

Current-Driven Molecular Machine with Rotation-Enhanced Conductance

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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:

Referee #1

(Remarks to the Author)

The manuscript reports experimental evidence of rotary motion of helical molecules in molecular break junctions, driven by an electric current through the molecule. The key evidence for the rotational motion is the observation of cyclical visits of three different conductance states. This is observed for about a third of the studied molecules, whereas for the remaining portion, less than three conductance states are observed. For the molecules exhibiting cyclical behavior (ABC), the direction reverses upon voltage reversal (ACB). Above a critical voltage V_{onset} , a drastic increase of overall conductance is observed. Rotation is not observed for non-helical molecules.

The above experimental observations are clearly documented and are interesting and intriguing. The authors offer also relevant theoretical calculations that help understand and interpret the observed conductance data. However, while the notion of torque generation by an applied voltage or current through the molecule is central to the observed effect and its interpretation, it is not discussed in any detail. It is unclear to this reviewer whether the authors believe that that torque generation is obvious and trivial (the authors write about the idea of “electric wind” blowing against “turbine blades”) or whether the mechanism is unresolved. This is a major shortcoming that must be addressed before publication can be considered.

Below, I discuss in more detail the theory parts and interpretations, some of which make sense to me, while others cause various degrees of discomfort in me. I address these in order, beginning with the parts I feel most comfortable with, then moving to the ones where I have questions or objections.

1) On top of page 8 and in Fig. 2a,b, a model based on an assumed tilted energy landscape is described that makes sense to me. Based on an assumption of thermally activated motion/current, values for energy barriers and landscape tilt are extracted from the experimental data.

a. As the model depends on kT , and the molecules effective temperature is not precisely known, I suggest to give the parameters primarily in units of kT . Then, they can also be expressed in eV based on assumptions on T .

b. Gamma is a measure of the tilt of the energy landscape as a function of voltage. I appreciate that it is defined as such and should be discussed in those terms. (Note: gamma is a voltage response parameter; it is not clear to this reviewer how to reconcile this with the language of “turbine blade effect” in response to an “electron wind” and “electron momentum transfer”; see also point 6d below)

2) I also like the model underlying Fig. 4a-d. This is a clear, objective presentation of the results of DFT calculations and makes sense to me. The calculated conductance depends on the molecular orientation. Based on the resulting conductance landscapes one can calculate currents. The language is scientific and objective.

It is, however, not clear to me what is plotted in Fig. 4e. How is “torque flow” defined and calculated? What is the assumed mechanism for how an applied voltage results in mechanical torque generation and how is this represented in DFT calculations?

3) Similarly, it is unclear to me what is plotted in Fig. 2f. Is the “torque transfer/rotation barrier” meant to be (γ/E_b) as defined on page 8? Note that the units don’t match. Gamma appears to be dimensionless while a torque should have units

of Nm. Also the numbers don't seem to match. The numerical γ / E_b -ratio from the model on page 8 is more something like 1/10 (units of 1/meV?).

4) The authors claim a qualitative difference between "stochastic" behavior for $V < V_{\text{onset}}$ versus different behavior for $V > V_{\text{onset}}$.

a. The authors also imply that all stochasticity disappears above V_{onset} . What is the experimental evidence for this assertion? Can the authors quantify stochasticity (e.g. using a rotational analogue of the randomness parameter sometimes used to describe linear molecular motors) and show a transition between the two regimes? If yes, this could include useful and interesting quantitative information. If no, the contrasting terminology should be clarified or removed.

b. Carefully check the language used to describe the behaviors. A sentence like "stochastically jumps between three different values in a well-ordered cyclical manner" raises the question: is it stochastic or well-ordered? The authors also use the term "periodic jumps" to describe the same phenomenon. Periodic suggests non-stochastic.

5) Statements about directionality.

a. The authors use the terms "100% preferred directionality" and "100% unidirectional". Please choose one term, give a strict definition, and show how the analysis was done.

b. On page 4 it also becomes clear that the "100%" term only applies to a fraction (122 out of 316) of the observed molecules.

i. What is the behavior of the other ones?

ii. Expressing 122/316 as "100%" is like having 122 green ones and 194 of undefined color and saying; "100% are green."

iii. For any type of molecular process, there should always be some level of mis- or backstepping. Truly 100% directionality would imply diverging energy dissipation (diverging entropy generation) during rotation. Please comment on the 100%-assessment from a stochastic thermodynamics perspective, and/or moderate the claim.

6) While the paper doesn't discuss the details of the imagined molecular torque mechanism, the authors generously use physical terms that normally are associated with the macroscopic world. This obscures the physics and risks creating confusion. For example:

a. "injected electrons transfer momentum" (p 4) to me implies a role for inertia at a molecular scale. This requires substantiation. Momentum transfer from whom to what and how? Is it momentum in the macroscopic (high Re) sense? It would be far more reasonable to me (and more in line with the model in Fig. 2) to discuss the physics in terms of biased diffusion.

b. Reverse of current direction is not proof of momentum transfer, it just means that linear (or uneven) response relations are observed.

c. I object to the terminology of "current booster", "current boosting effect" and "current amplification". These are imprecise terms and they suggest active amplification processes, which is misleading and physically incorrect. The established and correct term is "nonlinear conductance".

d. Macroscopic terminology such as "molecular windmills" and "electric winds" are imprecise at best and contribute to confusion rather than clarity. I have no idea what the authors mean by "electric wind". Electrons flying through vacuum? How would this be reconciled with the model in Fig. 4?

Other points:

7) It is not clear to me whether there is experimental evidence of rotation for $V > V_{\text{onset}}$, or whether rotation in this regime is an assumption?

8) At high currents, the authors observe evidence that the molecule is expelled and a metal contact is formed. This should be said clearer early on in the paper and the corresponding regime marked in all relevant figures. Since the authors tie this transition to $G \approx G_0$, I suggest to show a G-axis in addition to current in all relevant figures

Referee #2

(Remarks to the Author)

This paper presents a combined experimental and theoretical study examining the behaviour of a long helicene molecule placed between two electrodes in a break-junction configuration under applied bias. Specifically, the authors demonstrate that directional molecular motion is induced, which in turn drives a substantial enhancement in current. This is a fascinating observation that appears to support earlier theoretical predictions. Overall, the data seems robust and supports the interpretation proposed by the authors, at least in my opinion. I believe this study will be of broad interest, not only to researchers working on chirality and its role in molecular motion and electron transport, but also more generally to the molecular machines community.

I approached this paper primarily from the perspective of an experimentalist with expertise in chiral materials and their optical and electronic properties. However, I am not an expert in break-junction techniques or in the theory underpinning this study. As such, many of my comments and questions arise from scientific curiosity rather than deep technical familiarity.

The directionality of the molecular motion is inferred from the observation of three or more different current levels attributed to distinct rotational states (e.g. Figures 1d–i). Owing to the timescale shown, these three states are particularly clear in Figures 1d and 1g, but become progressively less distinct at higher applied voltages because of the increased rotational frequency. Could the authors confirm that, at 660 mV in particular — which lies beyond the threshold voltage associated with the

observed current enhancement — the three states remain clearly resolved? A zoomed-in trace over a shorter time interval might help to clarify this point.

I also noted that the authors report identifying only 122 out of 316 molecular junctions that exhibited three or more distinct current states. I was therefore curious about the behaviour observed in the remaining cases where fewer than three states are present. For example, could these correspond to different molecular junction configurations, and if so, might the later DFT calculations provide insight into these alternative behaviours? Alternatively, is there another origin for the observations in junctions that do not exhibit more than three states?

Finally, I had a question regarding the design of the helicene itself. The molecule employed in this study clearly derives from the synthetic chemistry developed by Michael Pittelkow's group and appears to perform very effectively in this context. However, the structure contains several notable features, including the two helical turns as well as the various heteroatoms and side chains introduced through the synthetic methodology. It was not entirely clear to me from the modelling section how these specific molecular features influence the measured properties. For example, would a fully carbon-based helicene exhibit similar behaviour, or would the effects differ significantly? If the authors are able to comment further on the role of these structural features this would strengthen the discussion.

Referee #3

(Remarks to the Author)

The manuscript presents experimental and theoretical results on a helicene molecular junction. At fixed bias, the current shows telegraph noise with three levels (A,B,C). A sequence A-B-C-A... occurs, as it is claimed, 100% times, unlike the odd one A-C-B-A-... This observation is linked to previous studies of molecules on surfaces, where such current dynamics implied current-induced rotation. In the present manuscript, the detailed analysis justifies the assumption of a rotation.

The theoretical analysis supports the claim by providing a potential energy surface from ab-initio. The focus is on explaining the extreme current blowup at a certain voltage. A Born-Oppenheimer-type mechanism with a resonance moving close to the Fermi level is consistent with the experimental observation. Other mechanisms are studied, namely, a pumping beyond Born-Oppenheimer. This analysis could deserve a separate publication.

The manuscript presents a significant advance in the field of electrically-powered molecular motors and I recommend its publication.

I have only minor remarks that the authors could take into account upon revision:

A.) The buzzword IETS (inelastic electron-tunneling spectroscopy) is introduced in the main text and Fig. 3. It suggests that a new measurement technique is employed. However, the authors just measure the electric current, as in Fig. 1. The subsequent analysis mentions a zero-bias conductance, which is not inelastic. A possible inelastic effect is discussed only superficially. The use of IETS as a 'spectroscopy' could be reconsidered.

B.) The work would benefit greatly from strengthening the role of structural chirality. What happens if the molecule is replaced by its mirror partner? Is it experimentally feasible? On the theoretical side, such discussion would involve the change of a few atomic coordinates or model parameters.

Version 1:

Reviewer comments:

Referee #1

(Remarks to the Author)

The authors provided a very thorough and thoughtful response and made significant updates, mainly to the SI but also to Methods section 4.4, which is very helpful. Thanks for this. Between the main text, the authors' response and the updated SI, I now feel comfortable with the experiment and the with the theoretical treatment.

Unfortunately, however, the changes to the main text (except the substantial update in Methods 4.4) are mostly cosmetic and do not fully reflect the substance of the responses given by the authors to the referees.

Key points that remain to be addressed in the main text:

1) My point about using macroscopic language was not so much about quantum versus classical, but about human-scale, macroscopic language (wind, turbine blades) versus molecular. The authors argue that this language is established in the field. In the main text they refer to Refs. 15 and 21-27. Checking the references does not back up this claim. For example:

a. Ref. 15 exclusively uses rigorous, scientific language such as "angular momentum transfer" - no "wind".

b. Ref. 22 uses the word "windmill" in the title, but the language in abstract and main text is rigorous and specific, for example: "The proposed force arises when a dc voltage is applied between the external electrodes, which produces a "wind" of electrons, for example, from left to right. The incident electron flux (from the achiral CNT) possesses zero angular

momentum, whereas after interacting with the chiral nanotube, the outgoing current carries a finite angular momentum. By Newton's third law, this flux of angular momentum produces a tangential force (and an associated torque) on the inner nanotube, causing it to rotate."

The everyday term "wind" is here used for illustration and not instead of a physical explanation, but as a complement.

d. In their response, the authors also refer to Refs. 3-5 of their response. However, Ref. 3 doesn't use the word wind a single time. Refs. 4 and 5 each use "electron wind" once, but in quotation marks when referring to other works. All three papers describe the actual physics using well-defined scientific terms, as one would expect.

In contrast, the main text of this work stands out in using human-scale language *instead of* a precise scientific explanation. The latter is, unfortunately, found mainly in the SI and in the response to reviewers.

2) I continue to object to the term "current booster". I maintain that the established physical terms should be used wherever possible. For example, "current boosting capabilities" is simply the property of a highly nonlinear conductance. Instead of "mechanical current booster" one can write mechanically induced, (highly) nonlinear conductance.

One reason for avoiding the booster-language is to not introduce new words to describe established concepts, which leads to confusion. An even more important reason is that "booster" and "amplification" suggest active, dissipating devices such as in a transistor-based current amplifier. This is misleading.

It may indeed be appropriate to coin a new word for the specific mechanism behind the nonlinear conductance observed in rotating molecules. As an inspiration, think of resonant tunneling. That is also a nonlinear conductance mechanism, with its own specific, descriptive name. In contrast, "booster" is neither specific nor descriptive.

3) The second paragraph of the introduction contrasts a "Brownian ratchet mechanism" against "electron-wind induced rotation", suggesting they are two different things. The authors' response to my comments makes clear that this contrast doesn't exist: the observed effects can very well be described in terms of diffusion or biased motion in a tilted potential. I liked the authors' text in the response:

"In general, for a tilted energy $U(x) = -Fx$, the slope F is the force acting on the particle. In our case, the energy is a function of a rotation angle θ , and in that case, the slope is a torque. The torque that gives rise to the tilt is the torque arising from the transfer of electron (angular) momentum to the molecule."

This is a nice, concise description of the physics and also highlights that there is no contrast between a ratchet-type energy landscape and the physics discussed here. My strong recommendation is to adjust the introduction to remove the impression of a mystery and to add the simple physical description above in the main text.

Minor points:

- New equation (caption of Fig. 1) uses a very informal notation. I do understand what is meant by #jumps but this is not how equations should be written.
- Typo summerised -> summarized

Heiner Linke

Referee #2

(Remarks to the Author)

Referee #3

(Remarks to the Author)

The authors responded to my report satisfactorily and I recommend publication.

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Reply to the Referees - Nature manuscript 2026-02-05351A

We are grateful to the referees for their generally positive assessment and for their valuable comments, which have substantially improved the quality and clarity of our manuscript. In particular, we have addressed the questions regarding the physical interpretation of our results by revising the main text, expanding the Methods section, and adding new data to the Supplementary Information. We have also refined the terminology throughout the manuscript to ensure a more accurate and precise presentation. Below, we provide a detailed, point-by-point response to each referee's comment.

The referees' comments are reproduced below in black. Our responses are shown in blue. In the revised manuscript and the Supplementary Information, revisions are also highlighted in blue.

Referee 1:

General Comment

The manuscript reports experimental evidence of rotary motion of helical molecules in molecular break junctions, driven by an electric current through the molecule. The key evidence for the rotational motion is the observation of cyclical visits of three different conductance states. This is observed for about a third of the studied molecules, whereas for the remaining portion, fewer than three conductance states are observed. For the molecules exhibiting cyclical behavior (ABC), the direction reverses upon voltage reversal (ACB). Above a critical voltage V_{onset} , a drastic increase in overall conductance is observed. Rotation is not observed for non-helical molecules.

The above experimental observations are clearly documented and are interesting and intriguing. The authors also offer relevant theoretical calculations that help understand and interpret the observed conductance data. However, while the notion of torque generation by an applied voltage or current through the molecule is central to the observed effect and its interpretation, it is not discussed in any detail. It is unclear to this reviewer whether the authors believe that torque generation is obvious and trivial (the authors write about the idea of "electric wind" blowing against "turbine blades") or whether the mechanism is unresolved. This is a major shortcoming that must be addressed before publication can be considered.

Author's Response: We thank the referee for the positive assessment of our work and appreciate the referee's comments, which have helped improve the manuscript. The general theory of current-induced forces and current-induced motion in nanoscale objects has been developed over the past two decades, establishing concepts such as "electron wind", "current-induced torque", "atomic-scale waterwheels", and nanoscale "electron-turbines" [1–8]. Electron-wind forces in conductors were studied even earlier, dating back to the 1970's [9–11]. Here, we build on these works, introduce the tilted washboard model in the context of molecular machines for the first time, and discuss its implications for a realistic physical system. To avoid highly technical discussions in the main text and to meet the length restrictions for *Nature* articles, we described the theory of current-induced forces in detail in the first version of our work primarily in the Supplementary Information. We have now better integrated this information by revising the main text, and in particular, we have added Methods Section 4.4, which summarizes the tilted washboard model of current-induced forces, to address the referee's comments and clarify the terminology, as discussed further below.

Below, I discuss in more detail the theory parts and interpretations, some of which make sense to me, while others cause various degrees of discomfort. I address these in order, beginning with the parts I feel most comfortable with, then moving to the ones where I have questions or objections.

1. On top of page 8 and in Fig. 2a,b, a model based on an assumed tilted energy landscape is described that makes sense to me. Based on an assumption of thermally activated motion/current, values for energy barriers and landscape tilt are extracted from the experimental data.
 - (a) As the model depends on kT , and the molecule's effective temperature is not precisely known, I suggest giving the parameters primarily in units of kT . They can also be expressed in eV based on assumptions on T .

Author's Response: We now provide the relevant values in units of $k_B T$ as well, both on page 8 and in Supplementary Table 2 (Supplementary Table 1 in the previous version).

- (b) γ is a measure of the tilt of the energy landscape as a function of voltage. I appreciate that it is defined as such and should be discussed in those terms. (Note: γ is a voltage response parameter; it is not clear to this reviewer how to reconcile this with the language of "turbine blade effect" in response to an "electron wind" and "electron momentum transfer"; see also point 6d below.)

Author's Response: Indeed, $e\gamma V$, is related to the tilt of the energy landscape, as rightly pointed out by the referee. In general, for a tilted energy $U(x) = -Fx$, the slope F is the force acting on the particle. In our case, the energy is a function of a rotation angle θ , and in that case, the slope is a torque. The torque that gives rise to the tilt is the torque arising from the transfer of electron (angular) momentum to the molecule. It is also intuitive to expect that the momentum transfer increases with the number of electrons traversing the molecule, i.e., with increasing bias voltage, V . We make this connection in several places in the manuscript: in Figure 2a and 2b, in the new Method section 4.4, and in much detail in the revised Supplementary Section 4.

2. I also like the model underlying Fig. 4a-d. This is a clear, objective presentation of the results of DFT calculations and makes sense to me. The calculated conductance depends on the molecular orientation. Based on the resulting conductance landscapes one can calculate currents. The language is scientific and objective. However, it is not clear to me what is plotted in Fig. 4e. How is "torque flow" defined and calculated? What is the assumed mechanism for how an applied voltage results in mechanical torque generation and how is this represented in DFT calculations?

Author's Response: We appreciate the referee's positive comments regarding the model presented in Fig. 4a-d. The current flow, exerts a torque on the molecule, causing it to rotate. This torque is non-conservative and can transfer energy into the molecule's rotational motion. The microscopic theory and the corresponding expressions for the torque are presented in Supplementary Section 5, and are now also summarized in Methods Section 4.4. The "torque flow" vector $\vec{\tau}_{LR}$ (note that this vector has components that describes how the molecular orbitals are coupled to the gold electrodes) is introduced to describe the work done by the incoming electrons on the molecular rotor during one rotation cycle by an area integral enclosed by the path in $\vec{t}_{LR} = (t_L, t_R)$ space,

$$W_\tau = \int_0^{2\pi} d\theta \tau_w(\theta) = \oint d\vec{t}_{LR} \cdot \vec{\tau}_{LR} = \int d^2 t_{LR} \mathbf{n} \cdot (\nabla \times \vec{\tau}_{LR}),$$

as detailed in Supplementary Section 6.2.2. In the linear approximation, this work can be estimated from the experimental measurement of γ using $W_\tau \simeq 6e\gamma V$ (see Supplementary Section 2.4.1).

In response to the referee comment, we have extended the Methods section in the main text to give a general overview of the theory, where we show how the torque is calculated [Eq. (2)] and how the torque-flow vector is defined [Eq. (3)]. Physically, the torque transfer originates from the scattering of traveling electrons by molecular nuclei, as discussed in detail before and after Eq. (2)

in the revised manuscript.

3. Similarly, it is unclear to me what is plotted in Fig. 2f. Is the “torque transfer/rotation barrier” meant to be (γ/E_b) as defined on page 8? Note that the units do not match. γ appears to be dimensionless while a torque should have units of N m. Also, the numbers do not seem to match. The numerical γ/E_b ratio from the model on page 8 is more something like 1/10 (units of 1/meV?).

Author’s Response: We thank the referee for the comment. The plot shows the work done by the torque over one rotation period, W_τ , at different interelectrode displacements and voltages, divided by the rotational barrier height, E_b . Both quantities are measured in units of eV, so the ratio is dimensionless. Supplementary Sections 3.1 and 3.3 contain further details on the computation of both quantities.

In response, we have modified the label of the y-axis of Fig. 2f and added an improved explanation in the caption.

4. The authors claim a qualitative difference between “stochastic” behavior for $V < V_{\text{onset}}$ versus different behavior for $V > V_{\text{onset}}$.
 - (a) The authors also imply that all stochasticity disappears above V_{onset} . What is the experimental evidence for this assertion? Can the authors quantify stochasticity (e.g., using a rotational analogue of the randomness parameter sometimes used to describe linear molecular motors) and show a transition between the two regimes? If yes, this could include useful and interesting quantitative information. If no, the contrasting terminology should be clarified or removed.

Author’s Response: We do not wish to claim that stochasticity disappears above V_{onset} . We therefore corrected the main text and the Supplementary Information accordingly. Within the tilted washboard model, at sufficiently low voltage and finite temperature, the molecular rotor jumps stochastically over barriers separating energetic minima, with dwell times between these events. This is experimentally evidenced by the exponentially decaying dwell-time distributions shown in the new Supplementary Fig. S9, which is characteristic of a Poisson process (see Supplementary Section 2.3.1 for dwell-time analysis). Above a threshold voltage, the model exhibits a transition to continuous rotation, in which stochasticity plays a minor role, while the system dynamics are primarily governed by the current-induced torque and friction. Experimentally, in this regime, we cannot assess the presence or absence of stochasticity because the expected molecular rotation frequency lies in the 10 - 100 GHz range (Supplementary Section 6.2.5 and Fig. S34). Given the characteristics of the signal and the studied system, the available electronics do not provide the temporal resolution required to detect these dynamics.

We thank the referee for drawing attention to the randomness parameter. Indeed, it is a useful concept also in the present context. The randomness parameter is defined as the ratio of the diffusion coefficient, D , to the drift velocity, v , times the motor step size, d :

$$r = \frac{2D}{vd}.$$

Functionally, it serves as the Fano factor for the motor’s stepping process, allowing one to infer the underlying kinetic topology of the motor without directly observing its intermediate states. This concept can be straightforwardly extended to the rotational motor studied here by using the unwrapped angle θ , rather than the reduced modulo 2π angle, as the input instead of the linear displacement. The step size is then $d = 2\pi$ and the randomness parameter becomes

$$r = \frac{D}{\pi v}.$$

In the regime, where the system is performing random jumps between minima, characteristic of a Poisson process, the randomness parameter is in the range

$$\frac{1}{N} \leq r \leq 1,$$

with N the number of minima in one cycle. The lower limit occurs for equivalent minima; the upper limit is approached when hopping from one of the minima is rate-limiting.

Beyond the critical voltage, V_{onset} , the motor continuously rotates or slides down along the tilted washboard potential. The mean velocity is dominated by the drift, $v \simeq \tau_w/\eta$, where η is the friction coefficient, and the effective diffusion coefficient asymptotically approaches the free-particle diffusion limit, $D \simeq D_0 = k_B T_{\text{eff}}/\eta$. The randomness parameter in this limit becomes

$$r \simeq \frac{2D_0}{(\tau_w/\eta)d} = \frac{2k_B T_{\text{eff}}}{W_\tau} \ll 1.$$

The stepping becomes highly sub-Poissonian, reflecting the fact that the motion is tightly coupled to the deterministic force, and thermal fluctuations are small compared to the massive mean drift. As such, it is a useful parameter to describe the characteristics of the two regimes. In fact, according to Ref. [12], r is super-Poissonian in the critical region.

We have added a discussion of these matters in the newly added Supplementary Section 4.2.3.

As mentioned above, in the high-voltage regime, the molecule is expected to rotate at frequencies beyond the temporal resolution of the available electronics, preventing the detection of discrete rotational steps, if they exist. Consequently, we cannot determine experimentally the randomness parameter in the high-voltage regime.

- (b) Carefully check the language used to describe the behaviors. A sentence like “stochastically jumps between three different values in a well-ordered cyclical manner” raises the question: is it stochastic or well-ordered? The authors also use the term “periodic jumps” to describe the same phenomenon. “Periodic” suggests non-stochastic behavior.

Author’s Response: Thank you for this input. We have revised the main text and the Supplementary Information to avoid confusing terminology.

In the voltage range where cyclic jumps between current states are observed, the stochastic dynamics are strongly biased toward a well-defined sequence of states. At each current state, the system behaves stochastically, as indicated by the above-mentioned exponentially decaying dwell-time distributions (see Supplementary Fig. S9). Yet, at the same time, we observe a well-ordered cyclic behavior (e.g., $A \rightarrow B \rightarrow C \rightarrow A \rightarrow \dots$).

It is important to appreciate that when these cyclic transitions are observed, the studied molecular machine operates at 4.2 K, or at slightly higher temperatures due to local heating, under the condition $eV \gg k_B T$. Therefore, despite its stochastic nature in this regime, the molecular machine’s dynamics are strongly biased, driving the system through the well-ordered sequence of current states observed in the experiment. See our related reply to item 5b,iii, where we comment about the extremely low probability for backward rotation.

5. Statements about directionality.

- (a) The authors use the terms “100% preferred directionality” and “100% unidirectional”. Please choose one term, give a strict definition, and show how the analysis was done.

Author’s Response: We have revised the main text to avoid the phrase “100% unidirectional”. Directionality is now defined in the manuscript as the percentage of observed jumps that follow a cyclic sequence of current steps (ABCA in Fig. 1k and ACBA in Fig. 1m) out of all observed jumps.

The analysis was performed as follows, as mentioned in the revised manuscript.

For Fig. 1k, Directionality = $100 \times (\# \text{jumps}_{A \rightarrow B \text{ or } B \rightarrow C \text{ or } C \rightarrow A}) / \# \text{jumps}$.

For Fig. 1m, Directionality = $100 \times (\# \text{jumps}_{A \rightarrow C \text{ or } C \rightarrow B \text{ or } B \rightarrow A}) / \# \text{jumps}$.

- (b) On page 4 it also becomes clear that the “100%” term only applies to a fraction (122 out of 316) of the observed molecules.
- i. What is the behavior of the other ones?

Author’s Response: In all other cases, only jumps between two current states are observed. Since a cyclic sequence cannot be established from jumps between two states alone, the directionality cannot be determined in these cases. The classification of the 316 cases is now presented in the revised Supplementary Table 1 (Section 2.3).

- ii. Expressing 122/316 as “100%” is like having 122 green ones and 194 of undefined color and saying: “100% are green.”

Author’s Response: 100% directionality refers to the behavior of each **individual** system among the 122 cases, for which the directionality can be determined. It does not refer to the fraction of systems that exhibit directionality.

In the text on page 4, it is now stated: “We identified 122 systems with three or more current values, required to determine directionality. In every case, all observed jumps followed a single cyclic direction, corresponding to 100% directionality.”

Previous version: “We identified 122 cases with three or more current values, required to determine directionality, all showed 100% unidirectional motion.”

- iii. For any type of molecular process, there should always be some level of mis- or back-stepping. Truly 100% directionality would imply diverging energy dissipation (diverging entropy generation) during rotation. Please comment on the 100%-assessment from a stochastic thermodynamics perspective, and/or moderate the claim.

Author’s Response: Indeed, backward rotation events have a finite probability, and in the limit of infinitely long observation times, we expect the directionality to fall below 100%. However, within the finite duration of our measurements, we did not observe any such events. Note also that our measurements of cyclic motion were performed at sufficiently high bias voltages ($eV \gg k_B T$) to drive the current-induced motion. Consequently, rotation against the driving force is highly suppressed in this regime. To estimate the probability of forward versus backward rotation events (see the revised Supplementary Section 2.4.1), we use the data set labeled 3 in Fig. 2d to determine the value of $\gamma e / k_B T$, which is then used to calculate the ratio between the probabilities of forward and backward jumps for an applied voltage of 420 mV. From Fig. 1d, recorded at 420 mV,

we obtain a forward jump rate of 2.7 Hz. Therefore, the expected average waiting time to observe a backward rotation event is on the order of 3.7×10^{22} seconds, rendering it a very unlikely event. In the caption of Fig. 1, we now mention this issue and refer readers to Supplementary Section 2.4.1.

6. While the paper does not discuss the details of the imagined molecular torque mechanism, the authors generously use physical terms that normally are associated with the macroscopic world. This obscures the physics and risks creating confusion.

Author's Response: The referee touches upon an important aspect, relevant when discussing goings on in the microscopic world. To what extent can one use concepts from the macroscopic classical world? In the world of electrons and atoms, we know that quantum mechanics governs. But, as objects become larger, classical notions are indeed useful. First, we certainly consider the rotation of the entire helicene molecule as a classical phenomenon. Force and torque are classical concepts, while energy, momentum and angular momentum are concepts that are meaningful in both worlds. Angular momentum, for example, can be considered classical if its value is much larger than Planck's constant, $\hbar$. In Supplementary Section 4.2.1, we estimate the angular momentum of the rotating molecule to be about $100 \hbar$, well within the classical regime. Electrons are certainly quantum objects, and if described by a given wavefunction, their momentum is not certain, but has a mean value. If the electron wave is scattered by an object — for example, the molecule — momentum is transferred from the electron to the molecule. There are many electrons in the electrodes, and those that get close to the molecule scatter off it, transferring momentum and angular momentum to the molecule. Force and torque are defined as the average (quantum and thermal) rates of transfer of momentum and angular momentum to the — classical — molecule. If the electrons are in thermal equilibrium, all these forces are what in classical physics terms “conservative”. They actually determine the equilibrium positions of the atoms, and the vibrational frequencies. If the electron system is out of equilibrium, as is the case when a bias voltage is applied and electrical currents flow, new forces and torques emerge, and they are in fact non-conservative. The last couple of decades have produced an extensive literature on these concepts, and we build on that [3–5]. In the Supplementary Information, we review that theory and show how it applies to our particular case of a molecule suspended between two metallic electrodes. It is common practice to refer to the electrons in a metallic wire as an “electron gas” and when current flows, there is an “electron wind” in that gas. This terminology is useful because these microscopic systems share many features with their macroscopic analogues.

In response to the referee's important comment, we have revised the relevant section of the main text (while keeping in mind the strict length limitations) to better explain the tilted washboard picture of the torque generated in the molecular motor by the impact of electrons injected across the molecular junction. Importantly, we have added Section 4.4 to the Methods, which concisely explains this mechanism. A more detailed description is provided in Supplementary Sections 4–6.

- (a) “Injected electrons transfer momentum” (p. 4) to me implies a role for inertia at a molecular scale. This requires substantiation. Momentum transfer from whom to what and how? Is it momentum in the macroscopic (high Reynolds number) sense? It would be far more reasonable to discuss the physics in terms of biased diffusion.

Author's Response: The electrons transfer momentum to the molecule, as they scatter off the molecule. See above discussion.

- (b) Reverse of current direction is not proof of momentum transfer; it only means that linear (or uneven) response relations are observed.

Author's Response: The reversal of the current direction, which leads to the inversion of the helicene rotation, is not, by itself, proof of momentum transfer. However, it is naturally explained by the mechanism of current-induced molecular rotation driven by momentum transfer. Within this framework, reversing the current direction also reverses the direction of the current-induced torque, thereby reversing the direction of molecular rotation. We therefore consider this observation on page 4 of the main text as an indication of the relevance of the proposed mechanism.

- (c) I object to the terminology "current booster", "current boosting effect", and "current amplification". These are imprecise terms and suggest active amplification processes, which is misleading and physically incorrect. The established and correct term is "nonlinear conductance".

Author's Response: Our terminology is intended to describe the key experimental observation of a pronounced current increase above a threshold voltage, see Fig. 1, i.e. a highly nonlinear current-voltage characteristic. In most molecular junction studies, molecules serve as inactive resistors. Our analysis, however, shows that the rotating helical molecule enhances the current. This occurs by visiting highly conductive regions in a rotational energy landscape, while current-induced forces drive the molecular rotation, leading to an avalanche effect that "boosts" the current. Theoretically, this behavior is explained by a crossover from a predominantly trapped state to a continuously rolling state in the tilted-washboard model. For all these reasons, we view the chiral molecule as an active component of the circuit. We therefore think that the term "current booster" captures both the nature of the component and the experimental observations, and would like to keep this expression. We hope this explanation clarifies the reasoning behind the selected terminology. We also note that, to our knowledge, a similar electronic circuit with a covalently connected rotating molecule has not yet been realized. The description of this novel device and the phenomenon, therefore, requires new, creative formulations.

- (d) Macroscopic terminology such as "molecular windmills" and "electric winds" are imprecise at best and contribute to confusion rather than clarity. I have no idea what the authors mean by "electric wind". Electrons flying through vacuum? How would this be reconciled with the model in Fig. 4?

Author's Response: In our general response to the referee's comment, item 6 above, we explain why classical concepts are indeed useful and relevant in this context. Note, specifically, that electrons in a crystalline metal, described by the so-called Bloch waves, propagate through the crystal much like free particles in a vacuum.

7. It is not clear to me whether there is experimental evidence of rotation for $V > V_{\text{onset}}$, or whether rotation in this regime is an assumption.

Author's Response: The expected frequency of the molecular rotation for $V > V_{\text{onset}}$ is in the range of 10-100 GHz (Supplementary Section 6.2.5 and Fig. S34). Given the characteristics of the associated current fluctuations and the studied system, the mentioned rotation cannot be directly detected using available commercial or home-built electronics. Several independent experimental observations indicate that the behavior observed above V_{onset} is associated with molecular rotation as explained in detail in Supplementary Sections 2.5.2, and 2.5.3, as well as in the main text (page 8, 2nd paragraph). These observations are also summarized below.

1. The tilted-washboard model inherently predicts a transition between two distinct behaviors: cyclic molecular jumps and continuous molecular rotation. Experimentally, we observe cyclic molecular jumps and an abrupt increase in current at $V > V_{\text{onset}}$. This already points to a possible link between continuous molecular rotation and the abrupt increase in current at $V > V_{\text{onset}}$.

2. Using an Arrhenius-like expression, the voltage dependence of the cyclic current jumps measured in the low-voltage regime predicts a transition to continuous rotation at the same V_{onset} at which the abrupt current increase is observed. This analysis indicates that the abrupt current increase above V_{onset} is indeed associated with continuous molecular rotation.

In more detail, we use the measured exponential dependence of the rotation rate on voltage (Fig. 2d,e) together with Eq. (4) in Supplementary Section 2.4 to extract the voltage at which the molecular rotor acquires sufficient energy to overcome the rotational energy barrier ($V = E_b/e\gamma$). In Supplementary Fig. S12, we compare these voltages (blue squares) with a histogram of the measured voltages at which the current shoot-up is observed (light blue). The agreement is good, with the histogram peak located at slightly lower voltages than the $E_b/e\gamma$ range. The tilted-washboard model indeed predicts that the crossover to free rotation occurs before the rotor acquires sufficient energy to overcome the energy barrier. Continuous rotation already sets in when the work done by the current-induced torque exceeds the work dissipated by friction (see Supplementary Section 2.5.3 for more details).

3. Calculations show that the work done by torque transfer from the incoming electrons to the helicene molecule depends non-monotonically on the molecular pitch, reaching a maximum at intermediate electrode separations (Fig. 2f). Experimentally, the current required to trigger the abrupt current increase (I_{onset}) shows the opposite trend, with a minimum at similar intermediate separations (Fig. 2g,h). Thus, when the work done by the transferred torque is maximal, as found by calculations, a lower current is required to induce the abrupt current increase, as found in the experiments. The agreement between these theoretical and experimental trends corroborates that the current increase arises from molecular rotation.

4. For Au-helicene-Au junctions, we first observe cyclic current jumps (e.g., $A \rightarrow B \rightarrow C \rightarrow A \rightarrow \dots$) at low bias voltages. As the voltage increases, these cyclic jumps disappear one by one, becoming too short to be resolved by the available electronics, and are eventually followed by an abrupt current increase above V_{onset} . This gradual evolution suggests that molecular rotation most likely persists above V_{onset} , but the molecular rotor dynamics become faster than the time resolution of our measurement setup and therefore cannot be directly detected. In contrast, control experiments using a linear π -conjugated dithiolated anthracene, which has a comparable molecular length and conductance but lacks chirality, showed neither cyclic current jumps at low voltages nor the abrupt current increase at high voltages. These observations provide evidence that the molecular rotation is closely associated with the behavior observed above V_{onset} (see Supplementary Section 2.5.2 for more details).

To conclude, the above set of complementary indications establishes, in our view, a robust picture in which the molecule rotates at $V > V_{\text{onset}}$. These indications are presented in the main text and in dedicated sections in the Supplementary Information.

8. At high currents, the authors observe evidence that the molecule is expelled and a metal contact is formed. This should be stated more clearly early on in the paper and the corresponding regime marked in all relevant figures. Since the authors tie this transition to $G \approx G_0$, I suggest showing a G -axis in addition to current in all relevant figures.

Author's Response: The current at which a metallic contact forms can only be determined statistically by terminating a large number of I - V measurements at different, arbitrary current values. This is because the abrupt current increase occurs too rapidly for us to interrupt the measurement at a predefined current during the jump. Instead, we terminate the measurement after observing an upward curvature in the current. By the time the electronics stop the voltage sweep and reduce the applied bias to zero, the junction has already undergone the abrupt current jump to an arbitrary and uncontrolled current value (denoted I_{max} in the manuscript), which can be either below or above the exact current at which the molecular explosion occurs.

In Fig. 1, we indicate the approximate current at which an Au contact is expected to form. This is also mentioned on page 4 and in the caption of Fig. 1. Figure 1 is the only figure that introduces the different regimes and effects; Fig. 2 focuses on the regime $V < V_{\text{onset}}$, Fig. 3 on the identification of Au or molecular junctions, and Fig. 4 shows theoretical results. At the high currents that lead to the formation of atomic junctions, the system operates in a highly nonlinear regime. The concept of conductance as a linear-response property is therefore not well defined, and we therefore refrain from including conductance as an axis in the plots.

Referee 2:

This paper presents a combined experimental and theoretical study examining the behaviour of a long helicene molecule placed between two electrodes in a break-junction configuration under applied bias. Specifically, the authors demonstrate that directional molecular motion is induced, which in turn drives a substantial enhancement in current.

This is a fascinating observation that appears to support earlier theoretical predictions. Overall, the data seem robust and support the interpretation proposed by the authors, at least in my opinion. I believe this study will be of broad interest, not only to researchers working on chirality and its role in molecular motion and electron transport, but also more generally to the molecular machines community.

I approached this paper primarily from the perspective of an experimentalist with expertise in chiral materials and their optical and electronic properties. However, I am not an expert in break-junction techniques or in the theory underpinning this study. As such, many of my comments and questions arise from scientific curiosity rather than deep technical familiarity.

Author's Response: We thank the referee for reviewing our work and for the supportive and constructive comments.

1. The directionality of the molecular motion is inferred from the observation of three or more different current levels attributed to distinct rotational states (e.g., Figures 1d-i). Owing to the timescale shown, these three states are particularly clear in Figures 1d and 1g, but become progressively less distinct at higher applied voltages because of the increased rotational frequency. Could the authors confirm that, at 660 mV in particular—which lies beyond the threshold voltage associated with the observed current enhancement—the three states remain clearly resolved? A zoomed-in trace over a shorter time interval might help to clarify this point.

Author's Response: Fig. S8 in the revised Supplementary Section 2.3 provides more data for the same measurement set shown in Fig. 1d-i presented with higher temporal resolution. This set includes data acquired at applied voltages of 600, 660, 700, and 740 mV. As shown in Fig. S8a, cyclic current jumps ($A \rightarrow B \rightarrow C \rightarrow A \rightarrow \dots$) are clearly observed at 600 mV. However, as the bias is increased to 660 mV (Fig. S8b), the dwell times in all three states decrease, with state C becoming the first to nearly disappear, being represented only occasionally by 1-3 data points. At

700 mV (Fig. S8c), current fluctuations are observed below state A, and it is no longer possible to determine reliably whether state C is still sampled or whether the fluctuations originate solely from states A and B. Note that given the very low temporal occurrence of state C already at 660 mV, it is most likely that only states A and B are sampled at 700 mV. Finally, at 740 mV (Fig. S8d), current fluctuations become the dominant feature, with most of the signal concentrated near and below state A, which is now less well resolved. We further refer to our response to Referee 1, item 7, which summarizes the indirect experimental evidence for molecular rotation in the high-voltage regime, where the expected rotation frequency exceeds the temporal resolution of the available electronics. A similar discussion has been added to Supplementary Section 2.3, together with the new Fig. S8.

2. I also noted that the authors report identifying only 122 out of 316 molecular junctions that exhibited three or more distinct current states. I was therefore curious about the behaviour observed in the remaining cases where fewer than three states are present. For example, could these correspond to different molecular junction configurations, and if so, might the later DFT calculations provide insight into these alternative behaviours? Alternatively, is there another origin for the observations in junctions that do not exhibit more than three states?

Author’s Response: The remaining 194 junctions exhibited jumps between two current states. The detailed distribution is now summarized in the revised Supplementary Table 1 (Section 2.3). Indeed, they should correspond to certain molecular junction configurations.

Our DFT calculations for both the top-top (TT) and hollow-hollow (HH) structures provide the following insights. A threefold electrode symmetry, typical of the (111) Au facet, gives rise to three minima in the total energy as a function of the molecular rotation angle. These minima are, however, separated by very different energy barriers, i.e. ~ 1 eV for HH and ~ 10 meV for TT, see Fig. S13. The minima all exhibit identical electron transmission (the conductance equals the transmission at the Fermi level, in units of G_0 , $G = G_0\tau(E_F)$) for highly symmetrical situations with the two electrodes directly facing each other. For TT the modulation of the conductance as a function of rotation angle is so low that there is only a single conductance state predicted, see Fig. S17. In Supplementary Section 3.2.3, we demonstrate by calculations of the HH geometry that a slight displacement of one of the two electrodes relative to the junction axis is sufficient to induce non-equivalent energy minima associated with three different transmissions. Unequal energy minima indicate non-identical metal-molecule-metal configurations. Thus, it is not surprising that they are associated with different electronic transmissions and hence electrical conductances.

In view of the intuition provided by our DFT and related quantum transport calculations, the observation of only two current states can result from various scenarios. A possible one is a junction with more than two energy minima, but with degeneracy in the transmission of some of the minima, such that only two distinct current states can be experimentally resolved (see calculated examples in Fig. S18). In addition to the 316 molecular junctions that exhibited two or more distinct current states, we also observed junctions with no detectable current jumps at voltages above 200 mV. These cases may either arise from rotational energy barriers that are too high to permit molecular rotation under the experimentally applied bias voltages or too low to resolve conductance modulation, as predicted for the TT geometries. A relevant discussion has been added next to Supplementary Table 1.

3. Finally, I had a question regarding the design of the helicene itself. The molecule employed in this study clearly derives from the synthetic chemistry developed by Michael Pittelkow’s group and appears to perform very effectively in this context. However, the structure contains several notable features, including the two helical turns as well as the various heteroatoms and side chains

introduced through the synthetic methodology. It was not entirely clear to me from the modelling section how these specific molecular features influence the measured properties. For example, would a fully carbon-based helicene exhibit similar behaviour, or would the effects differ significantly? If the authors are able to comment further on the role of these structural features, this would strengthen the discussion.

Author's Response: These are indeed interesting aspects. The side chains and, in particular, the bulky tert-butyl groups protect the non-bonding electron pairs of the nitrogen and oxygen atoms from unwanted binding to the electrode tips. This is an advantage for a molecular rotor structure, as such binding is likely to prevent its operation. Regarding the molecular rotor's operation, the side chains influence the molecule's moment of inertia. For fully carbon-based helicenes with 13 rings and the same binding groups to the electrodes, we expect, based on our theoretical models, a generally similar behavior, since the key features are the helical structure, the binding groups, and the interaction of the π -system with the electrodes. As for other helicene molecules with different lengths or structures, we hope that this work will stimulate follow-up studies of molecular machines based on such compounds, which could establish structure-function relationships across a wide range of torque-transfer molecular machines.

These aspects are now mentioned in the main text in relation to Fig. 4 and in Methods Sections 4.1 and 4.4.

Referee 3:

The manuscript presents experimental and theoretical results on a helicene molecular junction. At fixed bias, the current shows telegraph noise with three levels (A, B, C). A sequence $A \rightarrow B \rightarrow C \rightarrow A \rightarrow \dots$ occurs, as it is claimed, 100% of the time, unlike the alternative sequence $A \rightarrow C \rightarrow B \rightarrow A \rightarrow \dots$. This observation is linked to previous studies of molecules on surfaces, where such current dynamics implied current-induced rotation. In the present manuscript, the detailed analysis justifies the assumption of a rotation.

The theoretical analysis supports the claim by providing a potential energy surface from *ab initio* calculations. The focus is on explaining the extreme current blowup at a certain voltage. A Born–Oppenheimer-type mechanism with a resonance moving close to the Fermi level is consistent with the experimental observation. Other mechanisms are studied, namely pumping beyond Born–Oppenheimer. This analysis could deserve a separate publication.

The manuscript presents a significant advance in the field of electrically powered molecular motors, and I recommend its publication.

Author's Response: We thank the referee for reviewing our work, for the very positive assessment, and for the helpful suggestions.

I have only minor remarks that the authors could take into account upon revision:

- (A) The buzzword IETS (inelastic electron-tunnelling spectroscopy) is introduced in the main text and Fig. 3. It suggests that a new measurement technique is employed. However, the authors just measure the electric current, as in Fig. 1. The subsequent analysis mentions a zero-bias conductance, which is not inelastic. A possible inelastic effect is discussed only superficially. The use of IETS as a “spectroscopy” could be reconsidered.

Author's Response: Following the referee's comment, we have revised the manuscript. This in-

cludes replacing the term "IETS" with "differential conductance" and updating the corresponding explanation.

Differential conductance measurements are used in this work as a diagnostic tool to determine the type of junction. As it is not the subject of this study, we do not discuss the underlying inelastic electron-phonon interaction in detail. The detection of steps in the differential conductance vs. voltage (dI/dV vs. V) curves at voltages corresponding to energies characteristic of maxima in the Au phonon density of states is used to identify Au atomic junctions. In contrast, steps at voltages above the Au phonon energy range are used to identify molecular junction vibrations, confirming the presence of a molecule in the junction. In this sense, our differential conductance measurements serve as a spectroscopic tool that detects metallic phonons or molecular vibrations, as shown in Fig. 3b.

As an independent diagnostic tool, we also used the zero-bias differential conductance extracted from the same differential conductance measurements to distinguish between Au atomic junctions (which typically have conductance of one to several G_0) and molecular junctions (whose conductance is typically orders of magnitude lower than $1G_0$). As the referee correctly noted, the latter complementary analysis is not spectroscopic and does not probe the inelastic electron-phonon contribution, unlike the analysis described above.

We note that, from a technical point of view, I vs. V and dI/dV vs. V measurements are different and are performed using two separate electronic circuits in our setup. The first is a DC measurement, whereas the latter is an AC measurement performed using a standard lock-in technique.

- (B) The work would benefit greatly from strengthening the role of structural chirality. What happens if the molecule is replaced by its mirror partner? Is it experimentally feasible? On the theoretical side, such discussion would involve changing a few atomic coordinates or model parameters.

Author’s Response: In this work, we use a racemic mixture of left- and right-handed enantiomers. Based on our theoretical derivation in Supplementary Section 6.1.3 and the main text, inverting the molecular chirality while preserving the current direction reverses the torque acting on the molecule and, consequently, the direction of molecular rotation. Experimentally, we detect cyclic transitions between discrete current states (e.g., $A \rightarrow B \rightarrow C \rightarrow A \rightarrow \dots$). However, for two molecular junctions examined separately, the same sequence, $A \rightarrow B \rightarrow C \rightarrow A$, may correspond to opposite directions of rotation because these states are defined by their relative current values (A: highest; B: intermediate; C: lowest) rather than by their rotation angle. As a result, although it is possible to study enantiomers of opposite handedness in separate experiments, we cannot compare their directions of rotation. We note, however, that the manuscript presents experimental results, corroborated by calculations, demonstrating that mechanically tuning the molecule’s helicity through elongation or compression alters the onset current for current boosting.

These aspects are discussed in the revised main text and in the newly added Supplementary Section 6.1.3.

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Reply to Referees - Nature Manuscript 2026-02-05351A

The referees' comments are reproduced below in black. Our responses are shown in blue. In the revised manuscript, revisions are also highlighted in blue.

Referees' Comments

Referee #1 (Remarks to the Author)

The authors provided a very thorough and thoughtful response and made significant updates, mainly to the SI but also to Methods section 4.4, which is very helpful. Thanks for this. Between the main text, the authors' response and the updated SI, I now feel comfortable with the experiment and with the theoretical treatment.

Unfortunately, however, the changes to the main text (except the substantial update in Methods 4.4) are mostly cosmetic and do not fully reflect the substance of the responses given by the authors to the referees.

Author's Response:

We thank the referee for the positive assessment, thoughtful comments, and time invested in the review process.

Key points that remain to be addressed in the main text

1. My point about using macroscopic language was not so much about quantum versus classical, but about human-scale, macroscopic language (wind, turbine blades) versus molecular. The authors argue that this language is established in the field. In the main text they refer to Refs. 15 and 21–27. Checking the references does not back up this claim. For example:
 - (a) Ref. 15 exclusively uses rigorous, scientific language such as “angular momentum transfer”—no “wind”.
 - (b) Ref. 22 uses the word “windmill” in the title, but the language in abstract and main text is rigorous and specific, for example:

“The proposed force arises when a dc voltage is applied between the external electrodes, which produces a ‘wind’ of electrons, for example, from left to right. The incident electron flux (from the achiral CNT) possesses zero angular momentum, whereas after interacting with the chiral nanotube, the outgoing current carries a finite angular momentum. By Newton’s third law, this flux of angular momentum produces a tangential force (and an associated torque) on the inner nanotube, causing it to rotate.”

The everyday term “wind” is here used for illustration and not instead of a physical explanation, but as a complement.
 - (c) In their response, the authors also refer to Refs. 3–5 of their response. However, Ref. 3 does not use the word “wind” a single time. Refs. 4 and 5 each use “electron wind” once, but in quotation marks when referring to other works. All three papers describe the actual physics using well-defined scientific terms, as one would expect.

In contrast, the main text of this work stands out in using human-scale language *instead of* a precise scientific explanation. The latter is, unfortunately, found mainly in the SI and in the response to reviewers.

Author's Response:

In response to the referee's comments, we have revised our terminology in the main text and the Methods. That said, we believe it is important to clarify that the classical analogies used in our manuscript have a rigorous physical basis. The question of how extensively to use such analogies reflects a long-standing dilemma in communicating abstract scientific findings. For example, in our manuscript, we draw an analogy between the effect of an electron current on a helical molecule and that of wind on a windmill. This analogy is not merely illustrative: the same physics describes the transfer of angular momentum both from the wind to a windmill and from moving electrons to the helical molecule, causing rotation in both cases.

The use of macroscale analogies to describe abstract laws of the microscopic world has been with us since the beginning of quantum theory. For example, we speak of "electron waves" even though quantum-mechanical waves are very different from water waves. The connection lies in the underlying mathematics of both phenomena, making such analogies more than merely illustrative. In fact, they can provide valuable physical intuition and greatly facilitate the understanding of otherwise abstract concepts.

The classical analogies that we employ have been carefully considered, are scientifically grounded, and provide useful physical insight. We note that they are comparable in spirit to the term "resonant tunneling," which the referee suggested as a source of inspiration. Indeed, the term "tunneling" itself originates from a macroscopic, classical concept that was adopted to describe a quantum phenomenon.

A brief search on Google Scholar shows that the term "electron wind" is established terminology (e.g., by searching for "electron wind" + "current"). In fact, the term "electron wind" was coined as early as the late 1950s in the context of electromigration [1-5] and has since become a well-established scientific expression [6-12].

Despite our different perspectives on this issue, we have carefully considered the referee's comments. We have replaced some of the expressions, including "electron wind" and "molecular windmill", and have taken care to introduce classical analogies, such as the macroscale windmill and the pitch angle of turbine blades, in a clear and physically well-grounded context. More generally, we have tried to avoid overly pictorial descriptions. The detailed revisions can be found in a separate PDF of the main text, in which all changes have been highlighted.

2. I continue to object to the term "current booster". I maintain that the established physical terms should be used wherever possible. For example, "current boosting capabilities" is simply the property of a highly nonlinear conductance. Instead of "mechanical current booster" one can write mechanically induced, (highly) nonlinear conductance.

One reason for avoiding the booster-language is to not introduce new words to describe established concepts, which leads to confusion. An even more important reason is that "booster" and "amplification" suggest active, dissipating devices such as in a transistor-based current amplifier. This is misleading.

It may indeed be appropriate to coin a new word for the specific mechanism behind the nonlinear conductance observed in rotating molecules. As an inspiration, think of resonant tunneling. That is also a nonlinear conductance mechanism, with its own specific, descriptive name. In contrast,

“booster” is neither specific nor descriptive.

Author’s Response:

We have now replaced the words "boosting" or "booster". Specifically, “current boosting” was used by us in different contexts: (i) to describe an effect or (ii) to refer to an observation. We therefore revised the terminology according to the context.

We prefer to avoid phrases similar to “highly nonlinear conductance” for the following reason. In the theoretical model, the conductance is well defined and oscillates periodically with molecular rotation. However, the actual experimental observation is a nonlinear increase in current with voltage. In the measured I - V curve, the differential conductance (i.e., the slope, dI/dV) is not well-defined at the abrupt current increase and formally diverges.

The referee views the mechanism underlying the current increase as passive and interprets the phrase “current boosting” as implying an active device. The angle-dependent variation in conductance is indeed passive. However, the increase in current arises from current-induced rotation, a mechanically active dynamical process that causes the molecular rotor to spend finite amounts of time at different angles, thereby sampling also highly conducting junction geometries.

In seeking an appropriate term, we now use “current enhancer” as a compromise. It specifically describes the observed enhancement of the current without implying that the rotor "boosts" the current. This also distinguishes the effect from, for example, “current modulation,” which describes a variation in current but does not convey the observed substantial current increase.

The modifications to the main text, including a change of the title, are highlighted in a separate PDF.

3. The second paragraph of the introduction contrasts a “Brownian ratchet mechanism” against “electron-wind induced rotation”, suggesting they are two different things. The authors’ response to my comments makes clear that this contrast does not exist: the observed effects can very well be described in terms of diffusion or biased motion in a tilted potential. I liked the authors’ text in the response:

“In general, for a tilted energy $U(x) = -Fx$, the slope F is the force acting on the particle. In our case, the energy is a function of a rotation angle θ , and in that case, the slope is a torque. The torque that gives rise to the tilt is the torque arising from the transfer of electron (angular) momentum to the molecule.”

This is a nice, concise description of the physics and also highlights that there is no contrast between a ratchet-type energy landscape and the physics discussed here. My strong recommendation is to adjust the introduction to remove the impression of a mystery and to add the simple physical description above in the main text.

Author’s Response:

We thank the referee for the input. We have revised the first paragraph on page 6 to include all the components of this explanation. In addition, the remark allowed us to shorten the introduction (in line with one of the Editor’s requests) by omitting the discussion of differences between motion in a ratchet potential and current-induced rotation.

Minor points

- New equation (caption of Fig. 1) uses a very informal notation. I do understand what is meant by #jumps but this is not how equations should be written.

Author's Response:

Thank you. We have revised the equation.

- Typo: “summerised” → “summarized”.

Author's Response:

We have corrected the typo.

References:

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* According to Google Scholar, including self-citations.

Referee #3 (Remarks to the Author)

The authors responded to my report satisfactorily and I recommend publication.

We thank the referee for reviewing our work and for recommending it for publication.