmetry persists when participants are afforded more time to decide, our previous work [6] provides relevant evidence. In that study, we conducted experiments where participants walked at both slow and fast speeds, with the latter condition significantly reducing the time available for making turning decisions. In both cases, we consistently observed the emergence of a CCW bias, suggesting that the phenomenon is robust across different decision-time constraints.

Minor comments:

- I suggest reducing the number of acronyms to the bare minimum as they may negatively affect readability

We have removed all acronyms referring to right-turners, left-turners, and left-dominant participants, writing them out in full throughout the manuscript. At the same time, we wanted to retain the acronyms CCW and CW, as they are standard in the field, widely used in our article, and explicitly defined in both the Introduction and Methods sections as well as in the figure captions.

- In some instances, language should be more formal (e.g. expressions like “a little bit” or similar should be changed to more formal quantification).

We sincerely appreciate the careful revision of the manuscript’s language. Following this recommendation, we have replaced every instance of ‘a little bit’ with ‘slightly’ and revised similar expressions to adopt more formal quantitative phrasing.

- Line 188 (should “patent” be “present”?) We have corrected this typo to ”pronounced”.

Thank you again for all the constructive comments and suggestions made, which have undoubtedly helped to improve the manuscript.

REVIEWER #3 (REMARKS TO THE AUTHOR):

The article presents a comprehensive investigation of an intriguing phenomenon in human locomotion: spontaneous walking counterclockwise. As an aside, I am wondering how the British publisher feels about using the word ‘counterclockwise’, instead of ‘anticlockwise’, but I leave it to the copyeditor. The contribution is novel and interesting, and I do not have reservations about the rigour of the experiments, or their analysis. I am also aware that the subject has been discussed in the collective motion community for some time, and attracted a lot of interest during conferences. Thus, I am strongly in favour of publication in Nature Communications, but I also have a few suggestions how the work could be better contextualized.

We thank the referee for the positive feedback on our work and the strong support for its publication. We are also very grateful for the suggestions made, which we think are pertinent and have helped to improve our work. Below, we provide a point-by-point answer to each of them.

1. The introduction starts by discussing lane formation. I am not sure if this is helpful for the following reasons: (a) intentional bidirectional motion is significantly different than free roaming, so I am not sure that the symmetry breaking mechanisms are related at all; (b) some definitive statements lack empirical confirmation, e.g. there is no citation to support the statement that “In most countries, lanes tend to form to the right” (line 54), or a citation to support the hypothesis that lanes form because “individuals tend to follow others who move in the same direction” (line 34). In summary, I would recommend more care in describing the literature on lane formation and relating it to the present study.

The referee is totally right. Thank you for pointing this. Our intention was merely to mention “lane formation” and the “zip effect” in bottleneck flow as examples of spontaneous collective behaviour that improve the overall flow, but we went into excessive detail.

Following the referee comment, we have reduced all explanations about the origin of lane formation (which is still under discussion) and condensed the text to just a couple of sentences describing the phenomenon.

2. Given how important the rotation parameter is to the analysis, I suggest moving equation (1) to the main body of the manuscript.

Thank you for this. We agree. We have included the definition of M and the individual polarization $m_i(t)$ (Eq. 1) in the main body of the manuscript (see text below). Besides, we keep a complete description of this parameter and its variants in the Methods section (Metrics and Nomenclature) for reference.

To quantify the directionality of rotation, we employed the polarization parameter M , defined at each time step as the average of the individual polarizations $m_i(t)$. The latter are computed as $m_i(t) = \hat{v}_i(t) \cdot \hat{e}_i^\varphi(t)$, where $\hat{v}_i(t)$ is the normalized velocity vector of pedestrian i and $\hat{e}_i^\varphi(t)$ is the azimuthal unit vector relative to a central point [18]. A complete description of this measure and its variants is provided in the *Methods*.

3. Related to this, I also have a fundamental question. The rotation is always defined with respect to the centre of the arena, but what happens if the arena gets bigger and bigger, or less and less circular? Do we expect to get one large vortex or is it possible that the crowd will break into several smaller vertices rotating around local centres? Maybe when we cannot perceive the centre, the CCW motion ceases? I understand that answering these questions would require separate experiments, beyond the scope of this study, but I think it would be important to address this matter in the discussion. In other words, the authors have chosen to measure global rotation about the centre of the arena, which is more specific than just CCW motion. Apart from global CCW roaming, pedestrians could also execute various local CCW motion that could be detected by looking at individual acceleration vectors. I was wondering if the authors could elaborate on this choice.

We appreciate this inspiring and challenging comment. Let us first note that in the boundary-free schoolyard experiments we already faced a situation with a large arena without a well-defined geometrical centre and we keep observing a robust CCW bias. But still, this is not conclusive as the students may have a strong and accurate perception of the geometrical centre.

The idea of several smaller vortices rotating around local centres sounds really interesting and should not be discarded but as the referee says, more experiments in very large spaces would be necessary and these fall beyond the scope of the present work.

As a first approach to rule out the formation of small vortices (at least in the boundary-free experiment) we examined (i) the individual trajectories of each pedestrian and (ii) the centres of mass (CM) of their trajectories with respect to the global centre of mass. Both results are presented in Supplementary Figure 2 (also included here as Fig. 3). In Fig. 3a, it is observed that pedestrians do not tend to move around local centres to form small-vortex domains; otherwise, they move around the global centre of mass. The approximately homogeneous distribution of the individual CMs (Fig. 3b) relative to the global CM further suggests that there is no tendency to cluster into local domains.

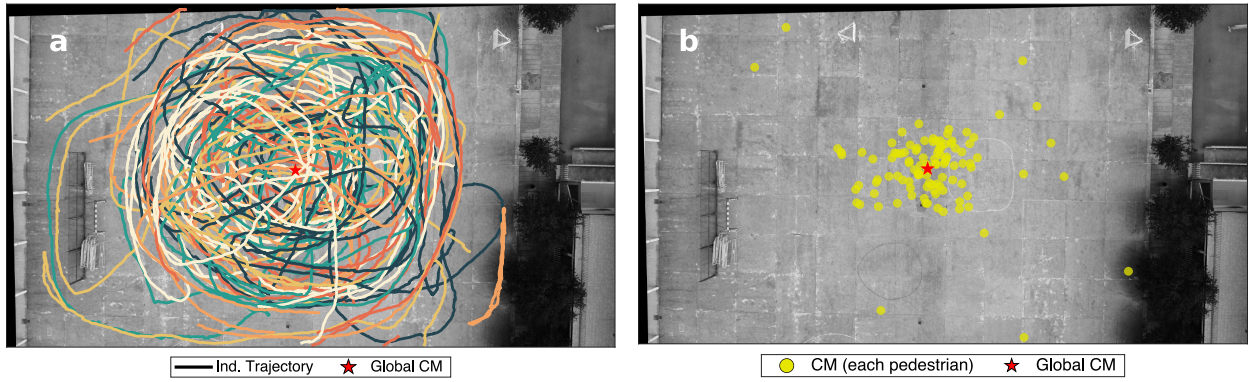


FIG. 3. Individual trajectories and centres of mass. (a) Full trajectories of half of the participants (randomly chosen). (b) Time-averaged centre of mass (CM) of each pedestrian, shown as yellow markers, computed from their individual trajectories. In both panels, the position of the global CM, calculated as the average position of all pedestrians over time, is indicated by a red star for reference.

4. For understandable reasons, the article is written for a physics-minded reader, interested in active matter and complex systems. I am a member of this community myself, and I find it very interesting. However, I imagine the article would also be of interest to biologists interested in human motor control. If I understand correctly, our best hypothesis is that the tendency to move CCW is physiological in nature. I was wondering therefore, how we could attract the attention of the biological community to this article. Nature Communication as an interdisciplinary journal is definitely a good venue. Maybe it would also be worth consulting this finding with a colleague in biomechanics? Maybe the authors could also cite some more biological literature?

Unfortunately, I cannot recommend anything myself, but I found an old article titled “Spiral movement in man” By AA Schaeffer interesting. Again, it is all going beyond the scope of this article, but I think it would be worth making sure that this article makes impact worthy of its potential.

We really appreciate this suggestion. Indeed, during the last few years, we have been asking to colleagues working with animals and other biological systems whether this breaking of symmetry could apply also to other cases. We thought specially about fish schools, due to their known rotating behaviour, but there is not definitive answer on this. In any case, aiming reaching the biological community, we have added a paragraph at the end of the manuscript where we leave this issue as an open question.

Beyond pedestrians, it seems rather plausible that CCW or CW preference motion also occurs for other animals or biological systems. Indeed, vortex-like behaviours have been reported in fish schools, which alternate between schooling and milling states [38], tadpole aggregations where circular motion enhances feeding efficiency [39], and insect collectives such as army ants, whose mills can persist for days [40]. Intriguingly, turning biases have also been documented at the individual level in some cases. For example, *Temnothorax* ants display a marked tendency to turn left while exploring [41], and flying budgerigars exhibit lateral preferences when choosing equivalent apertures during route choice [42]. Taken together, these parallels suggest that rotational coordination is a widespread motif across living systems, and that the CCW bias in human crowds may represent a manifestation of a deeper biological principle of symmetry breaking.

5. At times, the language of the article lapses into colloquialisms. Words such as 'bluish' (line 144) or 'nice' (line 264) should be avoided.

Thank you for this.

- bluish: changed to regions coded in blue.
- nice: changed to remarkable.

Thank you again for all the the constructive comments and suggestions made, which have undoubtedly helped to improve the manuscript.

REVIEWER #4 (REMARKS TO THE AUTHOR):

I read this paper with interest and I think that it is very good research. Specifically, the studies are well conducted and the analyses sophisticated and well-reported. My questions therefore focus less on the research and results themselves, and more on the assumptions underpinning the work, the reasoning behind the contributions made and the meaning of some of the findings.

We thank the referee for the interest in our work and the positive view. We are also grateful for the constructive nature of your comments, which clearly reflect a perspective that complements our own and which we consider to be highly enriching for the work. Below, we provide a detailed answer to each comment, as well as the modifications introduced in the manuscript (or Supplementary Material).

Regarding the assumptions, the paper opens by mentioning a very limited set of reasons why self-organised structures form. Also in the discussion it is said “emergent collective behaviours in pedestrian dynamics have been attributed **solely** to local interactions and social coordination”. I believe that this list is too short. Moreover, the multiple reasons that exist do not exclude one another. Birds can make beautiful swarms or murmurations because they follow very basic rules when in the air, because they are sociable and migratory animals, because they seek to mate, because dusk sets in and they look for a suitable place to roost and because they have evolutionarily encountered predators with certain properties they want to evade (e.g., able to fly in particular ways, rather than run): none of these explanations are mutually exclusive; indeed for some species all play a role. The current paper presents explanations in an either/or manner and I find this not so persuasive. And as the long list of factors involved in such phenomena in animals suggests, I believe that the entire spectrum of reasons for these phenomena for human behavior is even broader. The authors ignore factors that obviously play a role. For example, the physiology of the human body must be a factor. Because the present paper claims to have excluded the most obvious explanations of the phenomenon, it is important to demonstrate that the explanations mentioned exhaust the universe of possible/most likely explanations.

Thank you for this interesting issue. We totally agree with the referee, the sentence “emergent collective behaviours in pedestrian dynamics have been attributed **solely** to local interactions and social coordination” was not correct. We have modified it to address

the issues pointed out by the referee. Now it reads as:

Traditionally, most of the emergent collective behaviours in pedestrian dynamics have been attributed to local interactions and social behaviour (including many different factors that are not mutually exclusive).

In addition, we have toned down the wording of many of the sentences in the article to make it clear that our main discovery is that the CCW motion is an individual characteristic rather than a collective one (as demonstrated by the last series of experiments).

Regarding the first contribution, it is said. “First, we provide robust experimental evidence showing that the CCW bias is a universal characteristic of human locomotion, observable even in isolated trials. Our findings demonstrate that the symmetry-breaking phenomenon arises from innate individual predispositions, while ruling out the most obvious explanations—such as handedness, footedness, and eye dominance—thus leaving the precise origins open for further investigation.” I have a number of observations. First, there is a tension between claiming the phenomenon arises from innate predispositions and stating that its origins are unknown and require further investigation. Indeed, I read the paper as an excellent set of studies that attempted to rule out some obvious explanations. But from a series of null effects (it’s not handedness, it’s not eye dominance, etc.) I believe that one can at best (if the studies are flawless, which is always a tricky assumption) infer that it may not be these things that are causing the effect. So my impression is that the true contribution of the paper lies in ruling out some explanations, leaving us with an intriguing mystery for future research.

The referee is totally right. That sentence was misleading. What the paper demonstrates is that the CCW motion is an individual characteristic rather than a collective one. After excluding some of the first innate predispositions that came to our mind (handedness, footedness, and eye dominance) the actual origin of this intriguing motion is left for future experiments.

We have reworded the text at several places to make this idea clearer. In particular, the paragraph where the two main contributions of our work were explained has been completely reworded.

Our contribution is thus twofold. First, we provide robust experimental evidence showing that the CCW bias is a universal characteristic of human locomotion, opening new avenues for research and practical applications in areas such as urban planning and crowd

management. Second, our findings demonstrate that this phenomenon arises from individual behaviour rather than collectively emerging due to pedestrian-pedestrian or pedestrian-boundaries interactions. Then, we ruled out some of the most obvious individual symmetry breaking factors —such as handedness, footedness, and eye dominance— thus leaving the precise origins of this intriguing behaviour open for further investigation.

Regarding the second contribution, it is said: “Our findings suggest that nontrivial group behaviors can originate from basic individual locomotor predispositions”. Although this claim is slightly contradicted by the point above (a series of null effects can not logically prove that the effect is due to basic individual locomotor predispositions) I am also wondering how this is new. Maybe I am missing the point, but is this not what researchers in pedestrian dynamics have shown many times over, and is this not also what biologists have shown when modelling animal collective behaviour?

The referee is totally right. We wanted to explain that the “group behaviour” was not really a group behaviour but just the sum of individual behaviours. We really apologize for the sentence, which was really confusing. As said before, the paragraph has been reworded to clearly state the twofold contribution of our work.

Finally, I believe that the study showing these are individual tendencies is very informative. If the study were presented first, then all other studies are as expected. So what this leads me to suggest is that the contribution of the paper could also be described as showing that whilst some have thought the CCW preference was a collective phenomenon, it is not.

Exactly, the referee explained it perfectly. This is what we tried to convey, but it was not obvious due to our lack of clarity. In the present version of the manuscript, we have improved this.

A final point is that throughout the paper, I was trying to find indicators of the magnitude of effects and for the magnitude of variability. What does it mean that $M \sim 0.2$? And what are the implications of the fact that M is not 1.0, for the use of the word “universal” in this paper?

Thank you for this. The implications of $M \sim 0.2$ mean that not all the pedestrians walk in CCW circular motion all the time. Indeed, some of them walked CW and others followed more straight paths. But in all the cases studied, the proportion of CCW walkers was higher than CW ones. About the origin of $M \sim 0.2$ (and the shape of the PDF of M getting narrower as the number of pedestrians grows), this is now justified in the last

paragraph before the Discussion section.

Going a step further, we try to connect the individual motion results with the values of collective polarization (M mostly around 0.2 in Fig.1) as well as their distributions, where we observed a clear narrowing of the peak with the crowd size (Figs. 2 and 4). To this end, in Fig. 8d we synthetically build the PDFs of M by taking the actual values of m from the individual pedestrians moving alone (as shown in Fig. 8b) in a random manner....

Specific questions/comments. You write “his task allowed us to categorize participants as left-turners (LT) or right-turners (RT) based on the direction of their turn.” There seems to be an assumption that the execution of a single task is sufficient to characterize individuals as LT or RT. What is the basis of this assumption? What is the reliability of the test? What does that mean for the subsequent interpretation of the results, which hinge on the assumption that the groups are composed of 100% RT, etc.?

Thank you very much for this thoughtful comment. We agree that a single turn may seem limited to robustly characterize an individual’s turning preference. Indeed, in our first experiment in Spain we could only administer the turning test once for time reasons. Being aware of this limitation, in the Japanese campaign we explicitly introduced an improvement by repeating the test three times at different moments during the two-hour session (see Methods, Confined Random Motion in Japan in the main text, and Supplementary Table 2). These tests were rather conclusive, as the consistency of participants’ turning direction was very high (above 85% of individuals turned in the same direction all three times). The text has been modified to explain this in a clearer manner.

I think the study with children rests on an unfounded assumption that culture only begins to affect human beings when they enter primary school. The very fact that children start using language from age 1, shows otherwise. I also note that the source to corroborate this claim is a book about the development of morality, not about the acquisition of social motor skills.

We fully agree with the referee that it would be misguided to assume that children are not culturally influenced. In fact, we share the view that such influence is present almost from birth. However, our decision to include experiments with children was motivated by two main considerations: (i) children are likely to be less exposed to acquired adult conventions (e.g., signage norms or learned circulation habits), and (ii) in certain locomotor contexts, previous research has shown that their dynamic responses differ from those of adults [8] (as

also noted by Referee 2, Q. IV). On this basis, our aim was to explore a new scenario in which, in principle, children might behave differently from adults, with the expectation that this could lead to results distinct from those obtained in our previous adult-only experiments. Surprisingly, this was not the case: not only did the counterclockwise bias reappear, but it was even amplified. This outcome aligns with prior findings showing that young children are highly sensitive to peer consensus, prone to overimitation, and especially inclined to align with others in rhythmic or musical group activities [9–12].

To better clarify the reasons why we choose children to test the CCW bias, we have rewritten the corresponding paragraph in the main text as follows:

To explore this option, we analysed experiments conducted in a nursery school, as previous research has demonstrated that young children differ from adults in their emergent movement patterns [23], and it can reasonably be assumed that they are less influenced by acquired adult conventions (e.g. signage norms or circulation habits).

Thank you again for all the constructive comments and suggestions made, which have undoubtedly helped to improve the manuscript.

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REVIEWER #1 (REMARKS TO THE AUTHOR):

The manuscript is excellent and I have no further major concerns.

We thank the Reviewer for their recommendation to publish our work in Nature Communications.

I have only one minor suggestion for the authors to consider regarding the title. The term "Universal" is a very strong claim. While the main text and discussion do an excellent job of rigorously defining the scope and boundary conditions of this preference, the title itself might give readers an overly absolute first impression and could attract unnecessary debate. You might consider a slightly more nuanced term to pre-empt potential debate. For example, "An Intrinsic..." or "A Widespread Counterclockwise Bias..." could also capture the significance of your findings effectively. This is merely a suggestion for your final consideration. Congratulations on an outstanding study.

We appreciate the referee's thoughtful observation regarding the use of the term "Universal." We fully agree that this wording could convey an unnecessarily absolute interpretation of our results. Accordingly, we have revised the title and all related expressions throughout the manuscript to reflect a more precise and balanced description of our findings.

1. **Title:** Changed from "*Universal Counterclockwise Bias in Roaming Pedestrians*" to "*Individual bias drives counterclockwise motion in pedestrian crowds*".
2. **Abstract (l. 23–25):** Replaced "is universal" with "is a robust and reproducible feature".
3. **Introduction (l. 77–79):** "the CCW bias is a universal characteristic of human locomotion" → "the CCW bias is robust cross-cultural feature of human locomotion".
4. **Individual Behaviour (l. 236):** "demonstrated the universality of the CCW rotation effect" → "demonstrated the consistency and robustness of the CCW rotation effect".
5. **Discussion (l. 349–353):** "demonstrate the universality of CCW motion" → "demonstrate the robust and widespread occurrence of CCW motion".
6. **Discussion (l. 375–377):** "the notion of universality should not be interpreted as an absolute law" → "the generality of this behaviour should not be construed as evidence of a universal law".

REVIEWER #2 (REMARKS TO THE AUTHOR):

I thank the authors for addressing my comments, I think the manuscript has improved and is now ready for publication.

We thank the Reviewer for their final assessment. We are glad that all comments from the previous round have been addressed.

REVIEWER #3 (REMARKS TO THE AUTHOR):

The authors have addressed all of my comments professionally and accurately. I am happy to recommend the article for publication.

We thank the Reviewer for acknowledging the interest and quality of our paper.

REVIEWER #4 (REMARKS TO THE AUTHOR):

I thank the authors for their responsiveness and the revisions made. While I continue to support the publication of this paper, I have one remaining, fundamental objection regarding the claim of "universality" that must be addressed.

We are very pleased that the referee continues to support the publication of our paper and appreciates the revisions made in the previous round.

1. Core Issue: The Misleading Claim of "Universality"

My primary concern is the repeated claim that the research "demonstrates" a "universal" CCW or CW bias. This terminology is both empirically unsupported and conceptually misleading, a point also raised by other reviewers.

The authors have added a paragraph to clarify their use of "universal," but this clarification to me confirms the term's inappropriateness. They state that universality "should not be interpreted as an absolute law" and that the bias only emerges "when competing influences are minimal." This directly contradicts the standard meaning of the word and the strong inference it creates for readers.

I am concerned this is not the right term to use on three levels: (i) Empirical: The data itself argues against a strong universality claim. With an effect size of $M \sim 0.2$,

a significant portion of participants do not exhibit a CCW bias. A universal trait (e.g., having lungs) would show a near-perfect distribution, which is not the case here. The data supports a statistical tendency, not a universal law. (ii) Logical: It is logically impossible to prove universality across all human contexts based on only five studies. At best, the findings suggest a "robust tendency observed across the limited cultures and contexts tested so far." Claiming universality based on this sample is an overreach. (iii) Terminological: The word "bias" is used ambiguously throughout the paper, referring to individual, between-individual, and collective levels. To claim "universal counterclockwise bias" without specifying which level is meant adds significant confusion.

Suggested solution: I advise to replace "universal" with more precise and defensible language throughout the manuscript, particularly in the abstract and conclusions. If it were about handedness, one would not write "demonstrating that handedness bias is universal" but simply "there are more right-handers". In the same way, I would advise a rephrasing that is more straightforward, close to the evidence at hand and understandable without a dictionary, such as: "the CCW bias occurs because individuals make CCW turns more often". If authors want to write something about how it could be universal across other cultures and settings than the 5 settings now studied, they will have to use more speculative language (e.g., "this could be").

We appreciate the referee's thoughtful and constructive remarks. As also noted by Reviewer 1, we fully agree that the term universality could be misleading in this context. Following this advice, we have removed all mentions and claims referring to "universality" throughout the manuscript, including the title, abstract, and discussion. The revised text now describes our findings as a robust and recurrent counterclockwise tendency observed across diverse conditions and cultural contexts, avoiding any implication of an absolute or universal law. We believe this modification addresses the referee's concern and ensures that the terminology is now fully consistent with the scope and evidence of our study.

2. Specific points from previous reviews not addressed (related to point 1)

On Effect Size ($M \sim 0.2$): My original question was about the implications of $M < 1.0$ for the universality claim. The authors' response explained what $M \sim 0.2$ means

statistically but did not engage with its conceptual implication: that the data show a deviation from universality, not a confirmation of it.

In our previous response, we explained that a value of $M \sim 0.2$ indicates a statistically significant preference toward the CCW direction, although not all individuals exhibit this tendency. In this regard, the referee is entirely correct in noting that such a value of M does not support a claim of universality in the strict sense. Therefore, as mentioned above, we have removed all references to “universality” from the manuscript.

On the “Series of Null Effects”: The revised text now correctly explains the findings as a series of consistent results. However, consistency across five studies is evidence of robustness, not universality. The leap to “universality” remains unjustified.

We thank the referee for reiterating this important point. As already addressed in our previous responses, we fully agree that our findings reflect a robust rather than universal tendency. As said, all references to “universality” have been removed, hoping that this revised wording more accurately conveys the empirical scope of our results and fully resolves the referee’s concern.

3. Minor Point: strengthening the “Social Norm Elicitation” section

The new paragraph on social norms (page 5) is a step in the right direction, but can be improved.

Citation of Latané & Darley: While placing the work in a social science context is valuable, the citation to Latané and Darley’s bystander intervention work is tangential, as it does not directly address movement choices. Furthermore, the bystander effect itself has been nuanced by recent meta-analyses (e.g., Fischer et al., 2011; Levine & Manning, 2015). I recommend citing literature more directly relevant to movement and social norms, such as the work of Anna Sieben (on shared cultural understandings of the situation, social conventions and emergent dynamics in crowds) and Anne Templeton (on shared identity and movement).

Thank you for the careful read and for the constructive suggestions to improve the part of our manuscript related with social psychology. We are familiar with the works by Anna Sieben and Anne Templeton and we agree their recent research has direct implications to the phenomena we study or at least they deserve being cited while

discussing the results. From the works of both authors, we selected a few papers that help to elucidate how social norms arise even in the (apparent) absence of already existing norms [1–3]. There are probably more works to be cited by Sieben, Templeton, and colleagues, but we selected the newest and the ones we are more familiar with (and, of course, only those who are related to our study). We also added one work by John Drury which, although dealing with emergency situations, provide an exhaustive review on the processes leading to the emergence of shared identities in psychological crowds [4]. As a side note, we should also mention that the work by Latané & Darley was suggested by one of the reviewers, and, after examining the work, we believed it would be appropriate to cite it. After comparing the work by Latané & Darley with those by Sieben and Templeton, we understood the comment of the reviewer on the “tangential implication” and therefore decided to keep only the ones by Sieben and Templeton which are more related and also more recent in time.

In pedestrian dynamics, it is known that social influence is behind the observed behaviour in many different instances (for examples on the social influence or the emergence of social norms, see [1–3] for non-emergencies and [4] for emergency scenarios).

Now, from the perspective of that literature, the new paragraph (lines 223-234) can be improved further. I believe that what the authors are saying (when they use the word “intuition”, which I would avoid because it brings in yet another literature) fits with a distinction between different kinds of norms. I think that what the authors are trying to say here is better explained through the lens of explicit vs. implicit norms. Explicit norms: known, codified rules (e.g., “drive on the right”). The questionnaire correctly shows no evidence for an explicit norm governing turning direction. Implicit norms or conventions: automated, situation-specific behavioral conventions people may not be consciously aware of (e.g., subtle traffic interactions). The data suggests that if an implicit norm for CCW turning exists, participants are not conscious of it.

Thank you again for the careful read and for your guidance into social psychology. We believe indeed that the CCW norm is implicit. People are probably not aware of moving CCW, although we do not have that evidence since we never asked the participants of our experiments in which direction they walked. However, in the experiment in Japan some informal discussion was performed with some participants after the

experiments and people did not realize the reason why they were assigned a different cap colour (red caps were used for right turners after the test when walking against the wall). The paragraph mentioned by the reviewer has been now revised using the explicit vs. implicit framework. We believe the revised text is indeed clearer compared to the previous one and we would like therefore to thank again the reviewer for the constructive guidance.

The above result has interesting cognitive implications. In most social interaction contexts, people follow explicit and implicit norms that tell them how to behave in those situations, typically coming from moral, ethical, legal regulations, or conventions or cultural understandings. Explicit norms are a direct result of codified rules (e.g., laws or regulations), whereas implicit norms are situation-specific and act without conscious awareness. The norm in place emerges through interactions between people and the environment, potentially resulting in a situation where there is a contrast between the norm eventually adopted by people and what expected through explicit rules. Our experiment shows that turning direction is likely not determined by explicit rules, as confirmed through the results from the questionnaire contrasting what expected from codified motion rules. One could then conclude that the CCW turning norm (if there is any) is more likely implicit, consequently acting with participants not being aware of it. However, although the explicit vs. implicit framework allows to examine the observed CCW turning behaviour from a more systematic perspective, the mechanisms leading to this specific norm (or "behavioural repertoire", to use an alternative expression), are still unclear and would deserve a in-depth investigation.

This framework provides a clearer, more theoretical grounding for the authors' findings on participant awareness. It ties the current findings to Sieben's and Templeton's way of thinking (and their focus not on explicit norms, but on conventions and implicit norms). It also avoids making a complex and I think problematic distinction between personal and social norms (the personal norms are social too, if they stem from social injunctions or law? The social norms remain a bit hazy for me). In sum, for reasons my advice: stick more closely to the work that is directly relevant to pedestrian dynamics.

We agree with the reviewer that it is not easy to distinguish between social and personal norms, and that, indeed, explaining the process through implicit vs. explicit norms

would make the explanation easier and it would better fit with the latest research on this topic. As already mentioned, we modified the text to use the explicit vs. implicit framework, while also using some terms derived from the latest research by Sieben (e.g. behavioural repertoire), which we believe also concern our research. We hope the latest revision is not only correct from the standpoint of pedestrian dynamics but also that of social psychology.

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