

Approaching Landauer's limit in a single superconducting vortex memory bit

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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)

The authors present a method to measure the pinning potential of a vortex in a superconducting thin film by imaging vortex vibrations induced by an AC drive current and extracting the work associated with vortex motion from the vibration amplitude and direction. The study is potentially suitable for publication in Nature Communications after the following issues are addressed.

1. Which aspects of the methodology are genuinely novel? Vortex pinning and dynamics have previously been studied using SQUID-based techniques (for example Refs. 10 and 17), and the calculation of work appears to be a direct extension of earlier approaches. The authors should distinguish their contribution from prior work and clarify what new capability or insight their method provides.
2. In the abstract, the authors state that "at high frequencies, the work required for bit manipulation (erasure) was shown to be within an order of magnitude of Landauer's limit ($kBT \ln 2$) at 4.2 K." However, as the authors note, the experimental protocol does not implement an erasure operation in the information-theoretic sense. The authors should discuss whether a true erasure protocol could be realized experimentally, especially in the low-frequency regime where the simulations in Fig. 5 suggest that the Landauer limit can be approached. If such a regime is not experimentally accessible, the manuscript should explain which experimental parameters would need to be tuned to reach it and comment on the implications for practical vortex-based memory devices. In particular, it would be useful to discuss whether the low-frequency limit can be pushed into a technologically relevant operating range.
3. In Fig. 1d,e, the driving force acts along the x-axis, yet the vortex displacement occurs at 45 degrees. The authors should explain the origin of this directional response. It would also be useful to comment on whether a lower force would be required if the current were applied perpendicular to the observed displacement direction.
4. The procedure used to extract a quantitative displacement length, l , from the images is not sufficiently clear. In addition, what sets the detection threshold for l ? It is not clear whether the limiting factor is SQUID noise, mechanical vibrations, or another source.
5. I am sure the authors have considered this. But they should address the possibility that the applied current induces mechanical oscillations of the probe or tip that could produce a similar signal.
6. In Fig. S2, the noise within each level of the telegraph signal is several microtesla, which appears large compared with similar SQUID sensors (for example Nat. Nano. 8, 639–644 (2013)). The authors should comment on whether this reflects excess measurement noise or unresolved small-scale vortex motion. Showing comparable data acquired far from the vortex would provide a useful baseline for comparison.
7. In Fig. S4, the x-axis lacks numerical labels and should be corrected.

Reviewer #2

(Remarks to the Author)

The paper advertises in the abstract experimental manipulation of a single superconducting vortex in the Landauer limit of the work necessary to change its position in a bistable potential. In principle, such a demonstration would represent a scientifically robust case as it touches the heart of thermodynamics and its intimate connection with the information theory. However, I do not find convincing experimental data in the presentation which would support the promise of the abstract. The paper lacks thorough and convincing experimental support.

It is important to distinguish in the current presentation experimental facts from model- and parameter-dependent evaluations. The experimental fact is that authors can observe a single vortex, as documented by the SOT images (Fig.1) and its bistability revealed in measurement of telegraphic noise (albeit with dynamics well below 1Hz). While this part of presentation belongs to the state-of-the art in the field, the calculation of work done on vortex is strongly model- and parameter-dependent. I am missing important experimental data, which seem essential to defend the way of reasoning presented by authors.

The current manuscript shows the typical drawback for works published in the literature on the Landauer limit: it does not verify the Landauer prediction directly by measuring the dissipation associated with the manipulation of a single bit. Instead these works rely on models and assumptions, which may push the interpretation towards the desired result. At the moment direct measurement of the Landauer limit seems to remain a formidable task, but such direct verification is necessary for unambiguous confirmation of the theory.

I do not agree with the statement that “this work introduces a new approach to quantifying the dissipation of individual vortices under periodic drive”. I would say it proposes a qualitative viewpoint on the vortex dissipation, but since the proposed picture relies on particular models (not verified!), assumptions and not-well known parameters it cannot be expected to provide a reliable quantification for the vortex dissipation. As authors wrote in the caption of Fig.1, their modelling is based on a toy model. Toy models are expected to create a framework within which it is easier to consider a given physical process, but taken alone they hardly predict proper numbers, even in cases when they are qualitatively correct.

To publish a paper in Nature Comm. I guess that the description should be self-contained. Authors refer to previous works trying to make shortcuts in their presentation to arrive at final conclusions. It unfortunately kills the flow of narration and creates feeling, that authors rely more on literature than on their experiment. Below I am providing the concrete comments referring to specific sections of the paper.

Major allegations:

1. It is not clear what images 1d and 1e present and how the B-signal is measured. Is it a Lock-In measurement of the first harmonics for the in-phase and out-of-phase value of B respectively? How can you infer the position of the vortex from such images?
2. I am missing a calibration curve B vs. I_{ω} , which is the basis to go from values of magnetic field measured in the experiment to the vortex displacement I_{ω} . Such calibration is mandatory if you want to give credibility to Fig.2.
3. Authors write: “At specific values of I_{DC} , a sharp increase of B_{oop} is detected. It systematically coincides with the appearance of higher harmonics in $B(r,t)$. This rise in the measured work is interpreted as periodic vortex jumps over an energy barrier that separates two local potential minima (Figure 1g, i).”

My comment: According to Fig.1g a vortex jump over energy barrier should be seen as a sizeable discontinuous displacement of the vortex, once the force exceeds a threshold value. I would expect to see a corresponding $B(t)$ trace acquired by the SOT for a harmonic current drive, for which such a discontinuous jump could be identified. Such a trace is mandatory for giving credibility to the proposed interpretation. It cannot be replaced by a toy model of Fig. 1(f-i). Authors refer (in fact they should demonstrate it in the current work by displaying appropriate data) to the previous work to claim resolution of the SOT better than 1nm. If it is the case a real (experimental) version of Fig.1g is expected.

4. The calculation of the vortex displacement as $I_{\omega} = (I_{ip}^2 + I_{oop}^2)^{0.5}$ seems to be correct only for the movement within a single-well harmonic potential. For the claimed movement in between the two local potential minima higher harmonics should play a role, and the total displacement cannot be calculated via the same formula.

5. It is completely not proven that the Kramer's formula can be applied to description of the studied experimental realm. This part of presentation has a character of speculation. To make scenario of thermal activation from one well to another plausible authors should have presented the telegraphic noise for double wells #3 and #4 (Fig.4) with dynamics at the level of kHz (comparable to τ_k of Fig.4). Also, they could have measured the telegraphic noise at various temperatures, which is the way to establish the actual “vortex jumping” mechanism in the postulated double-well structure. These type of demonstrations are prerequisite experiments to come up not only with the proposed Monte-Carlo modelling, but also to establish the validity of the toy model in Fig.1.

Meanwhile, authors present the telegraphic noise with dynamics below 1Hz (Supp.Fig.S2). It looks more like a spurious noise-induced jumps than thermal activation of the vortex over the potential barrier. BTW I would suggest to display here the two SOT images each for vortex located in one of the wells. What is the distance between the two wells' minima?

6. Fig.5. and the accompanying discussion is a loose narrative which is not well-anchored in the experiment. It is a pure

speculation.

Other comments:

1. Authors forget to give numerical values for physical quantities controlled/measured in the SOT experiment. This involves: B_{dc} , B_{ip} , B_{oop} (lack of scale for the color plots), I_0 , J_{ω} .
2. Authors write about sweeping of I_{dc} to find a sharp increase of B_{opp} . The role of I_{dc} is not discussed in the physical terms. Also it is not clear how the amplitude of the accompanying oscillating current I_{ω} is set.
3. The physical origin of the theta angle between displacement and force is not discussed.
4. Authors write that they measure work. It is not true. They estimate/calculate/infer/guess/speculate, but do not measure. There are too many conceptual bottlenecks on the way from experimental data to the claimed value of work to talk about an act of measurement.
5. The calculation presented in Supp.Mat.4 is central to provide a transition from values measured and imposed in the experiment to the actual claimed work. It should be presented in the main text.
6. Fig.3 and Fig.4 are overcrowded with panels and curves to the extent that they cannot be distinguished one from another. Authors should focus here only on few curves, but first they need to prove that the model of thermal activation is applicable in their case (see p.5 of major allegations).
7. I am missing an appealing intuitive physical description which would explain why I_{oop} component of the displacement is solely responsible for dissipation, which is the result of calculation presented in the Supp. Note 4. The simplest description would tell that the displacement lags in phase behind the harmonic force and it can be thus decomposed into in-phase and quadrature components (IQ demodulation). In fact this is enough to arrive to the formula which you use to calculate work. The higher harmonics, according to your interpretation, indicate periodic vortex jumps over an energy barrier. These higher harmonics seem to be responsible for dissipation, when vortex irreversibly changes its position from one well to another. Nevertheless, they average out when calculating work. Can you discuss the involved physical processes to illustrate better links between your experiment (B-field harmonics) and the investigated work/dissipation?

In summary, the paper does not provide a firm experimental verification of the Landauer limit. It relies too much on particular models (not confirmed experimentally in the current presentation) and order-of-magnitude estimations of various physical quantities necessary to calculate work. Critically, the analysis is not well-rooted in the experiment, and thus it is not convincing. The forces, works and vortex displacements are not determined directly but either estimated (force), inferred from the SOT measurements (vortex displacements, surprisingly authors do not present any experimental data here) or calculated (work). It is very important to display the actual as –received measured data (e.g. $B(t)$, $B(\text{vortex displacement})$) and guide the reader step-by-step while recalculating them into the finally presented quantities (work, displacement, force). The only as-received experimental data in the main text are the SOT images (Fig.1b-e). A serious drawback of the presentation is the lack of the experimental confirmation of the double well vortex dynamics, which would involve the Kramer's thermal activation behavior. I cannot recommend the paper for publication.

Reviewer #3

(Remarks to the Author)

The present manuscript reports on quantifying a dissipation of single vortex motion under a periodic drive and suggests feasibility of the memory bit based on a superconducting vortex confined to a nanoscale double-well pinning potential using a SQUID-on-tip microscope.

By measuring the magnetic signal modulation induced by vortex motion, the average work per cycle is directly assessed. The experiment is performed in a Nb film of thickness 55 nm which was grown and patterned into a 10 μm -wide bridge. Because the value of thickness is chosen to be of the same order as the coherence length (12 nm) and the London penetration depth (220 nm), a vortex is regarded as a particle in a random 2D potential landscape. #

The results are arranged in three groups according to three possible vortex manipulation protocols: at weak drives, stochastic resonance behaviour; at strong, low-frequency drives, adiabatic manipulation; at high frequencies, the work required for bit erasure is shown to be within the order of magnitude of Landauer's limit.

The authors report measuring dissipation levels orders of magnitude below those achieved in previous works and reaching levels below the Landauer's limit (see, e.g., Fig. 2c, Fig. 4c and Supplementary Figure Fig. S5j,k,l).

The present work demonstrates, both experimentally and theoretically, the fact that the fundamental Landauer's limit that establishes the minimum energy needed to erase one bit of information within an irreversible change of information, can be overcome. The energy consumption below that limit is attributed to other measurement protocols, as compared to those implied in the original Landauer's derivation of 1961. A comprehensive analysis of different protocols is summarized in Fig. 5. The minimum energy needed to erase one bit of information in the present work relates to a continuous vortex

manipulation protocol, while the Landauer's limit recovers in the single-shot protocol. This finding is, however, not explicitly reflected in Abstract.

The distinct properties of the double-well systems under study are unified within a one-dimensional Monte-Carlo simulation assuming a simple model pinning potential with two fitting parameters, the height of the energy barrier between the two wells and the distance between the two potential minima. A sinusoidal driving force, matching the Lorentz force occurring in experiment, tilts the pinning potential. The stochastic physics behind the modelling gives, in principle, a fair description of the displacement and work as a function of the driving frequency, except for the regular pattern of the simulated Figs. 3d,h,f in contradistinction to irregular (fluctuating?) regimes in the experimental Figs. 3c,g,e, which should have been explained. Another point showing that the used simple model reveals some inconsistency, is the fact mentioned by the authors that for double-well #3 significantly different values of the distance between the two potential minima are used to fit the displacement and work (Supplementary Table S1). More measurements on every sample as well as experiments for a larger number of samples would provide an enhanced statistical representativeness to the proposed model.

A few refinements are needed in the manuscript. The explanation "another factor 2 related to the initial condition of the vortex" above Fig. 5 is not self-explanatory. In some places, e.g., below Fig. 4, the appropriate expression seems to be "double-wells #3 and #4 present". The sentence "For example, by integration with vortex control elements and readout demonstrated in Ref. 2" in Discussion is grammatically incomplete. In Fig. 5, the sequence of panels must be inverted.

In summary, the manuscript provides a sound validation of an efficient memory element for low-energy consumption memory devices. It can be suitable for publication in the Nature Communications once the authors have taken into account the above comments.

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

All my comments were satisfactorily addressed. One minor comment - from the replies and revisions, it seems like the technique presented here is not really applicable to efficient memory manipulation, at least not in the sense of realizing an erasure protocol in a useful frequency. I would therefore encourage the authors to step back the title of the manuscript, in addition to the changes already made to the abstract and main text.

Reviewer #2

(Remarks to the Author)

The paper has been improved and it is now more accessible for readers. I would like to thank authors for much better explanation of the determination of the vortex displacements measured with LOCK-IN technique, which is now well-described in Fig.1. However, I still find problematic issues and inconsistencies, which I am bringing to attention of authors in hope that their presentation may become an excellent piece of work, once they are able to address them. Here they are:

1. To investigate the energy required to manipulate a memory bit, first you have to prove experimentally that you have such a bit, i.e. you have a system with two states with ability (i.e. an experimental knob) to swap between them. It would be the most natural introduction to the further investigations, devoted to determination of the energetic cost of such operations.

Authors write in the response to my first report:

"Regarding the suggestion to directly image the vortex in the two metastable states, the expected separation between the two minima is of the order of ≤ 10 nm, which is significantly smaller than the spatial resolution of our SQUID-on-tip probe (~ 250 nm diameter). As a result, the two states cannot be reliably distinguished in DC imaging. This aspect was discussed in prior work (Ref. 13) where we compare the DC image displacement with the AC technique used here. We copied this comparative figure below taken from that reference."

I am not convinced with the presented explanation. For double-well presented in Fig.3d the displacement is $L_{\omega} \sim 50$ nm. Importantly, it is only the displacement resulting from the measurement of the first harmonics. The real-space displacement could be even larger. It should be well experimentally catchable with the DC imaging, if to trust the resolution presented in Fig. Vortex trajectory of a vortex upon varying F_{dc} at fixed F_{ω} , which authors provide in their response to my first review. Also judging from the experimental profile presented in Supplementary Figure S10. London penetration length measurement, 50-100 nm displacement of the vortex should be detectable by DC mode.

Further, the trace presented in Supplementary Figure S2. Telegraphic Noise is a proof that authors can distinguish two positions of the vortex experimentally: there are two well-separated levels of B corresponding to two states of the vortex. What is the distance L between these two minima? Why don't you present memory operation for the vortex from the presented trace? It seems quite obvious that applying a positive force $+F_{\omega}$ should keep the vortex in one well, while a negative force $-F_{\omega}$ should allow to trap it in the second well. The lack of such presentation makes your presentation not as convincing as it could be and leaves some room for criticism: if the reasoning presented in the paper is right,

swapping the vortex deterministically between the two wells with applied DC current and its detection should be possible. This seems actually a prerequisite to conduct further studies.

Moreover, such approach would allow to verify experimentally the presence of hysteresis postulated in the toy model of Fig.2b. It would be very desirable to see experimental hysteresis instead of its conceptual illustration. Here, after applying the force and removing it you could even use LOCK-IN technique to take advantage of your displacement sensitivity of $dS_x \sim 0.3\text{nm}$ to resolve two distinct positions of the vortex. The hysteresis should be seen for double-wells #1 and #2 since they exhibit very low thermal hopping rates.

These are obvious experimental tests which transparently would confirm existence of double-well vortex systems in your Nb strip.

2. Authors write: "Regarding the spurious tip-noise being the reason for the telegraphic noise, we added the measurement of the tip noise for the same period of time when the tip is far from the vortex. See the updated Supplementary Figure S2 below."

My response: It is obvious that trace collected in the background must not show jumps in the signal. My previous question was about the reason for jumps in the telegraphic noise. I wonder if the activation of the vortex over barrier is indeed of thermal character. It may be caused by a spurious noise propagating in the Nb sample or in the SOT tip.

3. Authors write: "Within this experimentally established framework, the use of a Kramers-type description is physically well justified and provides a natural and quantitative interpretation of the observed dynamics in the weakly driven regime. Our use of this formalism is therefore not intended as a speculative assumption, but as a consistent description of the measured behavior."

My response: As long as you do not prove it experimentally it remains an assumption. How can you be sure that vortex is not pinned strongly? If it is, it would make the thermal activation mechanism ineffective and negligible.

Authors: "We agree that additional measurements, such as temperature dependence, could provide further insight into the microscopic origin of the activation process. However, in superconducting vortex systems, varying the temperature also modifies key physical parameters such as the coherence length and pinning landscape. As a result, it is not possible to independently vary the thermal energy while keeping the underlying bistable potential unchanged, which limits the applicability of such an approach in the present context."

My response: Yes, the activation process should be tested experimentally. Even if there are certain limitations, which you mention, you should prove that your activation mechanism somehow feels the temperature. May be you cannot recover the theoretical Kramer's behavior from such analysis, but qualitatively you should see differences in the hopping rate for various temperatures. Particularly, you should see the difference in the telegraphic noise of Supp.Fig.2, when the temperature changes.

4. About your modelling of the thermal activation. You write in the description of the data presented in Fig.3: "The change in F_c can be explained by thermal activation, which plays a greater role at low frequencies." But in other place you estimate $r_k = 10^{(-45)}\text{Hz}$ and $r_k = 10^{(-6)}\text{Hz}$ for double-wells #1 and #2, respectively. E_b is U_{tot} dependent, and U_{tot} is dependent on F_{ω} . Are the quoted values of r_k given for $F_{\omega} = 0$? How do they change at $F_{\omega} = F_c$?

5. How does the distribution of the sheet current density change with frequency?

6. The coherence length in your experiment is $\xi = 12\text{nm}$, London penetration depth is $L = 220\text{nm}$. Meanwhile you fit $2l_0 = 7.8\text{nm}$ as the distance between the two wells for sample #3 (Supp.Table S2). It means that vortex hardly moves between the two positions. From that point of view the well presented in Fig.2a is overscaled with respect to the vortex size. Doesn't it require a comment? Is x_0 in the definition of the U_{pin} the same as $2l_0$?

7. It should be clearly mentioned that Fig.2. is an illustrative drawing of the postulated behavior. None of graphs presented there are measured experimentally or established by a simulation.

8. It is not clear how authors arrived to the estimation of the "energy penalty" equal to ~ 5 times Landauer's limit. You notice that too low writing energy cost makes the memory not very reliable. Do you mean that $5 \times$ Landauer limit is necessary to make the memory non-volatile. Please explain.

In my opinion the work is sufficiently original and interesting to appear in Nat. Comm., provided that authors present the additional experimental evidence, mentioned in the report, which will make their "energetic" investigations well-rooted in the convincingly tested experimental system. At the moment I have a feeling that authors did not make their best to give the full credibility to their interpretation and their presentation becomes at times too speculative.

Reviewer #3

(Remarks to the Author)

The authors have revised and improved their manuscript carefully considering all my comments and suggestions. They have

provided detailed and well-elaborated responses to my remarks in the first review report.

1. The authors have explicitly clarified a distinction of their continuous sinusoidal driving protocol from the single-shot erasure protocol considered by Landauer and emphasized in the refined Abstract that their numerical analysis demonstrates that the work required for such bit manipulation is within an order of magnitude of Landauer's limit.

2. The authors have convincingly explained that the fluctuations visible in the experimental arise from the finite signal-to-noise ratio of the measurement and become noticeable in the regime of low dissipation, where the measured signal approaches the noise floor of the SQUID-on-tip and the related electronics, and hence reflect instrumental noise rather than intrinsic vortex dynamics. At the same time, their Monte Carlo simulations do not include measurement noise and therefore produce smooth curves. The corresponding statement has been added in the revised manuscript.

3. The authors have performed an additional investigation of the issue that different values of the distance between the two minima are obtained when fitting the displacement and the work in double-well #3. Though the precise origin of this discrepancy is not still fully understood, I find well-grounded the statement of the authors that, as this behaviour is not detected in the six additional double-well systems presented in Supplementary Fig. S5, the discrepancy observed for double-well #3 is not representative of the general behaviour. The corresponding sentence is included in the revised manuscript.

4. The authors have included in the revision an exhaustive explanation of "another factor 2 related to the initial condition of the vortex" in the present protocol as compared to the Landauer's one.

5. Minor remarks concerning several grammatical errors and typos have been carefully taken into account in the revised manuscript.

The revised manuscript is suitable for publication in the Nature Communications in its present form.

Version 2:

Reviewer comments:

Reviewer #2

(Remarks to the Author)

Being a reviewer for Nature Communications I am asked by the editor whether the work supports the conclusions and claims, or additional evidence is needed. In my opinion the authors did not do their best to give the full credibility to their interpretation and their presentation lacks clear, simple and in my opinion accessible experimental evidence which makes the interpretation speculative. I suggested simple experiments which should be presented as a prerequisite for the conducted studies of the Landauer limit in the erasure of a single bit. My suggestions were ignored. Without the additional evidence the paper is not convincing and I do not recommend its publication.

Below I am just focusing on the two essential pieces of evidence which are lacking in the current manuscript. Both issues were described in my previous report. If the double well systems studied in the work behave as assumed by authors it should be no problem to present the simplest experiments that I proposed in my previous review:

1.
I asked for the experimental verification of hysteresis postulated in the toy model of Fig.2b. It would be very desirable to see experimental hysteresis instead of its conceptual illustration. Here, after applying the force and removing it you could take advantage of your displacement sensitivity of $dS_x \sim 0.3\text{nm}$ to resolve two distinct positions of the vortex. The hysteresis should be seen for double-wells #1 and #2 since they exhibit very low thermal hopping rates. Sample #1 shows displacement of 50nm (Fig.3d), by two orders of magnitude exceeding the quoted dS_x .

Authors response: "Regarding the proposed DC imaging experiment, we would like to clarify that the telegraphic-noise measurement and the DC imaging mode operate under very different experimental conditions. The telegraphic signal is acquired with the SOT held stationary above the vortex, making it sensitive to extremely small position changes. In contrast, DC imaging requires scanning the probe over more than a micron. In practice, scan distortions and drift of a few nanometers are unavoidable over the acquisition time of a complete image. As discussed in Ref. 13, this limits the effective spatial resolution of the DC imaging mode to approximately 20 nm, substantially worse than the displacement sensitivity required to image potential well #3 and #4, which are central to our work."

My answer: When applying a pulse of current to swap the vortex between the two minima you do not need to move the SOT. If the experiment works as authors describe, such imaging should be easily accessible. Why didn't authors present experimental hysteresis for sample #1 with SOT held stationary above the vortex? I understand that for samples approaching the Landauer limit such imaging would be more difficult but perhaps still possible with sufficient tilt in potential provided by the externally applied force which would suppress hopping. Anyway, experimental demonstration of hysteresis for larger wells is a necessary starting point to establish and validate experimentally postulated behavior of the double well systems. Then one can start analyzing smaller wells and see the limit when hysteresis disappears.

Why didn't authors demonstrate the ability to keep the vortex in one of the wells for the system for which they present telegraphic noise? It should be possible to capture the vortex in one of the wells as long as the force tilting the potential is applied.

The lack of such demonstrations makes me feel skeptical if the studied systems can be considered as memory elements. If it is so difficult to deliver the direct evidence (i.e. the experimental version of Fig.2b) then the title itself and the statement from the abstract -"These findings open possibilities for creating highly efficient memory elements (...)" - are overstated. It is also the issue which was raised by Reviewer #1.

2.
The thermal origin of the hopping mechanism is necessary to claim operation in the Landauer limit, since by itself it defines the necessary work to erase the bit as $k_B T \ln 2$. The thermal activation in the wells is not verified experimentally in the simplest way, namely, by varying temperature and observing whether it affects the hopping rate in telegraphic noise. It is a serious drawback of the presented work. I suggested that the activation process should be tested experimentally. Even if there are certain limitations, which you mention, you should prove that your activation mechanism somehow feels the temperature. Maybe you cannot recover the orthodox Kramers behavior from such analysis, but qualitatively you should see differences in the hopping rate for various temperatures. Particularly, you should see the difference in the telegraphic noise of Supp.Fig.2, when the temperature changes.

Authors write: "We thank the reviewer for raising this important point. We agree that temperature-dependent measurements are a common way to test activated dynamics in many systems. However, we respectfully disagree that such measurements are a necessary prerequisite for establishing the stochastic two-state dynamics reported here, nor would they provide an unambiguous test in the present superconducting vortex system. The reason is that temperature is not an independent control parameter for the activation energy of a superconducting vortex. Changing the temperature changes not only the thermal energy $k_B T$, but also superconducting parameters such as the coherence length ξ , and consequently the pinning potential itself. Therefore, a change in hopping rate with temperature would not isolate the effect of thermal fluctuations; it would also reflect a modified vortex potential landscape. To illustrate this point, consider the potential landscape used in our Monte Carlo simulation for double-well #3 with the parameter listed in Supplementary Table S1. Taking $T_c \approx 8$ K and using $\kappa(T) = \kappa(0) (1 - T/T_c)^{-1/2}$ a temperature change from 4.20 K to 4.21 K changes the coherence length by a factor $(4.21)/\xi(4.20) \approx 1.0013$. Although this change appears small, the corresponding modification of the pinning potential changes the calculated barrier from 1.20×10^{-21} J to 1.16×10^{-21} J in our model, which nearly doubles the hopping rate ν_k even while assuming that the thermal fluctuation did not change. Thus, even a very small temperature variation can strongly affect ν_k through changes in the potential landscape, not only through the Arrhenius factor. A temperature-dependent measurement would therefore not provide a clean verification of Kramers behavior in this system."

My response: I did not ask for verification of Kramers behavior. I wrote that you should prove qualitatively that your vortex somehow feels the temperature. Since the activation mechanism is sensitive not only to temperature but also to the potential landscape which is itself extremely sensitive to temperature you should see a very pronounced effect of small temperature variation in the telegraphic noise. Personally, I do not think that the telegraphic noise you present in Supplementary Figure 2 comes from thermal fluctuation: the hopping rate is there below 1Hz. Such low hopping rates show bistability of the system but their origin is perhaps a spurious noise, not thermal activation. You could easily show that I am not right by performing the additional experiment. It is the duty of experimentalists to convincingly present the data involving the results of sanity checks. Such additional checks aim at testing some other hypothesis which could possibly explain the behavior of the studied device in a different way than the one which was initially assumed. The tests that I propose are definitely within your reach. I am not asking about something infeasible.

The quoted temperature of 4.20K suggests that your system is thermally coupled to the bath of boiling He4. The boiling temperature depends on pressure. If you claim extreme sensitivity of the hopping rate on temperature, you should see it changing even from one day to another due to variation of atmospheric pressure. You can also pressurize the fridge in a trial to observe immediate changes in the hopping rate.

The last sentence from the authors' response- "A temperature-dependent measurement would therefore not provide a clean verification of Kramers behavior in this system"- is somewhat perplexing. Evaluating the system's response to temperature is fundamentally the only definitive experimental method to verify Kramers behavior. By not executing this measurement, the authors effectively acknowledge that they lack direct proof for their interpretation. If the system cannot be verified via temperature, then the authors' attribution of this specific mechanism remains circumstantial and has a character of a hidden assumption.

Authors write: "If the transitions were driven primarily by an external noise source originating from the measurement system, one would expect similar characteristic rates across different vortices. Instead, the hopping rates vary strongly from one vortex to another, indicating that they are governed by the local energy landscape."

My response: The first sentence is not true. An external noise has some power spectrum. Its effect on the particular vortex (hopping rate) will depend on the energy landscape and the position of the vortex in the strip (due to variation of the current across the strip). If the transitions were driven primarily by an external noise source originating from the measurement system, one would expect different characteristic rates across different vortices, depending on the local energy landscape and the position of the vortex in the strip.

In conclusion, the paper is missing the simplest possible explicit experimental demonstration of the double-well vortex system (lack of the experimental version of Fig.2b) and a direct simple proof that such system is governed by thermal activation mechanism (evidence that small variation of temperature changes also the hopping rate of the telegraphic noise in Supp.Fig.2). Without these experiments, the investigations conducted by the authors are circumstantial in nature and remain speculative. I am not convinced with explanations presented by authors: why to rely on models which are sometimes misleading especially when they are forced towards a desired interpretation, if the direct evidence is available. In my opinion such evidence is a necessary prerequisite to start further considerations. The point is that if the presented double-well systems work as claimed, these two demonstrations should be easily accessible experimentally. The paper is not ready to appear in Nature Communications.

Reviewer #3

(Remarks to the Author)

The second revised manuscript contains several useful refinements in response to the criticisms and recommendations of Reviewer #2. I would like to highlight the exceptionally conscientious and thorough work of Reviewer #2, aimed at bringing this manuscript to the highest level of excellence and soundness. At the same time, I would like to highlight the extremely thorough and in-depth responses provided by the authors, who set out their well-grounded position on all issues raised by Reviewer #2.

1. Reviewer #2 suggests to achieve direct experimental visualization of deterministic switching and hysteresis. The authors indicate the following. (i) The existence of a two-state system is supported by five independent experimental observations, which already now provide a confident basis for the present manuscript. (ii) The DC imaging experiment is highly challenging owing to scan distortions and drift of a few nanometers and the averaging over the presently available acquisition time of a DC image. Attempting to address these tasks would lead to an indefinite delay in publishing the obtained results and the advanced ideas of the present manuscript, which deserve to be made public already at the present stage.

2. Reviewer #2 asks whether the jumps in the telegraphic noise are of thermal or another, external nature ("spurious noise propagating in the Nb sample or in the SOT tip"). The authors provide a few pieces of experimental evidence and modelling results that observed switching dynamics is stochastic activation in the local double-well potential. Of primary importance for recommending acceptance of the manuscript in its current form is the authors' well-justified statement that even in the presence of an external stochastic source the key conclusions of the present work remain unchanged.

3. Reviewer #2 advises to experimentally demonstrate that the vortex is not pinned strongly and to qualitatively fix differences in the hopping rate for various temperatures. The authors underline that such measurements are a necessary prerequisite for establishing the stochastic two-state dynamics reported in the present manuscript. (i) The pinned-vortex scenario is inconsistent with several experimental observations. (ii) Temperature is not an independent control parameter for the activation energy of a vortex. Quite similarly to point 1 above, attempting to implement these suggestions would lead to an indefinite delay in publishing the obtained results and the advanced ideas of the present manuscript, which deserve to be made public already at the present stage.

4. Reviewer #2 has observed an inconsistency in the presentation of the quoted values of r_k . The authors have clarified this issue in the second revised manuscript.

5. Reviewer #2 asks, how does the distribution of the sheet current density change with frequency. The authors have provided a graph of the current distribution as obtained by measuring the magnetic field induced by the circulating current across the superconducting bridge, implying no significant change in the distribution at all the driving frequencies.

6. Reviewer #2 expresses his/her concern about the physical consequences of fact that the displacement between the two minima is very small and asks to clarify two related characteristic distances, x_0 and ℓ_0 . The authors reply that their lock-in technique allows them to resolve vortex displacements which are much smaller than the apparent vortex width in DC magnetic imaging and clarified the distinction between x_0 and ℓ_0 in the revised manuscript.

7. Reviewer #2 suggests to clearly mention that Fig.2. is an illustrative drawing. The authors have accepted this suggestion both in the caption to Fig. 2 and in the main text in the revised manuscript.

8. Reviewer #2 asks to clarify the estimation of the "energy penalty". The authors have provided an explanation of how they arrived at their estimate of the "energy penalty" to work with a useful non-volatile memory.

Confirming my previous recommendation, I like to conclude that the manuscript is original, scientifically sound and well-substantiated to be published in "Nature Communications" in its current form. I am convinced that the comments and recommendations of Reviewer #2 will be invaluable for the authors' future research.

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Reviewer #1 Remarks to the Author:

The authors present a method to measure the pinning potential of a vortex in a superconducting thin film by imaging vortex vibrations induced by an AC drive current and extracting the work associated with vortex motion from the vibration amplitude and direction. The study is potentially suitable for publication in *Nature Communications* after the following issues are addressed.

We thank the reviewer for the careful reading of our manuscript and for the constructive and insightful comments. We appreciate the reviewer's clear summary of our work and are encouraged by the assessment that it is potentially suitable for publication in *Nature Communications*. We believe that the comments have helped us to significantly improve the clarity and rigor of the manuscript, and we have carefully addressed all the points raised below.

1. Which aspects of the methodology are genuinely novel? Vortex pinning and dynamics have previously been studied using SQUID-based techniques (for example Refs. 10 and 17), and the calculation of work appears to be a direct extension of earlier approaches. The authors should distinguish their contribution from prior work and clarify what new capability or insight their method provides.

We thank the reviewer for raising this important point regarding the novelty of our work.

The main novelty of the present work is the ability to *quantitatively* extract the work (and associated dissipation) at the level of a single vortex under periodic drive. This enables, for the first time in the context of vortices, a direct and quantitative comparison with theoretical descriptions of driven bistable systems and with fundamental limits such as the Landauer bound.

Previous studies have addressed aspects of vortex dynamics using SQUID-based techniques. In particular, Ref. 10 investigates vortex motion in a very different regime of continuous high-velocity flux flow (~ 10 km/s), where the signal reflects an average over a large number of rapidly moving vortices, and dissipation is macroscopic and accessible via transport measurements. The measured global dissipated power was estimated to ~ 400 μ W, which is approximately 11 orders of magnitude larger than in the present study. In contrast, our work focuses on the controlled manipulation on the nanoscale and measurement of a single-vortex in a bistable potential in the low-dissipation regime.

Ref. 17 employs a similar experimental approach to probe single-vortex motion within a pinning potential. However, that study is limited to intra-well dynamics and does not address dissipation or work extraction. Other works (Refs. 13, 14, 19) have explored related phenomena, but do not provide a quantitative determination of the work, either due to incomplete analysis or lack of access to the force acting on the vortex.

To better reflect this context, we have revised the corresponding statement in the Introduction to better reflect prior works.

"This work introduces a new approach to quantifying the dissipation of individual vortices under periodic drive."

to:

"This work builds on previous studies^{13,14,17} to provide a quantitative measure of the dissipation of individual vortices under periodic drive."

2. In the abstract, the authors state that “at high frequencies, the work required for bit manipulation (erasure) was shown to be within an order of magnitude of Landauer’s limit ($k_B T \ln 2$) at 4.2 K.” However, as the authors note, the experimental protocol does not implement an erasure operation in the information-theoretic sense. The authors should discuss whether a true erasure protocol could be realized experimentally, especially in the low-frequency regime where the simulations in Fig. 5 suggest that the Landauer limit can be approached. If such a regime is not experimentally accessible, the manuscript should explain which experimental parameters would need to be tuned to reach it and comment on the implications for practical vortex-based memory devices. In particular, it would be useful to discuss whether the low-frequency limit can be pushed into a technologically relevant operating range.

We thank the reviewer for highlighting this point, which was not sufficiently clear in the original manuscript.

In principle, a true erasure protocol could be implemented experimentally in our double-well vortex system by preparing the vortex in a random initial state and then erasing the bit by tilting the potential in a chosen direction. However, such a single-shot protocol does not provide direct access to the dissipation in our measurement scheme, which relies on a steady sinusoidal drive and Fourier analysis of the response. Therefore, in the present work, we do not directly measure the dissipation of an erasure operation, but instead use our experimentally constrained model and simulations to estimate the energy cost that would be associated with such a protocol.

We have added the following statement to the end of the second paragraph of the Discussion:

“We emphasize that a true erasure protocol could in principle be implemented with the double-well potentials presented here, but our present measurement scheme would not allow direct measurement of the resulting dissipation.”

The low-frequency regime ($\Omega \ll r_k$) is fundamentally unsuitable for memory applications. Under such conditions, the vortex undergoes frequent thermally activated transitions between the two states, preventing reliable information storage. A functional memory element must therefore operate in the opposite regime, $\Omega \gg r_k$, where the stored state remains stable on the timescale of operation.

In the high-frequency regime and for sufficiently strong drive, our analysis shows that a functional memory element based on a superconducting vortex would require an energy cost of about 5 times Landauer’s limit at $\Omega/2\pi = 150$ kHz.

In response to the reviewer’s comment, we clarified this point in the Discussion by adding:

“Notably, in such a regime, the vortex cannot serve as a memory bit since it cannot retain its information on that timescale. Therefore, in our case, a memory bit requires an energy penalty of ~ 5 times Landauer’s limit at $\Omega/2\pi = 150$ kHz.”

We also modified the abstract to emphasize what is actually measured by adding the following phrase.

“Although, our experimental method requires a steady sinusoidal drive incompatible with Landauer’s erasure protocol, our numerical analysis demonstrate that the work required for such bit manipulation (erasure) is within an order of magnitude of Landauer’s limit ($k_B T \ln 2$) at 4.2 K. “

3. In Fig. 1d,e, the driving force acts along the x-axis, yet the vortex displacement occurs at 45 degrees. The authors should explain the origin of this directional response. It would also be useful to comment on whether a lower force would be required if the current were applied perpendicular to the observed displacement direction.

We thank the reviewer for pointing out that the physical origin of the angle θ between the applied force and the vortex displacement was not discussed.

This angle arises from the anisotropy of the local pinning potential experienced by the vortex. In general, the restoring force is determined by the local curvature of the pinning potential landscape, which need not be isotropic. As a result, the vortex tends to move along the direction of lowest effective stiffness, which is not necessarily aligned with the applied driving force. In the given example, this leads to a displacement direction that is tilted by approximately 45° with respect to the applied force, reflecting the anisotropic structure of the local pinning landscape. In our experience, the vortex motion is never fully aligned with the driving force.

Regarding the reviewer's question, applying the driving force along the direction of vortex motion (i.e., along the effective "soft" axis of the potential) would, in principle, reduce the force required to induce displacement or barrier crossing. However, this would not change the work per cycle, since the work is given by $W = \oint \mathbf{F}(t) \cdot d\mathbf{l}$, which already accounts for the projection of the force along the direction of motion.

In our experimental geometry, the current direction is fixed by the device design, and the vortex explores different local pinning configurations within this constraint.

Such behavior has been reported previously in vortex dynamics in anisotropic pinning landscapes (see, e.g., Ref. 17 for experimental observations and Ref. 18 for theoretical description). We have clarified this point in the revised manuscript as follows:

"The angle between the applied force and the vortex motion arises from the anisotropy of the pinning potential and has been discussed in prior works^{17,18}."

4. The procedure used to extract a quantitative displacement length, l , from the images is not sufficiently clear. In addition, what sets the detection threshold for l ? It is not clear whether the limiting factor is SQUID noise, mechanical vibrations, or another source.

We thank the reviewer for pointing out this omission.

The quantities $B_{\Omega}^{ip}(\mathbf{r})$ and $B_{\Omega}^{oop}(\mathbf{r})$ are obtained using a lock-in amplifier, which measures the first harmonic modulation of the local magnetic field at the drive frequency. These correspond respectively to the in-phase and out-of-phase components of the magnetic first harmonic, respectively.

The inference of the vortex displacement from these measurements relies on the fact that the SOT probe measures the local magnetic field generated by the vortex, which has a well-defined magnetic spatial profile $B_v(l_0 - l(t))$, where l_0 is the equilibrium position of the vortex and $l(t)$ is the time-dependent position with respect to that position. For small oscillations of the vortex position $l(t)$, the resulting modulation of the magnetic field at a fixed probe position can be linearized as:

$$\delta B(r, t) = \nabla B_v \cdot \delta \mathbf{I}(t)$$

In our measurement, we are interested in the first harmonic, so this equation linearization translates explicitly into these two equations that we fit independently

$$B_{\Omega}^{ip}(\mathbf{r}) = x_{\Omega}^{ip} \frac{\partial B_{DC}}{\partial x} + y_{\Omega}^{ip} \frac{\partial B_{DC}}{\partial y}$$

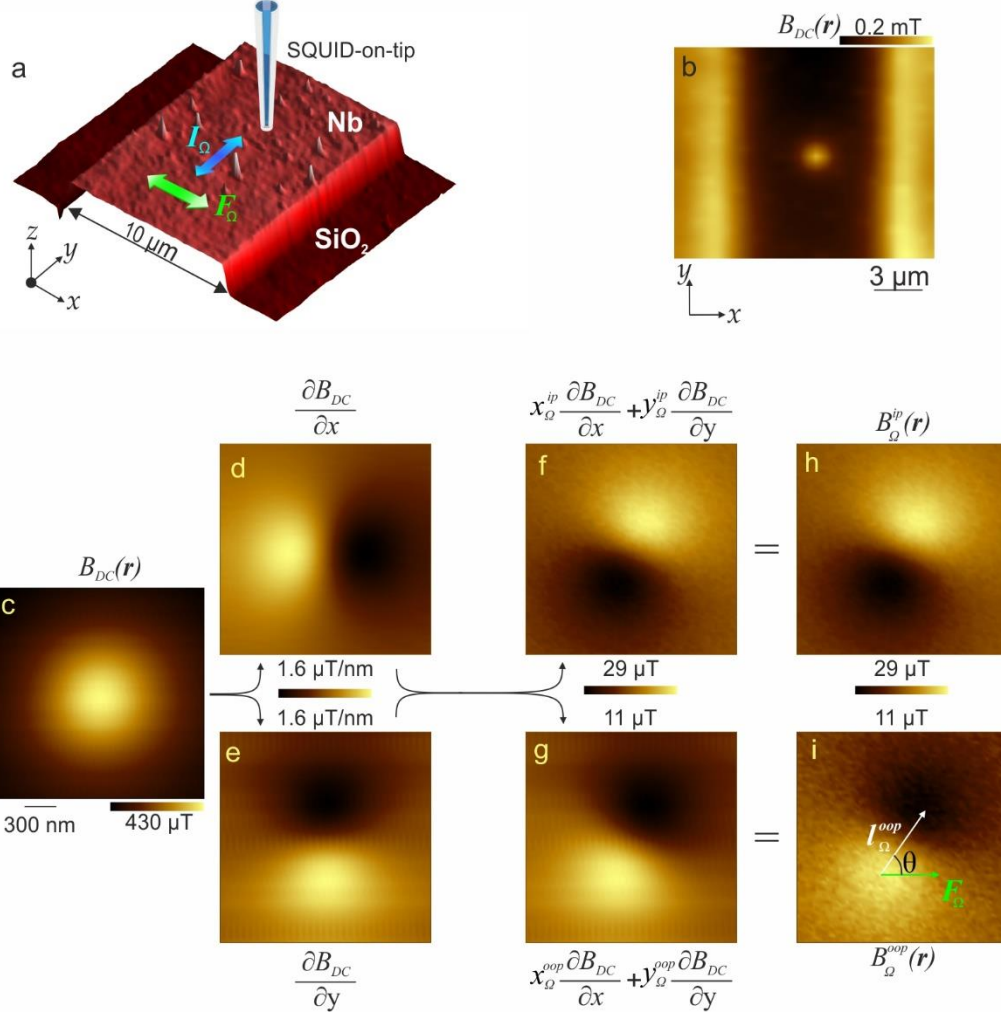
$$B_{\Omega}^{oop}(\mathbf{r}) = x_{\Omega}^{oop} \frac{\partial B_{DC}}{\partial x} + y_{\Omega}^{oop} \frac{\partial B_{DC}}{\partial y}$$

We measure $B_{\Omega}^{ip}(\mathbf{r})$, $B_{\Omega}^{oop}(\mathbf{r})$ and $B_{DC}(\mathbf{r})$ and $x_{\Omega}^{ip,oop}$, $y_{\Omega}^{ip,oop}$ are fitting parameters.

Regarding the minimum detectable displacement is limited by the SQUID noise and it can be estimated as follows:

$$\Delta S_{l_{\Omega}} = \frac{\Delta S_{B_{\Omega}}}{\max\left(\frac{dB_{DC}}{dx}\right)} \approx 0.3 \text{ nm}$$

In the revised manuscript, we have clarified this point in the main text and expanded the explanation (previously in Supplementary Note 4) to make the procedure fully transparent. In addition, we have reorganized Figure 1 into two figures to better illustrate the measurement protocol and its interpretation. See, below the relevant text and figure.



“The periodic force displaces the vortex away from its equilibrium position, resulting in a time-dependent magnetic field, $B(\mathbf{r}, t)$. Our methodology relies on measuring the Fourier components of $B(\mathbf{r}, t)$ with a lock-in amplifier. In general, $B(\mathbf{r}, t)$ can be expressed as $B_{\Omega}(\mathbf{r}, t) = \sum_{n=1}^{\infty} B_{n\Omega}^{ip}(\mathbf{r}) \cos(n\Omega t) + B_{n\Omega}^{oop}(\mathbf{r}) \sin(n\Omega t)$, where $B_{n\Omega}^{ip}(\mathbf{r})$ and $B_{n\Omega}^{oop}(\mathbf{r})$ represent the magnetic field amplitude modulation of the n -th harmonic that is in-phase (ip) and out-of-phase (oop) with respect to the phase of the driving current I_{Ω} , and $\mathbf{r}$ is the coordinate of the SOT. The reference phase is set so that the magnetic field caused by the current, according to the Biot-Savart law, is detected solely in the ip signal (see Supplementary Note 3 and Supplementary Figure S1). The scanning SOT microscope simultaneously measures the spatial distributions of the DC magnetic field $B_{DC}(\mathbf{r})$ (Figure 1c) and of the 1st harmonic AC field $B_{\Omega}^{ip}(\mathbf{r})$ and $B_{\Omega}^{oop}(\mathbf{r})$ (Figure 1h,i). For small displacements of the vortex, the AC and DC field images are related by

$$B_{\Omega}^{ip}(\mathbf{r}) = x_{\Omega}^{ip} \frac{\partial B_{DC}}{\partial x} + y_{\Omega}^{ip} \frac{\partial B_{DC}}{\partial y}$$

, where x_{Ω}^{ip} and y_{Ω}^{ip} are displacement amplitude at frequency Ω , in-phase with the applied force, along the x and y axis, respectively. Using this relation, we measure the ip and oop displacement amplitude independently as illustrated in Figure 1c-i. The very high sensitivity of the SOT allows

us to measure sub-nm displacements.¹⁷ The displacement sensitivity is roughly estimated by dividing the SOT and RMS electronic noise background in a typical $\Delta S_{B_\Omega} \sim 250$ nT by the maximum vortex DC field gradient $\max \frac{\partial B_{DC}}{\partial x} = 0.8$ $\mu\text{T}/\text{nm}$, which give a displacement sensitivity of $\Delta S_{x_\Omega} \sim 0.3$ nm.

For the rest of this article, we will use polar coordinates $l_\Omega^{ip} = \sqrt{(x_\Omega^{ip})^2 + (y_\Omega^{ip})^2}$, and θ is the angle with respect to the Lorentz force as depicted in Figure 1i. The angle between the applied force and the vortex motion arises from the anisotropy of the pinning potential and has been discussed in prior works^{17,18}.

We calculate the work exerted by the Lorentz force on the vortex $W = \oint \vec{F} \cdot d\vec{l} = \cos \theta \int_0^{2\pi/\Omega} F_\Omega \cos \Omega t \frac{dl}{dt} dt$, where the vortex position is expressed in terms of its Fourier components:

$l(t) = \sum_{n=1}^{\infty} l_{n\Omega}^{ip} \cos n\Omega t + l_{n\Omega}^{oop} \sin n\Omega t$. Substituting $\frac{dl}{dt}$ into the integral, all the terms of the Fourier series vanish except the term in $l_{n\Omega}^{oop}$, which results in $W = \pi \cos \theta F_\Omega l_{n\Omega}^{oop}$. Given that the average temperature is constant in time, the work exerted on the vortex is equal to the energy dissipated by the vortex."

5. I am sure the authors have considered this. But they should address the possibility that the applied current induces mechanical oscillations of the probe or tip that could produce a similar signal.

We thank the reviewer for highlighting this potential artifact. Before quantitatively demonstrating that this effect is much smaller than the measured vibration, we first provide several qualitative arguments that rule out such an artifact.

First, the tip and the vortex mechanical response differ vastly. The tip is expected to behave like a harmonic oscillator with relatively high-quality factor. For example, we know that our quartz tuning fork that is cooled down in the same conditions of vacuum and temperature have a quality factor larger than 10^4 . We should then observe a relatively sharp peak around the resonance frequency. In our frequency range, the induced motion should vanish less than a Hz away from the resonance. The vortex on the other hand behaves like a highly overdamped particle. At our frequency, we can neglect its mass and assume that the vortex is always at the point where the applied force matches in magnitude the restoring force. For that reason, finite motion is detected at frequencies far from its resonance. Second, the tip should behave like a harmonic oscillator (intra-well behavior) and not a two-level system, as observed in our measurements. Third, vortex behave very differently depending on their potential landscape. See below an example where we have a few vortices in the field of view. It is clear that some vortices oscillate with different amplitude, including some vortices with vanishing $B_\Omega^{oop}(\mathbf{r})$. If the signal were induced by tip vibration, the mechanical response would always be identical. Finally, the applied force on the tip will be oriented along the z and x axis (see Figure below). An oscillation along the z axis would not produce a $B_\Omega(\mathbf{r})$ image that resemble what is observed. Instead, it would be shaped like a vortex (bell shaped) with smaller signal. We never observed such an AC signal. We also never saw a vibration purely along the x axis.

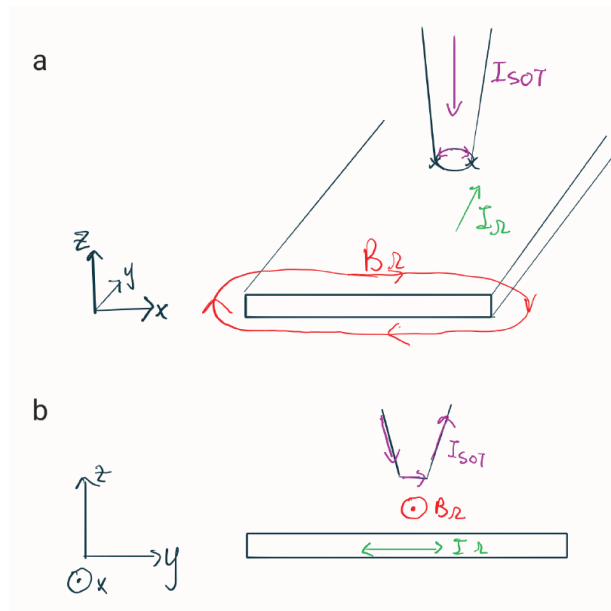
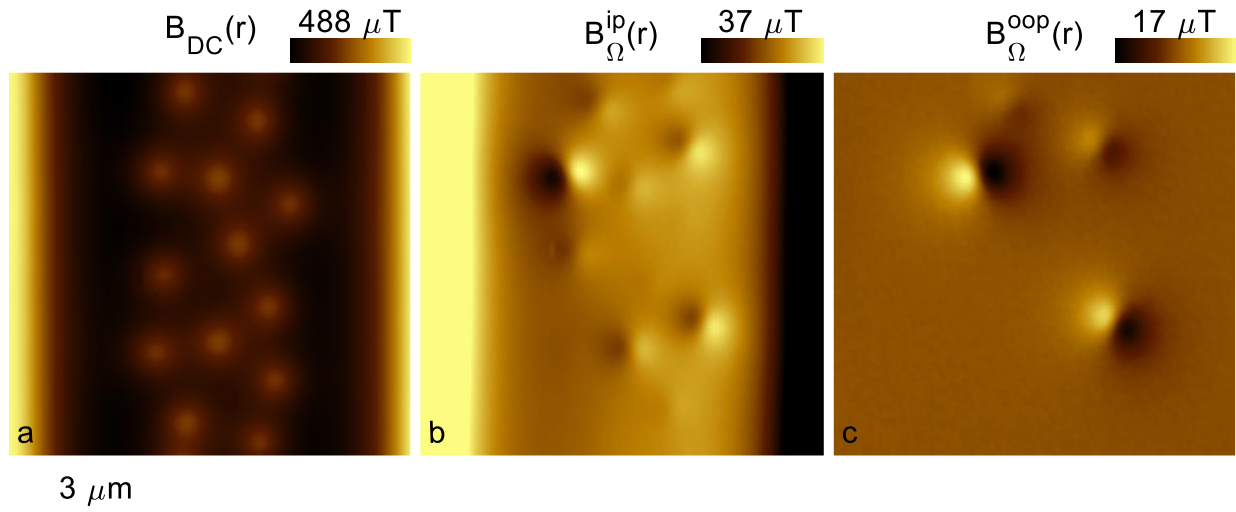


Figure | Schematic drawing of the SQUID-on-tip near the superconducting bridge. (a) 3D perspective of the squid near the bridge. The red color represents the stray field caused by the running currents. Green represents the current driving the vortex. The purple color represents the current running through the SQUID-on-tip. (b) Same as a but from another perspective.

For the largest applied current (1.2 mA), the magnetic field at a height of 500 nm above the center of the bridge is on the order of $\sim 50 \mu\text{T}$, as experimentally measured values (Fig. S1). In the center of the bridge, the field is predominantly oriented along the x direction, with a component along z that is roughly an order of magnitude smaller (Fig. S1). The resulting Lorentz force $F = I_{SOT}LB = 3 \text{ fN}$, where L is the length of the SQUID arm ($\approx 250 \text{ nm}$ tip diameter), $I_{SOT} = 300 \mu\text{A}$ is the current flowing through the tip. Most of this force is directed along the z direction but $\sim 10 \%$ is along the x direction.

We now calculate the tip displacement under this load. The tip geometry consists of a cone with varying wall thickness. At its base, the tip has an inner diameter of 0.5 mm and an outer diameter of 1 mm. At the end of the tip, the inner diameter 200 nm and the outer diameter 300 nm. The tapered part of the tip measures $L = 1 \text{ cm}$. For simplicity, let's consider the worst-case scenario with a constant wall thickness of 50 nm. For a load at the end of the tip and a fixed base the strain energy is written as follows:

$$U = \int_0^L \frac{M^2}{2EI} dx$$

Where M is the bending moment, E is Young's modulus and I is the second moment area. For a thin cone with a force perpendicular to its axis, $I \approx \pi t r^3$, where t is the wall thickness. We first consider the force is applied at the end of the tip along the x axis, thus $M = Fx$, where F is the applied force (estimated above). We now use Castigliano's theorem which says that the beam bending in the direction of the applied force F is $\delta = \partial U / \partial F$. Taking the derivative of the above expression, we find:

$$\delta = \frac{\partial U}{\partial F} = \int_0^L \frac{Fx^2}{EI(x)} dx = \int_0^L \frac{Fx^2}{E\pi t(r(x))^3} dx$$

$r(x)$ can be written explicitly: $r(x) = r_t + \frac{(r_b - r_t)x}{L}$, where r_b is the radius of the tip base and r_t is the radius of the tip. The integral is solvable analytically:

$$\delta = \frac{FL^3}{2\pi E t r_b^3} \frac{2 \ln(r_b/r_t) - 3 + 4(r_b/r_t) - (r_b/r_t)^2}{(1 - r_b/r_t)^3}$$

Given that $r_b = 2.5 \times 10^{-4} \text{ m}$ and $r_t = 1.0 \times 10^{-7} \text{ m}$, it is a good approximation to write

$$\delta \approx \frac{FL^3(2 \ln(r_b/r_t) - 3)}{2\pi E t r_b^3} \approx 8.0 \times 10^{-14} \text{ m}$$

Considering $F = 0.3 \text{ fN}$, $E = 90 \text{ GPa}$, and $t = 50 \text{ nm}$. We note that this is roughly four orders of magnitude smaller than the measured displacement $l_{\Omega}^{ip,oop} \sim 1 \times 10^{-9} \text{ m}$. Moreover, this simplified model is a higher bound given that we considered that the thickness of the glass is constant (50 nm) for the entire length. Notably, considering a large quality factor ($>10,000$), one could, in principle see motion of the order of 1 nm. However, for high quality factor the amplitude of motion would decay quickly a few Hz off-resonance.

The spring constant is $k = \frac{F}{\delta} = \frac{2\pi E t r_b^3}{L^3(2 \ln(r_b/r_t) - 3)} = 4 \times 10^{-2}$ N/m. This is at least ~ 200 times stiffer than the stiffest vortex measured (see Supplementary Figure S3). Considering the mass of such oscillator, $m \approx \frac{\rho \pi L}{3} ((r_b + t)^2 - (r_b)^2) \approx 7 \times 10^{-10}$ kg, and considering the density of glass $\rho = 2.5 \times 10^3$ kg/m³. We can crudely estimate the resonance frequency $f_0 = 1/2\pi \sqrt{\frac{k}{m}} = 1.2$ kHz. Such resonance was never observed. It could be because we never swept the frequency with sub-Hz resolution or because the tip is stiffer and we cannot detect the motion even on resonance.

We now turn to the out-of-plane force (z axis), which would cause a compressive stress of the tip. There we can allow ourselves to further simplify the problem by considering a tube of 200 nm inner diameter with wall thickness 50 nm. This is quite a crude approximation which gives a much higher bound (by about a factor 100). However for simplicity we keep this model. There, it is known that

$$\delta_{\perp} = \frac{FL}{AE} \approx 8.5 \times 10^{-15} \text{ m}$$

Considering the cross-section to be $A = \pi(r_o^2 - r_i^2) \approx 4 \times 10^{-14}$ m², the applied compressive force $F = 3 \times 10^{-15}$ N. Here the displacement for a static force is even smaller, the spring constant is higher $k = 4 \times 10^{-1}$ N/m. $f_0 \approx 100$ kHz, which was also never observed.

We therefore conclude that this artifact cannot affect our measurements.

6. In Fig. S2, the noise within each level of the telegraph signal is several microtesla, which appears large compared with similar SQUID sensors (for example Nat. Nano. 8, 639–644 (2013)). The authors should comment on whether this reflects excess measurement noise or unresolved small-scale vortex motion. Showing comparable data acquired far from the vortex would provide a useful baseline for comparison.

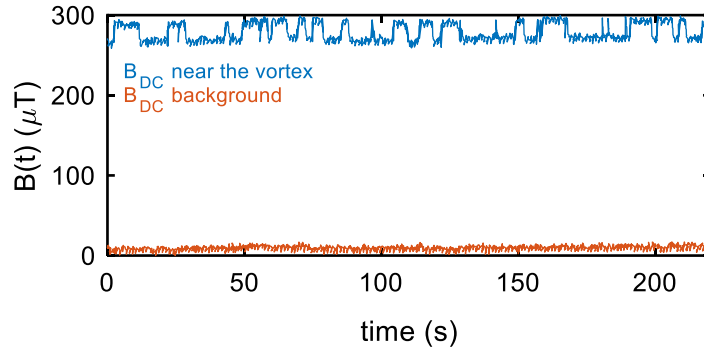
We thank the reviewer for this relevant comment.

Apart from the telegraphic noise, the noise observed in Fig. S2 originates from the intrinsic noise of the SQUID-on-tip and its readout electronics. As is typical for SQUID sensors, the noise spectrum consists of a low-frequency $1/f$ component and a white noise component at higher frequencies, with a crossover typically occurring around the kHz range.

For comparison, in Ref. [Nat. Nano. 8, 639–644 (2013)], the flux noise at 1 Hz, for a large SQUID loop of 160 nm diameter SQUID loop, is reported $\sim 10 \mu\Phi_0/\sqrt{\text{Hz}}$. Converting this to field noise using the effective sensor area yields values of $1 \mu\text{T}/\sqrt{\text{Hz}}$, which is of the same order of magnitude than the noise level observed in Fig. S2. We note that the values reported in that work correspond to optimal operating conditions, whereas in our experiment the SQUID is operated at very low magnetic fields, where the sensitivity is typically reduced.

Importantly, this noise does not translate into additional vortex motion. This background noise is position independent and does not grow where the vortex gradient is large. As discussed in our response to comment 4, the vortex displacement is extracted from the phase-coherent first harmonic component of the signal, compared to the gradient of the DC signal. Thus, random noise only affects the measurement sensitivity, not the measured displacement itself.

Following the reviewer's suggestion, we have added a reference time trace acquired with the tip positioned approximately 1 μm away from the vortex (see below). This baseline measurement, now included in Supplementary Fig. S2, shows a comparable background noise without the telegraphic noise, confirming that the telegraphic noise is really due to the vortex dynamics.



7. In Fig. S4, the x-axis lacks numerical labels and should be corrected.

We thank the reviewer pointing out this omission. The numerical labels were added.

Reviewer #2 Remarks to the Author:

The paper advertises in the abstract experimental manipulation of a single superconducting vortex in the Landauer limit of the work necessary to change its position in a bistable potential. In principle, such a demonstration would represent a scientifically robust case as it touches the heart of thermodynamics and its intimate connection with the information theory. However, I do not find convincing experimental data in the presentation which would support the promise of the abstract. The paper lacks thorough and convincing experimental support.

It is important to distinguish in the current presentation experimental facts from model- and parameter-dependent evaluations. The experimental fact is that authors can observe a single vortex, as documented by the SOT images (Fig.1) and its bistability revealed in measurement of telegraphic noise (albeit with dynamics well below 1Hz). While this part of presentation belongs to the state-of-the art in the field, the calculation of work done on vortex is strongly model- and parameter-dependent. I am missing important experimental data, which seem essential to defend the way of reasoning presented by authors.

The current manuscript shows the typical drawback for works published in the literature on the Landauer limit: it does not verify the Landauer prediction directly by measuring the dissipation associated with the manipulation of a single bit. Instead these works rely on models and assumptions, which may push the interpretation towards the desired result. At the moment direct measurement of the Landauer limit seems to remain a formidable task, but such direct verification is necessary for unambiguous confirmation of the theory.

I do not agree with the statement that “this work introduces a new approach to quantifying the dissipation of individual vortices under periodic drive”. I would say it proposes a qualitative viewpoint on the vortex dissipation, but since the proposed picture relies on particular models (not verified!), assumptions and not-well known parameters it cannot be expected to provide a reliable quantification for the vortex dissipation. As authors wrote in the caption of Fig.1, their modelling is based on a toy model. Toy models are expected to create a framework within which it is easier to consider a given physical process, but taken alone they hardly predict proper numbers, even in cases when they are qualitatively correct.

To publish a paper in Nature Comm. I guess that the description should be self-contained. Authors refer to previous works trying to make shortcuts in their presentation to arrive at final conclusions. It unfortunately kills the flow of narration and creates feeling, that authors rely more on literature than on their experiment. Below I am providing the concrete comments referring to specific sections of the paper.

We thank the reviewer for the constructive and insightful comments. We agree that the distinction between directly measured quantities and model-dependent interpretation is essential, and we recognize that this was not sufficiently clear in the original manuscript. We have substantially revised the presentation to clarify this point.

In particular, we emphasize that the in-phase and out-of-phase displacement components are obtained directly from SOT images, without relying on any specific model. The work is then calculated using conventional Fourier analysis, without relying on a specific model. The curves shown in the revised version Figures 3–5 correspond to these experimentally measured quantities unless labeled as “simulation”.

We acknowledge that our original submission may have suggested a stronger reliance on modeling than is actually the case. In the revised manuscript we included a revised version of the method and added a Figure to better illustrate the method.

Finally, we clarify that the models used (stochastic resonance and Monte Carlo) are not employed to extract the experimental quantities, but only to interpret the observed behavior. We have revised the manuscript to make this distinction explicit and improve the overall clarity and self-contained nature of the presentation.

Major allegations:

1. It is not clear what images 1d and 1e present and how the B-signal is measured. Is it a Lock-In measurement of the first harmonics for the in-phase and out-of-phase value of B respectively? How can you infer the position of the vortex from such images?

We thank the reviewer for pointing out that the description of Figs. 1d–e was not sufficiently clear.

The quantities $B_{\Omega}^{ip}(\mathbf{r})$ and $B_{\Omega}^{oop}(\mathbf{r})$ are indeed obtained using a lock-in amplifier, which measures the first harmonic response of the local magnetic field at the drive frequency. These correspond respectively to the in-phase and out-of-phase components of the magnetic signal.

The inference of the vortex displacement from these measurements relies on the fact that the SOT probe measures the local magnetic field generated by the vortex, which has a well-defined spatial profile $B_v(l_0 - l(t))$, where l_0 is the equilibrium position of the vortex and $l(t)$ is the time-dependent position with respect to that equilibrium. For small oscillations of the vortex position $l(t)$, the resulting modulation of the magnetic field at a fixed probe position can be linearized as:

$$\delta B(\mathbf{r}, t) = \nabla B_v \cdot \delta \mathbf{l}(t)$$

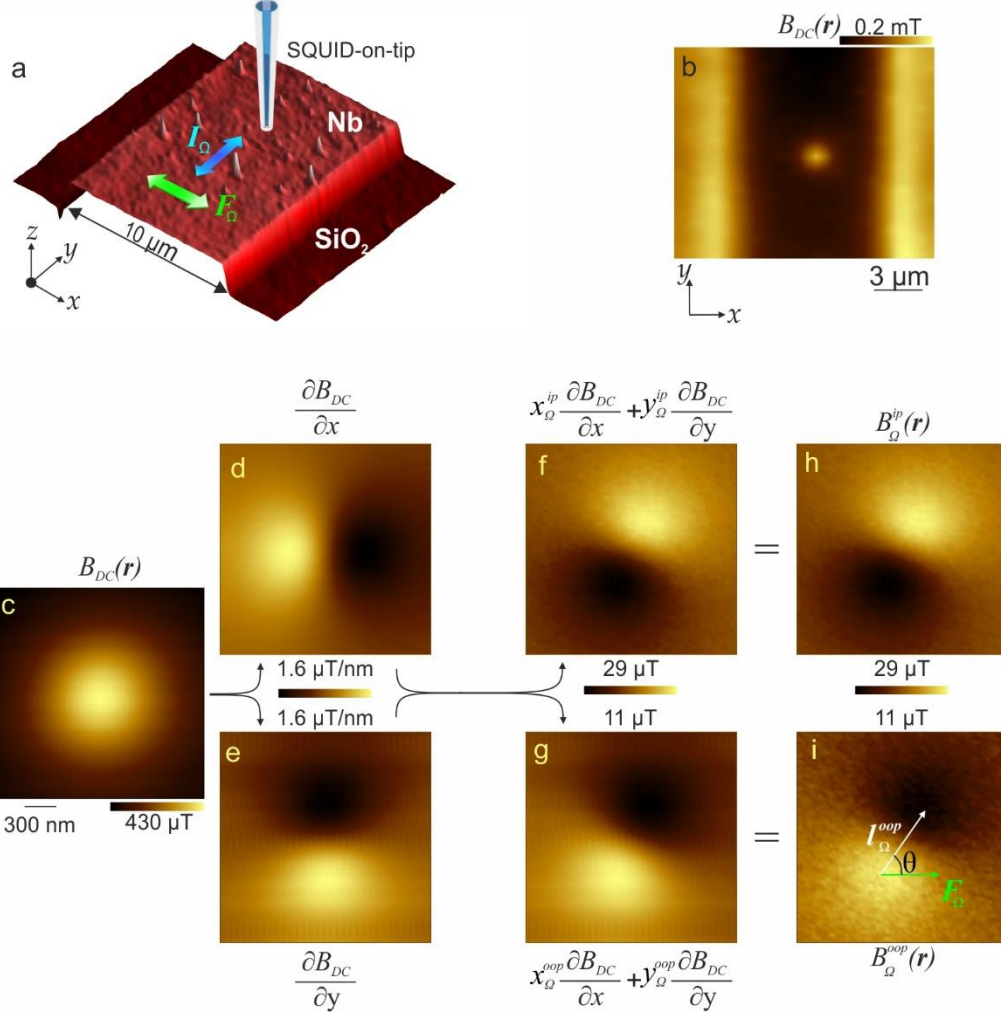
In our measurement, we are interested in the first harmonic, so this equation linearization translates explicitly into these two equations that we solve independently

$$B_{\Omega}^{ip}(\mathbf{r}) = x_{\Omega}^{ip} \frac{\partial B_{DC}(\mathbf{r})}{\partial x} + y_{\Omega}^{ip} \frac{\partial B_{DC}(\mathbf{r})}{\partial y}$$

$$B_{\Omega}^{oop}(\mathbf{r}) = x_{\Omega}^{oop} \frac{\partial B_{DC}(\mathbf{r})}{\partial x} + y_{\Omega}^{oop} \frac{\partial B_{DC}(\mathbf{r})}{\partial y}$$

We measure $B_{\Omega}^{ip}(\mathbf{r})$, $B_{\Omega}^{oop}(\mathbf{r})$ and $B_{DC}(\mathbf{r})$ and $x_{\Omega}^{ip,oop}$, $y_{\Omega}^{ip,oop}$ are considered as fitting parameters.

In the revised manuscript, we have clarified this point in the main text and expanded the explanation (previously in Supplementary Note 4) to make the procedure fully transparent. In addition, we have reorganized Fig. 1 into two figures to better illustrate the measurement protocol and its interpretation. See, below the relevant text.



“The periodic force displaces the vortex away from its equilibrium position, resulting in a time-dependent magnetic field, $B(\mathbf{r}, t)$. Our methodology relies on measuring the Fourier components of $B(\mathbf{r}, t)$ with a lock-in amplifier. In general, $B(\mathbf{r}, t)$ can be expressed as $B_{\Omega}(\mathbf{r}, t) = \sum_{n=1}^{\infty} B_{n\Omega}^{ip}(\mathbf{r}) \cos(n\Omega t) + B_{n\Omega}^{oop}(\mathbf{r}) \sin(n\Omega t)$, where $B_{n\Omega}^{ip}(\mathbf{r})$ and $B_{n\Omega}^{oop}(\mathbf{r})$ represent the magnetic field amplitude modulation of the n -th harmonic that is in-phase (ip) and out-of-phase (oop) with respect to the phase of the driving current I_{Ω} , and $\mathbf{r}$ is the coordinate of the SOT. The reference phase is set so that the magnetic field caused by the current, according to the Biot-Savart law, is detected solely in the ip signal (see Supplementary Note 3 and Supplementary Figure S1). The scanning SOT microscope simultaneously measures the spatial distributions of the DC magnetic field $B_{DC}(\mathbf{r})$ (Figure 1c) and of the 1st harmonic AC field $B_{\Omega}^{ip}(\mathbf{r})$ and $B_{\Omega}^{oop}(\mathbf{r})$ (Figure 1h,i). For small displacements of the vortex, the AC and DC field images are related by

$$B_{\Omega}^{ip}(\mathbf{r}) = x_{\Omega}^{ip} \frac{\partial B_{DC}}{\partial x} + y_{\Omega}^{ip} \frac{\partial B_{DC}}{\partial y}$$

, where x_{Ω}^{ip} and y_{Ω}^{ip} are displacement amplitude at frequency Ω , in-phase with the applied force, along the x and y axis, respectively. Using this relation, we measure the ip and oop displacement amplitude independently as illustrated in Figure 1c-i. The very high sensitivity of the SOT allows

us to measure sub-nm displacements.¹⁷ The displacement sensitivity is roughly estimated by dividing the SOT and RMS electronic noise background in a typical $\Delta S_{B_\Omega} \sim 250$ nT by the maximum vortex DC field gradient $\max \frac{\partial B_{DC}}{\partial x} = 0.8$ $\mu\text{T}/\text{nm}$, which give a displacement sensitivity of $\Delta S_{x_\Omega} \sim 0.3$ nm.

For the rest of this article, we will use polar coordinates $l_\Omega^{ip} = \sqrt{(x_\Omega^{ip})^2 + (y_\Omega^{ip})^2}$, and θ is the angle with respect to the Lorentz force as depicted in Figure 1i. The angle between the applied force and the vortex motion arises from the anisotropy of the pinning potential and has been discussed in prior works^{17,18}.

We calculate the work exerted by the Lorentz force on the vortex $W = \oint \vec{F} \cdot d\vec{l} = \cos \theta \int_0^{2\pi/\Omega} F_\Omega \cos \Omega t \frac{dl}{dt} dt$, where the vortex position is expressed in terms of its Fourier components: $l(t) = \sum_{n=1}^{\infty} l_{n\Omega}^{ip} \cos n\Omega t + l_{n\Omega}^{oop} \sin n\Omega t$. Substituting $\frac{dl}{dt}$ into the integral, all the terms of the Fourier series vanish except the term in l_Ω^{oop} , which results in $W = \pi \cos \theta F_\Omega l_\Omega^{oop}$. Given that the average temperature is constant in time, the work exerted on the vortex is equal to the energy dissipated by the vortex."

2. I am missing a calibration curve B vs. l_Ω , which is the basis to go from values of magnetic field measured in the experiment to the vortex displacement l_Ω . Such calibration is mandatory if you want to give credibility to Fig.2.

We thank the reviewer for emphasizing the need for a clear calibration between the measured magnetic signal and the vortex displacement. We agree that this point was not sufficiently explained in the original manuscript.

As discussed in the previous point, the conversion from the lock-in magnetic response $B_\Omega(\mathbf{r})$ to the displacement amplitude l_Ω is obtained directly from the spatial variation of the simultaneously measured static vortex image $B_{DC}(\mathbf{r})$. For a small vortex displacement l_Ω , the magnetic response is given to first order by

$$B_\Omega^{ip}(\mathbf{r}) = \nabla B_{DC}(\mathbf{r}) \cdot l_\Omega^{ip}$$

$$B_\Omega^{oop}(\mathbf{r}) = \nabla B_{DC}(\mathbf{r}) \cdot l_\Omega^{oop}$$

Thus, the local gradient of the DC image provides the calibration factor that converts the measured AC magnetic signal into a displacement. However, the gradient varies spatially so each pixel has a different calibration. Nevertheless, a conversion factor can be found considering the amplitude of both images.

$$l_\Omega^{ip,oop} = \frac{\max(B_\Omega^{ip,oop})}{\max\left(\frac{dB_{DC}(\mathbf{r})}{dx}\right)} = 1.3 \times 10^{-3} \max(B_\Omega^{ip,oop}) \text{ m/T}$$

This provides the intuitive B_Ω -to- l_Ω calibration requested by the reviewer. For example, for a signal of $\max(B_\Omega^{ip}) = 1 \mu\text{T}$, (meaning that the signal goes from -1 to +1 μT) we get a displacement of $l_\Omega^{ip} = 1.3 \text{ nm}$

In the actual analysis, however, we do not rely only on this single-pixel estimate. Instead, we fit the full spatial image of B_Ω^{ip} or B_Ω^{oop} using the measured B_{DC} , which implements the same calibration principle over the entire image. This procedure improves the signal-to-noise ratio and also allows us to determine the displacement direction θ relative to the applied force.

We have revised the manuscript to make this calibration procedure explicit and self-contained. In particular, we now explain that the DC and AC images are acquired simultaneously and that the displacement extraction is based directly on the measured spatial derivative of the static vortex field, not on an assumed model of the vortex dynamics.

3. Authors write: “At specific values of I_- , a sharp increase of B_{oop} is detected. It systematically coincides with the appearance of higher harmonics in $B(r)$. This rise in the measured work is interpreted as periodic vortex jumps over an energy barrier that separates two local potential minima (Figure 1g, i).”

My comment: According to Fig. 1g a vortex jump over energy barrier should be seen as a sizeable discontinuous displacement of the vortex, once the force exceeds a threshold value. I would expect to see a corresponding $B(t)$ trace acquired by the SOT for a harmonic current drive, for which such a discontinuous jump could be identified. Such a trace is mandatory for giving credibility to the proposed interpretation. It cannot be replaced by a toy model of Fig. 1(f-i). Authors refer (in fact they should demonstrate it in the current work by displaying appropriate data) to the previous work to claim resolution of the SOT better than 1nm. If it is the case a real (experimental) version of Fig. 1g is expected.

We thank the reviewer for this important comment regarding the direct observation of vortex jumps.

We agree that a direct time-domain measurement of $B(t)$ under periodic drive would provide an intuitive signature of barrier crossing. However, such measurements are currently limited by the bandwidth of our time-domain detection system. In the regime explored here, the vortex dynamics occur at frequencies beyond what can be resolved in time-domain acquisition.

Nevertheless, we provide several independent experimental observations that consistently support the interpretation in terms of barrier crossing between two metastable states.

First, we observe the systematic appearance of higher harmonics in $B(r, t)$ at the same values of I_{DC} where a sharp increase in the out-of-phase component B_Ω^{oop} is detected. The emergence of higher harmonics is a direct signature of strongly nonlinear dynamics, as expected when the vortex motion involves abrupt transitions between two potential wells rather than harmonic oscillations.

Second, in a regime where the characteristic hopping rate r_k falls within our experimental bandwidth, we directly observe telegraphic switching in $B(t)$ (see Fig. S1), corresponding to stochastic transitions between two metastable vortex positions. This provides direct experimental

evidence for bistability and vortex hopping at least in some location of the superconducting material. We emphasize that the double-well where we observe the telegraphic noise in time-domain does *not* correspond to any of the other double-well systems studied in this paper. That explains the discrepancy noted regarding the $r_k \sim 1$ Hz vs. kHz for double-wells #3 and #4.

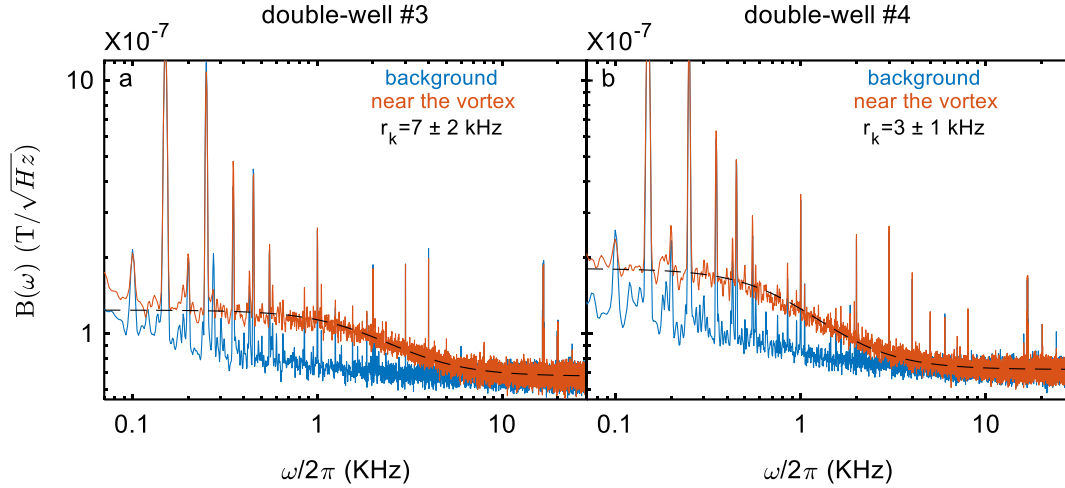
Third, we have the hardware to access the vortex dynamics $B(t)$ in the frequency domain by measuring the magnetic noise spectrum $B(\omega)$. For a two-state stochastic process, the power spectrum is expected to follow a Lorentzian form:

$$B(\omega) \propto \frac{1}{(4r_k^2 + \omega^2)}$$

We measure $B(\omega)$ at positions of maximal field gradient, which is significantly above the background SOT noise measured 1 microns away from the vortex. The resulting spectra are well described by this Lorentzian form, allowing us to extract r_k independently. The values obtained for double-well potentials #3 and #4 are $r_k = 6$ and $r_k = 3.1$ kHz, respectively. These values are consistent with those extracted from the stochastic resonance analysis presented in Fig. 5 of the revised manuscript ($r_k = 5$ and $r_k = 3.1$ kHz). Importantly, this measurement is performed in the absence of AC drive, providing an independent characterization of the underlying bistable thermally activated dynamics. The corresponding data have been added to the Supplementary Information.

In response to the reviewer's comment we added the following text referring to the Supplementary Figure shown below.

“The average hopping frequency r_k can be independently probed for an undriven vortex. In this measurement, the SOT is held stationary and positioned at one of the lobes of the AC magnetic field image $B_\Omega(\mathbf{r})$, where the signal arising from vortex motion is maximal. In this configuration, the measured signal contains an additional telegraphic noise component originating from the stochastic vortex dynamics, as illustrated in the time domain for another double-well potential in Supplementary Figure S2. We analyze this signal in the frequency domain by measuring the noise spectrum $B(\omega)$ at that location (Supplementary Figure S6). The measured spectrum consists of two contributions: (i) the intrinsic background noise of the SOT, characterized independently when the tip is positioned far from the vortex, and (ii) a Lorentzian component associated with telegraphic noise, expected for a two-state stochastic process, with spectral dependence proportional to $1/(4r_k^2 + \omega^2)$.²⁷ This contribution reflects the thermally activated transitions of the vortex between the two minima of the double-well potential. By fitting the measured spectra with this expression, we extract hopping rates of $r_k = 7 \pm 2$ kHz and $r_k = 3 \pm 1$ kHz for double wells #3 and #4, respectively. These values are consistent with those obtained independently from fitting the frequency dependence of the work $W(\Omega)$ and the first harmonic displacement $l_\Omega(\Omega)$ using Eqs. (1) and (2) in the weak-drive limit. This agreement provides further evidence for the stochastic nature of the vortex dynamics and strengthens the experimental basis for both the Monte Carlo simulations and the stochastic resonance analysis.”



Supplementary Figure S6 | Power spectra density measured near double-wells #3 and #4. (a-b) (blue curves) Background power spectral density of the magnetic field $B(\omega)$ acquired with the SQUID-on-tip (SOT) at a distance of $\sim 1 \mu\text{m}$ from the vortex center and contains solely the experimental setup noise floors. (orange curves) $B(\omega)$ measure with the SQUID SOT positioned at one of the lobes seen in $B_{\Omega}(r)$ for double wells #3 **a** and #4 **b**. Measurements were conducted in the absence of an external driving force. (black dashed lines) The experimental data are fitted to a Lorentzian model to extract the characteristic thermally activated hopping rates. $r_k = 7 \text{ kHz}$ and $r_k = 3 \text{ kHz}$ is obtained for double-wells #3 and #4, respectively.

Taken together, the direct observation of telegraphic switching at low rates, the emergence of higher harmonics and out-of-phase response under drive, and the Lorentzian noise spectrum in the frequency domain for undriven vortex provide consistent and mutually supporting evidence for vortex dynamics involving barrier crossing.

Finally, we clarify that the toy model shown in Fig. 1f–i (Fig. 2 in the revised manuscript) is intended only as an illustrative framework to guide intuition and is not used to extract any quantitative values from the data.

Regarding displacement sensitivity, we note that the SOT technique enables sub-nanometer resolution, as demonstrated in Ref. 13. In our setup, the sensitivity can be estimated from the measured noise level ($\Delta S_{B_{\Omega}(r)} \approx 0.25 \mu\text{T}$) and spatial gradient of the DC signal $\max\left(\frac{dB_{DC}(r)}{dx}\right) \approx 800 \text{ T/m}$, yielding a displacement sensitivity of order $\Delta S_{l_{\Omega}} = \frac{\Delta S_{B_{\Omega}(r)}}{\max\left(\frac{dB_{DC}(r)}{dx}\right)} = 0.3 \text{ nm}$. This estimate is now included in the revised manuscript.

4. The calculation of the vortex displacement as $l_{\Omega} = (l_{ip}^2 + l_{oop}^2)^{0.5}$ seems to be correct only for the movement within a single-well harmonic potential. For the claimed movement in between the two local potential minima higher harmonics should play a role, and the total displacement cannot be calculated via the same formula.

We thank the reviewer for raising this important point.

We agree that the expression $l_{\Omega} = \sqrt{(l_{\Omega}^{ip})^2 + (l_{\Omega}^{oop})^2}$ corresponds strictly to the amplitude of motion only in the case of a purely harmonic response, i.e., when the vortex remains within a single potential well. In the regime where the vortex undergoes transitions between two minima, the motion becomes strongly non-sinusoidal and higher harmonics are indeed present. In this case, the total real-space displacement is not fully captured by the first harmonic alone.

However, we emphasize that in our article l_{Ω} is not intended to represent the full real-space excursion of the vortex, but rather the amplitude of the first harmonic component of its motion. This is the quantity directly measured via lock-in detection and is the relevant observable in the context of our analysis.

Importantly, both theoretical frameworks used in this work—the stochastic resonance model and the Monte Carlo simulations—predict the first harmonic response of the system under periodic driving. Therefore, the comparison between experiment and theory is performed consistently at the level of this observable and not the total real-space displacement.

Furthermore, the work per cycle W , which is the central quantity of our study, is obtained from the out-of-phase first harmonic displacement. This definition remains valid even in the presence of higher harmonics, as it directly reflects the energy exchange associated with the periodic drive at the driving frequency.

We agree that this distinction was not sufficiently clear in the original manuscript. In the revised version, we explicitly clarify that l_{Ω} represents the first harmonic response and not the total displacement amplitude, particularly in the nonlinear regime where higher harmonics are present. See the statement below

“Notably, for such strong drives, the total real-space displacement amplitude is not equal to l_{Ω} due to the presence of higher harmonics. Our analysis only requires us to measure l_{Ω} and W , which are measured properly even in presence of higher harmonics.”

5. It is completely not proven that the Kramer’s formula can be applied to description of the studied experimental realm. This part of presentation has a character of speculation. To make scenario of thermal activation from one well to another plausible authors should have presented the telegraphic noise for double wells #3 and #4 (Fig.4) with dynamics at the level of kHz (comparable to rk of Fig.4). Also, they could have measured the telegraphic noise at various temperatures, which is the way to establish the actual “vortex jumping” mechanism in the postulated double-well structure. These type of demonstrations are prerequisite experiments to come up not only with the proposed Monte-Carlo modelling, but also to establish the validity of the toy model in Fig.1. Meanwhile, authors present the telegraphic noise with dynamics below 1Hz (Supp.Fig.S2). It looks more like a spurious noise-induced jumps than thermal activation of the vortex over the potential barrier. BTW I would suggest to display here the two SOT images each for vortex located in one of the wells. What is the distance between the two wells’ minima?

We thank the reviewer for this thoughtful comment. We respectfully note that while temperature-dependent measurements are a common approach to establishing thermally activated behavior, they are not the only route to identifying such dynamics.

In our system, we provide multiple independent pieces of evidence for bistable dynamics and stochastic switching, including (i) time-domain observation of telegraphic noise for one double-well in the accessible regime, (ii) Lorentzian field spectra $B(\omega)$ in the frequency domain (added in the revised version), and (iii) consistent hopping rates extracted independently from the work $W(\Omega)$ and the first harmonic displacement $l_\Omega(\Omega)$ in the weakly driven limit (stochastic resonance). (iv) Qualitative agreement with our Monte-Carlo simulations. Together, these observations establish the presence of a two-state system undergoing stochastic transitions at finite temperature.

Within this experimentally established framework, the use of a Kramers-type description is physically well justified and provides a natural and quantitative interpretation of the observed dynamics in the weakly driven regime. Our use of this formalism is therefore not intended as a speculative assumption, but as a consistent description of the measured behavior.

We agree that additional measurements, such as temperature dependence, could provide further insight into the microscopic origin of the activation process. However, in superconducting vortex systems, varying the temperature also modifies key physical parameters such as the coherence length and pinning landscape. As a result, it is not possible to independently vary the thermal energy while keeping the underlying bistable potential unchanged, which limits the applicability of such an approach in the present context.

Regarding the suggestion to directly image the vortex in the two metastable states, the expected separation between the two minima is of the order of $\lesssim 10$ nm, which is significantly smaller than the spatial resolution of our SQUID-on-tip probe (~ 250 nm diameter). As a result, the two states cannot be reliably distinguished in DC imaging. This aspect was discussed in prior work (Ref. 13) where we compare the DC image displacement with the AC technique used here. We copied this comparative figure below taken from that reference.

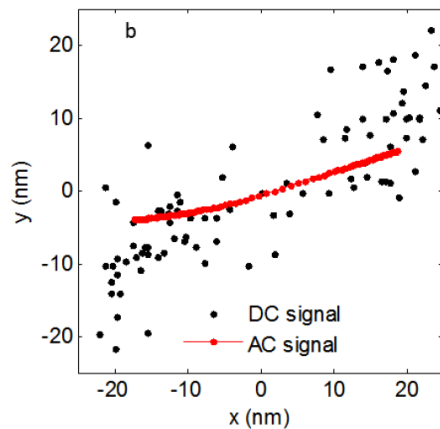
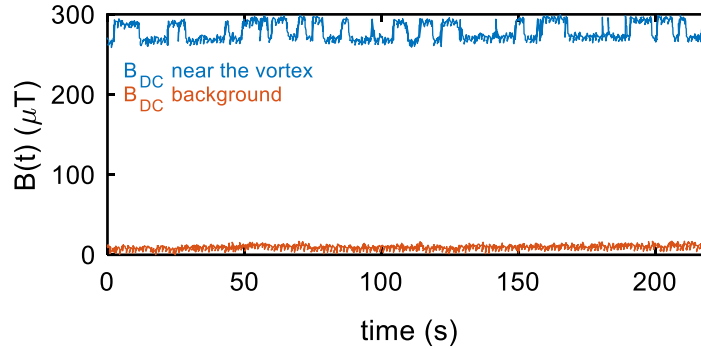


Figure | Vortex trajectory of a vortex upon varying F_{dc} at fixed F_Ω . The two data sets were extracted from the same experimental run using two methods: the DC signal was derived from the peak position of the B_{DC} image at each applied F_{DC} , and the AC signal was derived by integration of l_Ω as described in the revised main text.

Moreover, even if such spatial resolution were available, static imaging would not provide access to the frequency-dependent response of the vortex, which is the central observable required for comparison with stochastic resonance theory. For this reason, our analysis relies on the AC response of the vortex dynamics, as detailed in our response to Comment 1 and clarified in the revised manuscript.

Regarding the spurious tip-noise being the reason for the telegraphic noise, we added the measurement of the tip noise for the same period of time when the tip is far from the vortex. See the updated Supplementary Figure S2 below



6. Fig.5. and the accompanying discussion is a loose narrative which is not well-anchored in the experiment. It is a pure speculation.

We thank the reviewer for this comment.

The purpose of Fig. 6 of the revised manuscript is to provide a conceptual comparison between the experimentally implemented protocol (periodic AC drive) and the erasure protocol introduced by Landauer, which operates on a two-level system initially in a random state. This comparison is carried out using Monte Carlo simulations of a thermally activated two-state system.

Within this framework, we find that at high frequencies ($\Omega \gg r_k$), the work associated with the AC protocol would exceed by a factor 4 the energy required for a proper Landauer erasure protocol. At low frequencies ($\Omega \ll r_k$), the AC work vanishes, in contrast to the finite work required for an erasure operation. These trends are discussed in the manuscript and supported by the simulations.

We emphasize, however, that this part of the analysis is intended as an interpretation and extrapolation based on a minimal two-level model. The comparison relies on the assumption—supported by the experimental evidence discussed above—that the vortex dynamics can be described as a thermally activated two-state system.

Regardless of the interpretation, our raw measurement of the work W is model independent as the values of the measured work are indeed close to Landauer's limit.

In response to the reviewer's comment, we have revised the manuscript to make this distinction explicit, clarifying the assumptions underlying the simulation and avoiding any overstatement of the experimental scope.

Other comments:

1. Authors forget to give numerical values for physical quantities controlled/measured in the SOT experiment. This involves: B_{dc} , B_{ip} , B_{oop} (lack of scale for the color plots), I_0 , J_{Ω} .

We thank the reviewer for pointing out these omissions. Following the reviewer's comment, we added color bar to $B_{DC}(\mathbf{r})$, $B_{\Omega}^{ip}(\mathbf{r})$ and $B_{\Omega}^{oop}(\mathbf{r})$.

The values of I_0 and J_{Ω} can be calculated from the Lorentz force $F_{\Omega} = \Phi_0 J_{\Omega}$. In a thin film superconductor, an applied current I_0 results in a sheet current density $J(x) = \frac{I_0}{\pi\sqrt{(w_b/2)^2 - (x)^2}}$ where $w_b = 10 \mu\text{m}$ is the width of the bridge and $|x| \lesssim 1 \mu\text{m}$ is the distance of the tip with respect to the center of the bridge along the x axis. For example, for a typical current of $I = 0.5 \text{ mA}$, we obtain $J(0) \approx 31.8 \text{ A/m}$, and $F_{\Omega} = 65 \text{ fN}$. This was added in the manuscript as follows:

"The resulting Lorentz force is given by $\mathbf{F}_{\Omega}(x, t) = \Phi_0 J_{\Omega}(x) \hat{x} = F_{\Omega} \cos \Omega t \hat{x}$, where Φ_0 is the magnetic flux quantum and $J_{\Omega}(x) = \frac{I_{\Omega}}{\pi\sqrt{(w_b/2)^2 - (x)^2}}$ is the sheet current density at a distance x from the center of the bridge of width w_b as calculated in Ref. 26. For example, for a typical current of $I = 0.5 \text{ mA}$, we obtain $J(0) \approx 31.8 \text{ A/m}$, and $F_{\Omega} = 65 \text{ fN}$ $\hat{x}$ (green double-headed arrow in Figure 1a)."

2. Authors write about sweeping of I_{dc} to find a sharp increase of B_{oop} . The role of I_{dc} is not discussed in the physical terms. Also it is not clear how the amplitude of the accompanying oscillating current I_{Ω} is set.

We thank the reviewer for pointing out that the roles of I_{DC} and I_{Ω} were not sufficiently explained in the original manuscript.

The DC current I_{DC} generates a static Lorentz force on the vortex, which effectively tilts the pinning potential landscape and shifts the equilibrium position of the vortex across the sample. By sweeping I_{DC} , we probe different regions of the local pinning landscape. For specific values of I_{DC} , the vortex is positioned in a bistable region, as evidenced by the appearance of higher harmonics and an increase in the out-of-phase response $B_{\Omega}^{oop}(\mathbf{r})$. Once such a double-well configuration is identified, I_{DC} is fixed for the subsequent measurements.

The AC current I_{Ω} provides a time-dependent driving force with controlled amplitude and frequency. Its amplitude is systematically varied to explore different dynamical regimes, ranging from small oscillations within a single well ($F < F_c$) to periodic transitions between wells ($F > F_c$). The corresponding force scale is determined experimentally from the observed crossover between linear and nonlinear response, as reflected in the onset of higher harmonics and enhanced out-of-phase signal.

We have clarified these points in the revised manuscript to better describe the experimental protocol and its physical basis.

"The force scale was determined experimentally from the observed crossover between linear and nonlinear response, as reflected in the onset of higher harmonics and enhanced out-of-phase signal."

3. The physical origin of the theta angle between displacement and force is not discussed.

We thank the reviewer for pointing out that the physical origin of the angle θ between the applied force and the vortex displacement was not sufficiently discussed.

This angle arises from the anisotropy of the local pinning potential experienced by the vortex. In general, the restoring force acting on the vortex is determined by the local curvature of the potential landscape, which need not be isotropic. As a result, the direction of the restoring force—and therefore the resulting displacement—is not necessarily collinear with the applied driving force.

Such behavior has been reported previously in vortex dynamics in anisotropic pinning landscapes (see e.g. Ref. 17 for experimental observations and Ref. 18 for theoretical description). We have clarified this point in the revised manuscript as follow:

“The angle between the applied force and the vortex motion arises from the anisotropy of the pinning potential and has been discussed in prior works^{17,18}.”

4. Authors write that they measure work. It is not true. They estimate/calculate/infer/guess/speculate, but do not measure. There are too many conceptual bottlenecks on the way from experimental data to the claimed value of work to talk about an act of measurement.

We thank the reviewer for raising this important point regarding the definition of the measured work.

We agree that it is essential to clearly distinguish between directly measured quantities and model dependent quantities. In our experiment, the primary observables are the in-phase and out-of-phase components of the magnetic response at the drive frequency, obtained via lock-in detection. These correspond to the first harmonic components of the vortex displacement via the method described above and illustrated in Figure 1.

The work per cycle is then obtained from these measured quantities using the general definition of work for a periodically driven system, $W = \oint F(t) \cdot v(t) dt$, which can be expressed in terms of the first harmonic components of force and displacement. This leads to the relation used in the manuscript, where the work is directly proportional to the out-of-phase component of the displacement.

Importantly, this relation does not rely on a specific model of the vortex dynamics (such as a two-level system or a particular potential shape), but follows from the general properties of periodic driving and Fourier decomposition. In this sense, the work is obtained directly from experimentally measured quantities through a well-defined and model-independent procedure.

The role of theoretical models in this work is not to extract the work, but to interpret the measured results.

5. The calculation presented in Supp.Mat.4 is central to provide a transition from values measured and imposed in the experiment to the actual claimed work. It should be presented in the main text.

We thank the reviewer for his suggestion. We agree with the reviewer and moved the content that was written in Supplementary Note 4 to the main text. In addition, we are now dedicating an entire figure to describe our method.

6. Fig.3 and Fig.4 are overcrowded with panels and curves to the extent that they cannot be distinguished one from another. Authors should focus here only on few curves, but first they need to prove that the model of thermal activation is applicable in their case (see p.5 of major allegations).

We thank the reviewer for pointing out the lack of clarity in Figs. 3 and 4 of the submitted version (figure 4 and 5 in the new submission).

In response, we have simplified these figures by reducing the number of displayed curves and focusing on representative datasets. See the figures below:

Regarding the applicability of the thermally activated two-state model, we refer to our responses to the major comments, where we provide additional experimental evidence supporting this interpretation, including the frequency-domain measurement of $B(\omega)$ and the consistency of independently extracted hopping rates. We have also revised the manuscript to clarify the assumptions and scope of this model.

7. I am missing an appealing intuitive physical description which would explain why I_{oop} component of the displacement is solely responsible for dissipation, which is the result of calculation presented in the Supp. Note 4. The simplest description would tell that the displacement lags in phase behind the harmonic force and it can be thus decomposed into in-phase and quadrature components (IQ demodulation). In fact this is enough to arrive to the formula which you use to calculate work. The higher harmonics, according to your interpretation, indicate periodic vortex jumps over an energy barrier. These higher harmonics seem to be responsible for dissipation, when vortex irreversibly changes its position from one well to another. Nevertheless, they average out when calculating work. Can you discuss the involved physical processes to illustrate better links between your experiment (B-field harmonics) and the investigated work/dissipation?

We thank the reviewer for highlighting the need for a more intuitive physical explanation. We agree that this point is important and have clarified it in the revised manuscript.

As the reviewer correctly points out, the key physical idea is that dissipation arises from a phase lag between the applied force and the resulting displacement. When the vortex response is purely in-phase with the drive, the motion is reversible and no net work is performed over a cycle. In contrast, an out-of-phase (quadrature) component indicates that the displacement lags behind the force, leading to a finite energy dissipation per cycle. This is directly analogous to standard IQ (in-phase/quadrature) decomposition used in driven systems. This intuitive picture is hopefully given by our toy model shown in Figure 2 of the revised manuscript.

Formally, the work per cycle is given by:

$$W = \oint \vec{F} \cdot d\vec{l}$$

We perform the dot product and change the space integral to a time integral.

$$W = \cos \theta \int_0^{2\pi/\Omega} F \frac{dl}{dt} dt$$

Where θ is the angle between the force and the direction of motion and F is the driving force $F = F_0 \cos \Omega t$. In general, for a closed orbit, the position l of the vortex is described by a Fourier. Taking the time derivative of such a series gives the following:

$$\frac{dl}{dt} = \sum_{n=1}^{\infty} n\Omega l_{n\Omega}^{ip} \sin n\Omega t + n\Omega l_{n\Omega}^{oop} \cos n\Omega t$$

Substituting this expression into the time integral of the work we obtain

$$W = \cos \theta \int_0^{2\pi/\Omega} F_0 \cos \Omega t \left[\sum_{n=1}^{\infty} n\Omega l_{n\Omega}^{ip} \sin n\Omega t + n\Omega l_{n\Omega}^{oop} \cos n\Omega t \right] dt$$

It is known that all the terms of this sum is equal to zero once integrated over a period because these function are orthogonal. The only terms that survive is the out-of-phase 1st harmonic term.

$$W = \pi \cos \theta F_0 l_{\Omega}^{oop}$$

Where l_{Ω}^{oop} is measured using the method described in point 1 of the major allegations section.

We believe that this point is now clearly explained in the main text as stated in point 1 of the major allegation section.

In summary, the paper does not provide a firm experimental verification of the Landauer limit. It relies too much on particular models (not confirmed experimentally in the current presentation) and order-of-magnitude estimations of various physical quantities necessary to calculate work. Critically, the analysis is not well-rooted in the experiment, and thus it is not convincing. The forces, works and vortex displacements are not determined directly but either estimated (force), inferred from the SOT measurements (vortex displacements, surprisingly authors do not present any experimental data here) or calculated (work). It is very important to display the actual as – received measured data (e.g. B(t), B(vortex displacement)) and guide the reader step-by-step while recalculating them into the finally presented quantities (work, displacement, force). The only as-received experimental data in the main text are the SOT images (Fig.1b-e). A serious drawback of the presentation is the lack of the experimental confirmation of the double well vortex dynamics, which would involve the Kramer's thermal activation behavior. I cannot recommend the paper for publication.

We thank the reviewer for the detailed and critical assessment of our work. We fully recognize the importance of clearly establishing the experimental basis of our conclusions and of distinguishing measured quantities from model-dependent interpretations.

In response to these concerns, we have substantially revised the manuscript to make the experimental methodology more transparent and self-contained. In particular, we have reorganized the presentation to explicitly show how the primary measured quantities (lock-in signals of the magnetic field) are converted step-by-step into the first harmonic displacement $l_\Omega(\Omega)$ and the work $W(\Omega)$, without invoking any specific dynamical model. We have also expanded the main text to clearly separate experimentally measured observables from the theoretical frameworks used for interpretation.

To strengthen the experimental grounding, we have added new data, including the frequency-domain measurement of the magnetic noise $B(\omega)$, which provides independent evidence of stochastic dynamics consistent with a bistable potential landscape. In addition, we have clarified the role of higher harmonics and their connection to nonlinear vortex motion.

We hope that these substantial revisions address the reviewer's concerns and clarify that the results are firmly rooted in experimentally measured quantities, with theoretical models used only as a framework for interpretation.

Reviewer #3 (Remarks to the Author):

The present manuscript reports on quantifying a dissipation of single vortex motion under a periodic drive and suggests feasibility of the memory bit based on a superconducting vortex confined to a nanoscale double-well pinning potential using a SQUID-on-tip microscope.

By measuring the magnetic signal modulation induced by vortex motion, the average work per cycle is directly assessed. The experiment is performed in a Nb film of thickness 55 nm which was grown and patterned into a 10 μm -wide bridge. Because the value of thickness is chosen to be of the same order as the coherence length (12 nm) and the London penetration depth (220 nm), a vortex is regarded as a particle in a random 2D potential landscape. #

The results are arranged in three groups according to three possible vortex manipulation protocols: at weak drives, stochastic resonance behaviour; at strong, low-frequency drives, adiabatic manipulation; at high frequencies, the work required for bit erasure is shown to be within the order of magnitude of Landauer's limit.

The authors report measuring dissipation levels orders of magnitude below those achieved in previous works and reaching levels below the Landauer's limit (see, e.g., Fig. 2c, Fig. 4c and Supplementary Figure Fig. S5j,k,l).

The present work demonstrates, both experimentally and theoretically, the fact that the fundamental Landauer's limit that establishes the minimum energy needed to erase one bit of information within an irreversible change of information, can be overcome. The energy consumption below that limit is attributed to other measurement protocols, as compared to those implied in the original Landauer's derivation of 1961. A comprehensive analysis of different protocols is summarized in Fig. 5. The minimum energy needed to erase one bit of information in the present work relates to a continuous vortex manipulation protocol, while the Landauer's limit recovers in the single-shot protocol. This finding is, however, not explicitly reflected in Abstract.

We thank the reviewer for the positive assessment of our work and for highlighting the importance of clearly distinguishing between different manipulation protocols.

We would like to clarify that our results do not demonstrate a violation or overcoming of the Landauer limit in the case of an erasure protocol. Rather, they highlight the fact that the minimal work required depends on the specific protocol used to manipulate the system. In particular, the continuous sinusoidal drive employed in our experiment differs fundamentally from the single-shot erasure protocol considered in Landauer's original formulation.

In our work, we show that while the work associated with the continuous driving protocol can fall below $k_B T \ln 2$, the Landauer limit is recovered when considering an appropriate erasure protocol applied to the same system. Our numerical analysis (Fig. 6 of the revised version) is intended to illustrate this distinction between protocols, rather than to suggest a violation of the fundamental bound.

Following the reviewer's comment, we have revised the abstract to make this point more explicit. It now reads:

“Although our experimental method requires a steady sinusoidal drive incompatible with Landauer’s erasure protocol, our numerical analysis demonstrates that the work required for such bit manipulation is within an order of magnitude of Landauer’s limit ($k_B T \ln 2$) at 4.2 K.”

The distinct properties of the double-well systems under study are unified within a one-dimensional Monte-Carlo simulation assuming a simple model pinning potential with two fitting parameters, the height of the energy barrier between the two wells and the distance between the two potential minima. A sinusoidal driving force, matching the Lorentz force occurring in experiment, tilts the pinning potential. The stochastic physics behind the modelling gives, in principle, a fair description of the displacement and work as a function of the driving frequency, except for the regular pattern of the simulated Figs. 3d,h,f in contradistinction to irregular (fluctuating?) regimes in the experimental Figs. 3c,g,e, which should have been explained.

We thank the reviewer for pointing out the difference between the smooth behavior of the simulations and the fluctuations observed in the experimental data.

The fluctuations visible in the experimental curves (e.g., Fig. 4c,e,g of the new submission) arise from the finite signal-to-noise ratio of the measurement. In particular, they become noticeable in the regime of low dissipation, where the measured signal approaches the noise floor of the SQUID-on-tip and its readout electronics. As a result, these variations reflect instrumental noise rather than intrinsic vortex dynamics.

In contrast, the Monte Carlo simulations do not include measurement noise and therefore produce smooth curves.

Importantly, these fluctuations are confined to the low-signal regime (e.g., $W \lesssim 10^{-23}$ W) and do not affect the main trends or the quantitative comparison between experiment and simulation in the regimes of interest.

In response to the reviewer’s comment, we have clarified this point in the manuscript with the following addition:

“Notably, our simulations capture all the features discussed above except for the experimental noise, which becomes visible in the low-dissipation regime ($W \lesssim 10^{-23}$ W). This noise originates from the finite field sensitivity of the SOT and its readout electronics.”

Another point showing that the used simple model reveals some inconsistency, is the fact mentioned by the authors that for double-well #3 significantly different values of the distance between the two potential minima are used to fit the displacement and work (Supplementary Table S1). More measurements on every sample as well as experiments for a larger number of samples would provide an enhanced statistical representativeness to the proposed model.

We thank the reviewer for highlighting this important point.

We note that the discrepancy identified for double-well #3 arises in the stochastic resonance analysis, where slightly different values of the distance between the two minima (l_0) are obtained when fitting the displacement and the work. The difference is small (on the order of ~ 2 nm) but statistically significant given the reproducibility of the data.

We verified that enforcing a common value of $l_0 = 3$ nm leads to a visibly poorer fit (see figure below), indicating that this discrepancy is statistically significant. At present, its precise origin is not fully understood. However, it is important to emphasize that this deviation is limited in magnitude and does not affect the main conclusions of the work.

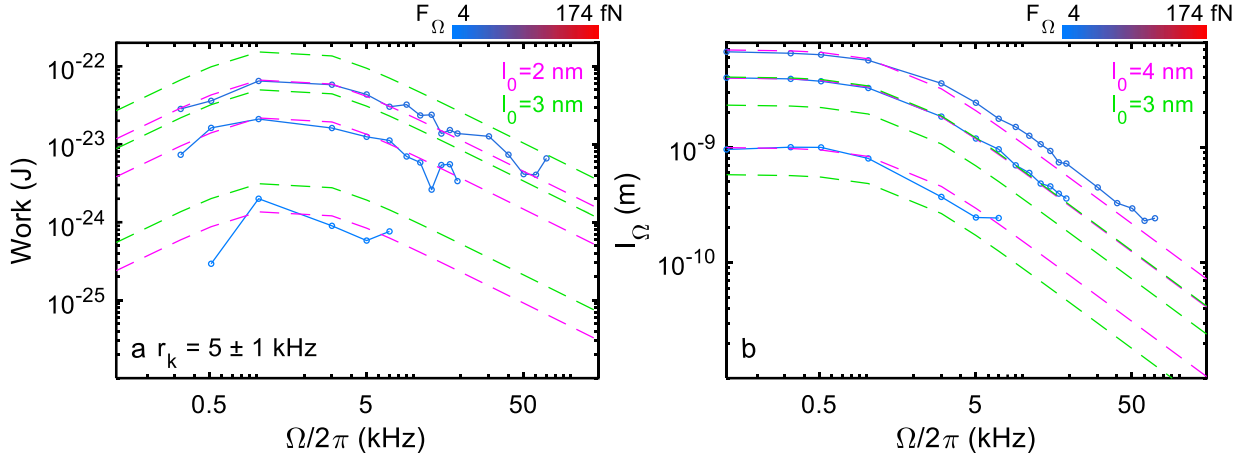


Figure – Measured vortex displacement and work of double-well #3. (a) Work W as a function of the driving frequency Ω . The black dashed line marks four times Landauer's limit. **(b)** Displacement $l_\Omega = \sqrt{(l_\Omega^{ip})^2 + (l_\Omega^{op})^2}$, as a function of the driving frequency Ω . In both each curve represents the measured W or l_Ω for a given driving force F_Ω . The color of each curve represents F_Ω as indicated by the respective color bars. The pink dashed curves correspond to equations (1) and (2) using $r_k = 5$ kHz and $l_0 = 3$ nm.

Importantly, this behavior is not observed in the six additional double-well systems presented in Supplementary Fig. S5, where the stochastic resonance model provides consistent fits using the same parameters for both observables. This supports the overall validity of the model and indicates that the discrepancy observed for double-well #3 is not representative of the general behavior. It might be caused by an additional systematic error that was only observed once.

In the revised manuscript, we have clarified this point and explicitly noted this limitation to ensure transparency.

“This small discrepancy is statistically significant but its origin remains unknown, although the fact that it was not observed in six other examples shown in Supplementary Figure S5 suggests that it might be due to a small systematic error.”

A few refinements are needed in the manuscript. The explanation “another factor 2 related to the initial condition of the vortex” above Fig. 5 is not self-explanatory.

We thank the reviewer for pointing out the lack of clarity in the explanation of this factor.

The additional factor of 2 arises from the difference in initial conditions between the Landauer erasure protocol and our experimental protocol. In Landauer's protocol, the initial state of the memory is assumed to be random, with equal probability of being in state 0 or 1. If the system is already in the target state, the erasure operation requires no work. As a result, on average, only half of the realizations have a finite contribution to the erasure cost.

In contrast, in our experimental protocol, the continuous sinusoidal drive effectively prepares the system in a well-defined initial state prior to each cycle. As a result, the vortex must be driven across the barrier in every cycle, leading to an average work that is larger by a factor of 2 compared to the Landauer erasure protocol.

This intuitive picture is supported by our Monte Carlo simulations (Fig. 6 in the revised manuscript), which reproduce the same factor between the two protocols.

Following the reviewer's comment, we have clarified this explanation in the manuscript to make the origin of this factor more explicit. The passage now reads as follows:

"... another factor 2 related to the initial condition of the vortex. In Landauer's erasure protocol, the initial state of the memory is assumed to be random, with equal probability of being in state 0 or 1. If the system is coincidentally already in the target state, the erasure operation requires no work. As a result, on average, only half of the realization requires hopping over the barrier and thereby contributing to the erasure cost. In contrast, in our experimental protocol, the continuous sinusoidal drive effectively prepares the system in a well-defined initial state prior to each operation. As a result, the vortex must be driven across the barrier in every operation, leading to an average work that is larger by a factor of 2 compared to the Landauer protocol."

In some places, e.g., below Fig. 4, the appropriate expression seems to be "double-wells #3 and #4 present".

We thank the reviewer for pointing out these grammatical mistake. We revised the entire article.

The sentence "For example, by integration with vortex control elements and readout demonstrated in Ref. 2" in Discussion is grammatically incomplete. In Fig. 5, the sequence of panels must be inverted.

We thank the reviewer for pointing out this typo. The phrase now reads as follow:

Finally, our results allow us to evaluate the potential of vortices as low-dissipation memory elements. This could be achieved, for example, by integration with vortex control elements and readout as demonstrated in Ref. 2.

Regarding Figure 5, which is now Figure 6, the order of the panels was inverted (see below).

In summary, the manuscript provides a sound validation of an efficient memory element for low-energy consumption memory devices. It can be suitable for publication in the Nature Communications once the authors have taken into account the above comments.

We thank the reviewer for the thorough and constructive evaluation of our work, and for recognizing its potential significance. We believe that the reviewer's comments have helped us substantially improve the clarity and rigor of the manuscript. We have carefully addressed all the points raised and hope that the revised version meets the reviewer's expectations.

Reviewer #1 :

All my comments were satisfactorily addressed. One minor comment - from the replies and revisions, it seems like the technique presented here is not really applicable to efficient memory manipulation, at least not in the sense of realizing an erasure protocol in a useful frequency. I would therefore encourage the authors to step back the title of the manuscript, in addition to the changes already made to the abstract and main text.

We thank the reviewer for the positive assessment and for the helpful suggestion regarding the title.

We agree that the title should not imply that the sinusoidal-drive protocol used in the experiment itself constitutes a practical erasure protocol for memory operation. As clarified in the revised abstract and main text, our experiment measures the work associated with periodic vortex manipulation, while the energy cost of a true erasure protocol is inferred using the experimentally constrained model. At the same time, we respectfully believe that the current title *“Approaching Landauer’s limit in a single superconducting vortex memory bit”* remains appropriate because the work does address the energetic cost of manipulating a vortex-based memory bit. A true erasure operation could in principle be implemented in the same double-well system by applying a current pulse that tilts the potential toward a prescribed final state. The limitation of the present technique is not the implementation of the operation itself, but rather the direct measurement of dissipation during such a single-shot protocol.

For that reason, we believe that this title is appropriate but are open for suggestions.

Reviewer #2

The paper has been improved and it is now more accessible for readers. I would like to thank authors for much better explanation of the determination of the vortex displacements measured with LOCK-IN technique, which is now well-described in Fig.1. However, I still find problematic issues and inconsistencies, which I am bringing to attention of authors in hope that their presentation may become an excellent piece of work, once they are able to address them. Here they are:

We thank the reviewer for the careful reading of the revised manuscript and for acknowledging the substantial improvements in the clarity of the presentation, particularly regarding the determination of vortex displacements using the lock-in technique.

1.To investigate the energy required to manipulate a memory bit, first you have to prove experimentally that you have such a bit, i.e. you have a system with two states with ability (i.e. an experimental knob) to swap between them. It would be the most natural introduction to the further investigations, devoted to determination of the energetic cost of such operations.

Authors write in the response to my first report:

“Regarding the suggestion to directly image the vortex in the two metastable states, the expected separation between the two minima is of the order of $\lesssim 10$ nm, which is significantly smaller than the spatial resolution of our SQUID-on-tip probe (~ 250 nm diameter). As a result, the two states cannot be reliably distinguished in DC imaging. This aspect was discussed in prior work (Ref. 13) where we compare the DC image displacement with the AC technique used here. We copied this comparative figure below taken from that reference.”

I am not convinced with the presented explanation. For double-well presented in Fig.3d the displacement is $I_{\omega} \sim 50$ nm. Importantly, it is only the displacement resulting from the measurement of the first harmonics. The real-space displacement could be even larger. It should be well experimentally catchable with the DC imaging, if to trust the resolution presented in Fig. Vortex trajectory of a vortex upon varying F_{dc} at fixed F_{ω} , which authors provide in their response to my first review. Also judging from the experimental profile presented in Supplementary Figure S10. London penetration length measurement, 50-100 nm displacement of the vortex should be detectable by DC mode.

Further, the trace presented in Supplementary Figure S2. Telegraphic Noise is a proof that authors can distinguish two positions of the vortex experimentally: there are two well-separated levels of B corresponding to two states of the vortex. What is the distance l between these two minima? Why don't you present memory operation for the vortex from the presented trace? It seems quite obvious that applying a positive force $+F_{\omega}$ should keep the vortex in one well, while a negative force $-F_{\omega}$ should allow to trap it in the second well. The lack of such presentation makes your presentation not as convincing as it could be and leaves some room for criticism: if the reasoning presented in the paper is right, swapping the vortex deterministically between the two wells with applied DC current and its detection should be possible. This seems actually a prerequisite to conduct further studies.

Moreover, such approach would allow to verify experimentally the presence of hysteresis postulated in the toy model of Fig.2b. It would be very desirable to see experimental hysteresis instead of its conceptual illustration. Here, after applying the force and removing it you could even use LOCK-IN technique to take advantage of your displacement sensitivity of $dS_{x_{\omega}} \sim 0.3$ nm to resolve two distinct positions of the vortex. The hysteresis should be seen for double-wells #1 and #2 since they exhibit very low thermal hopping rates.

These are obvious experimental tests which transparently would confirm existence of double-well vortex systems in your Nb strip.

We thank the reviewer for this thoughtful suggestion. We agree that direct visualization of deterministic switching and hysteresis would provide an intuitive illustration of the double-well dynamics.

Before discussing the proposed experiment, we would like to emphasize that the existence of a two-state system is already supported by several independent experimental observations:

- (i) The appearance of a critical force (F_c), above which the response becomes strongly nonlinear and finite dissipation is observed and reported in Figure 3.
- (ii) The telegraphic noise shown in Supplementary Fig. S2, which directly demonstrates stochastic switching between two distinct vortex states.
- (iii) The frequency dependence of the work ($W(\Omega)$) and first-harmonic displacement ($l_\Omega(\Omega)$), which is quantitatively described by the stochastic resonance theory of a weakly driven two-state system (Figure 4 and 5).
- (iv) The independently measured noise spectrum ($B(\omega)$) measured near an undriven vortex (Supplementary Figure S6), which exhibits the expected Lorentzian form of a two-state stochastic process and yields hopping rates consistent with those extracted from ($W(\Omega)$) and ($l_\Omega(\Omega)$).
- (v) The agreement between the experimental observations and the Monte Carlo simulations based on a thermally activated two-state system.

Regarding the proposed DC imaging experiment, we would like to clarify that the telegraphic-noise measurement and the DC imaging mode operate under very different experimental conditions. The telegraphic signal is acquired with the SOT held stationary above the vortex, making it sensitive to extremely small position changes. In contrast, DC imaging requires scanning the probe over more than a micron. In practice, scan distortions and drift of a few nanometers are unavoidable over the acquisition time of a complete image. As discussed in Ref. 13, this limits the effective spatial resolution of the DC imaging mode to approximately 20 nm, substantially worse than the displacement sensitivity required to image potential well #3 and #4, which are central to our work.

We also note that the vortices most relevant to the low-dissipation regime investigated in this work exhibit hopping rates that are much faster than the acquisition time of a DC image (typically (~ 100) s). Consequently, any bistable structure would be averaged during image acquisition and could not be resolved directly in DC mode. The proposed experiment would therefore only be feasible for the slowest double wells (#1 and #2), which correspond to the highest-dissipation regime and are not representative of the operating regime relevant to the Landauer discussion.

For these reasons, while we agree that such measurements could provide an additional qualitative illustration of bistability in a subset of vortices, we do not believe they would substantially strengthen the central conclusions of the manuscript beyond the five independent pieces of evidence already presented.

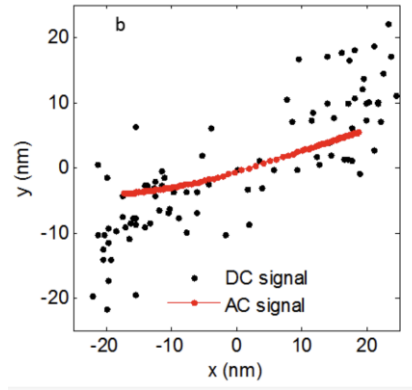


Figure 1 | Vortex trajectory of a vortex upon varying F_{DC} at fixed F_{Ω} . The two data sets were extracted from the same experimental run using two methods: the DC signal was derived from the peak position of the B_{DC} image at each applied F_{DC} , and the AC signal was derived by integration of I_{Ω} as described in the revised main text.

2. Authors write: “Regarding the spurious tip-noise being the reason for the telegraphic noise, we added the measurement of the tip noise for the same period of time when the tip is far from the vortex. See the updated Supplementary Figure S2 below.”

My response: It is obvious that trace collected in the background must not show jumps in the signal. My previous question was about the reason for jumps in the telegraphic noise. I wonder if the activation of the vortex over barrier is indeed of thermal character. It may be caused by a spurious noise propagating in the Nb sample or in the SOT tip.

We thank the reviewer for raising this important point.

We agree that the observation of telegraphic noise alone does not, by itself, prove that the underlying activation mechanism is purely thermal. In principle, any stochastic force acting on the vortex could contribute to the observed transitions.

However, several observations strongly suggest that the hopping dynamics are dominated by the local properties of the vortex potential landscape rather than by a common instrumental noise source. All measurements presented in this work were performed on the same sample, during the same cooldown, using the same SOT and the same amplification chain. Nevertheless, we observe characteristic hopping rates spanning many orders of magnitude, from approximately sub-Hz to several hundred kHz depending on the specific local vortex and double-well configuration. If the transitions were driven primarily by an external noise source originating from the measurement system, one would expect similar characteristic rates across different vortices. Instead, the hopping rates vary strongly from one vortex to another, indicating that they are governed by the local energy landscape.

Furthermore, the hopping rates independently extracted from the telegraphic-noise spectrum ($B(\omega)$), the stochastic-resonance analysis of ($W(\Omega)$) and ($I_{\Omega}(\Omega)$), and the Monte Carlo simulations are mutually consistent. This consistency would be difficult to reconcile with an uncontrolled external noise source dominating the dynamics.

We note that our measured dissipation approaches within a factor of approximately five of Landauer’s limit. An additional source of noise would have the effect of raising the effective temperature. That noise would raise Landauer’s bound to $k_B T_{eff} \ln 2 > k_B 4.2 \ln 2$. Therefore leading to substantially larger dissipation than observed experimentally. While we cannot completely exclude the presence of additional non-thermal noise sources, the fact that the measured energy scale remains close to the thermal limit provides further evidence that such contributions, if present, are not dominant.

We also considered that the vortex could self heat as a result of the dissipation. However, the r_k obtained from the telegraphic noise of the undriven vortex $B(\omega)$ is very similar to the one obtain from equations 1 and 2 fitting $W(\Omega)$ and $l_\Omega(\Omega)$ for perturbative drive F_Ω .

Taken together, the available experimental evidence indicates that the dominant mechanism governing the observed switching dynamics is stochastic activation in the local double-well potential.

Finally, we note that even if an additional stochastic force were present, it would not alter the central conclusions of this work. The experimentally measured quantities are the work (W), the first-harmonic displacement (l_Ω), the hopping rate (r_k), and the telegraphic-noise spectrum. These observables demonstrate that the vortex behaves as a stochastic two-state system and that its manipulation requires an exceptionally small amount of energy. The precise microscopic origin of the fluctuations only enters through the origin of the effective hopping rate (r_k). Therefore, while identifying the exact source of the stochastic activation is an interesting question, the main conclusions regarding the observed vortex dynamics, the stochastic-resonance behavior, and the low dissipation associated with vortex manipulation remain unchanged.

3. Authors write: "Within this experimentally established framework, the use of a Kramers-type description is physically well justified and provides a natural and quantitative interpretation of the observed dynamics in the weakly driven regime. Our use of this formalism is therefore not intended as a speculative assumption, but as a consistent description of the measured behavior."

My response: As long as you do not prove it experimentally it remains an assumption. How can you be sure that vortex is not pinned strongly? If it is, it would make the thermal activation mechanism ineffective and negligible.

Authors: "We agree that additional measurements, such as temperature dependence, could provide further insight into the microscopic origin of the activation process. However, in superconducting vortex systems, varying the temperature also modifies key physical parameters such as the coherence length and pinning landscape. As a result, it is not possible to independently vary the thermal energy while keeping the underlying bistable potential unchanged, which limits the applicability of such an approach in the present context."

My response: Yes, the activation process should be tested experimentally. Even if there are certain limitations, which you mention, you should prove that your activation mechanism somehow feels the temperature. May be you cannot recover the theoretical Kramer's behavior from such analysis, but qualitatively you should see differences in the hopping rate for various temperatures. Particularly, you should see the difference in the telegraphic noise of Supp.Fig.2, when the temperature changes.

We thank the reviewer for raising this important point. We agree that temperature-dependent measurements are a common way to test activated dynamics in many systems. However, we respectfully disagree that such measurements are a necessary prerequisite for establishing the stochastic two-state dynamics reported here, nor would they provide an unambiguous test in the present superconducting vortex system.

The reason is that temperature is not an independent control parameter for the activation energy of a superconducting vortex. Changing the temperature changes *not* only the thermal energy $k_B T$, but also superconducting parameters such as the coherence length ξ , and consequently the pinning potential itself. Therefore, a change in hopping rate with temperature would not isolate the effect of thermal fluctuations; it would also reflect a modified vortex potential landscape.

To illustrate this point, consider the potential landscape used in our Monte Carlo simulation for double-well #3 with the parameter listed in Supplementary Table S1. Taking $T_c \approx 8$ K and using

$$\xi(T) = \frac{\xi(0)}{\sqrt{1 - \frac{T}{T_c}}}$$

a temperature change from 4.20 K to 4.21 K changes the coherence length by a factor $\frac{\xi(4.21)}{\xi(4.20)} \simeq 1.0013$. Although this change appears small, the corresponding modification of the pinning potential changes the calculated barrier from 1.20×10^{-21} J to 1.16×10^{-21} J in our model, which nearly doubles the hopping rate r_k even while assuming that the thermal fluctuation did not change. Thus, even a very small temperature variation can strongly affect r_k through changes in the potential landscape, not only through the Arrhenius factor. A temperature-dependent measurement would therefore not provide a clean verification of Kramers behavior in this system.

Regarding the possibility that the vortex is strongly pinned, we note that this scenario is inconsistent with several experimental observations. A strongly pinned vortex with negligible thermal activation would not show spontaneous telegraphic switching, a Lorentzian noise spectrum characteristic of a two-state stochastic process, or stochastic-resonance behavior with hopping rates ranging from ~ 1 Hz to several hundred kHz depending on the local double-well configuration. The fact that finite hopping rates are observed, and that these rates vary strongly from vortex to vortex under the same experimental conditions, indicates that the dynamics are governed by the local energy landscape and that the barriers are accessible to stochastic activation.

We also note that our conclusions do not rely on proving that the stochastic fluctuations are purely thermal in a microscopic sense. The experimentally measured quantities are the work W , the first-harmonic displacement l_Ω , the hopping rate r_k , and the magnetic noise spectrum $B(\omega)$. These observables establish that the vortex behaves as a stochastic two-state system under periodic drive. If a small additional stochastic noise source were present, it would enter through the effective hopping rate r_k , but it would not change the central conclusions regarding the measured low dissipation, stochastic-resonance behavior, and controllable vortex dynamics.

Finally, we note that the present cryogenic setup is optimized for stable operation at 4.2 K and does not allow the controlled variable-temperature measurements that would be required for such a study.

4. About your modelling of the thermal activation. You write in the description of the data presented in Fig.3: "The change in F_c can be explained by thermal activation, which plays a greater role at low frequencies." But in other place you estimate $r_k=10^{(-45)}\text{Hz}$ and $r_k=10^{(-6)}\text{Hz}$ for double-wells #1 and #2, respectively. E_b is U_{tot} dependent, and U_{tot} is dependent on F_Ω . Are the quoted values of r_k given for $F_\Omega=0$? How do they change at $F_\Omega=F_c$?

We thank the reviewer for pointing out this ambiguity.

The quoted values of r_k for double-wells #1 and #2 correspond to the undriven case, $F_\Omega = 0$. When an AC force is applied, the force tilts the double-well potential and lowers the barrier during part of the drive cycle. As a result, the hopping rate becomes time-dependent:

$$r_k(t) = r_0 e^{-E_b(F(t))/k_B T}$$

where $E_b(F(t))$ is the instantaneous barrier height in the tilted potential.

Because of this exponential dependence, even a modest reduction of the barrier can increase the hopping rate by many orders of magnitude compared with its zero-drive value. Therefore, the very small values of r_k quoted for $F_\Omega = 0$ do not represent the hopping rate at the maximum force during the drive cycle.

At $F_\Omega = F_c$, the relevant condition is that the integrated hopping probability during the portion of the cycle where the barrier is sufficiently lowered becomes appreciable.

This explains why the critical force F_c depends on the driving frequency. At low frequency, the potential remains tilted for a longer time, so hopping can occur even when the barrier is only moderately reduced, leading to a smaller F_c . At higher frequency, the vortex has less time to hop during each cycle, so the barrier must be reduced further, requiring a larger F_c .

This behavior is included in our Monte Carlo simulations, where the instantaneous barrier height and the corresponding hopping probability are updated throughout the drive cycle. We have clarified in the revised manuscript that the quoted r_k values correspond to the undriven potentials.

5. How does the distribution of the sheet current density change with frequency?

We thank the reviewer for pointing out this relevant point. The current distribution was measured at all the driving frequencies by measuring the magnetic field induced by the circulating current across the superconducting bridge $B_\Omega(x)$. No significant change in the distribution was found as shown in Figure 2 below.

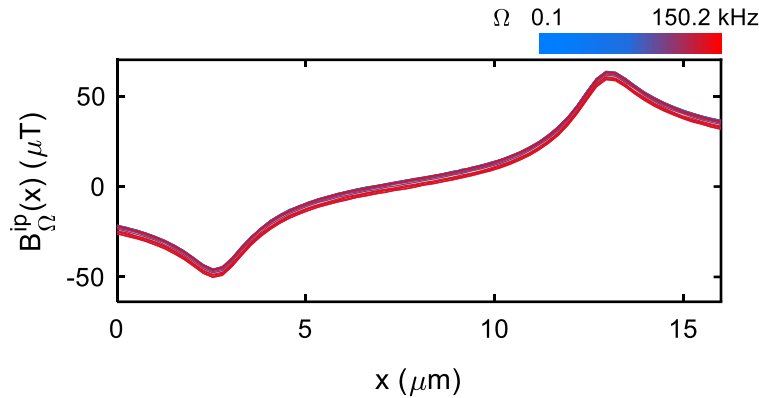


Figure 2 | Out-of-plane magnetic field as a function of position at distinct driving frequencies. In-phase signal of the out-of-plane magnetic field $B_\Omega^{ip}(x)$. The field is measured across the bridge along the x direction. Each measured curve at different driving frequency using the same current (1.2 mA).

6. The coherence length in your experiment is $\xi = 12\text{nm}$, London penetration depth is $L = 220\text{nm}$. Meanwhile you fit $2l_0 = 7.8\text{nm}$ as the distance between the two wells for sample #3 (Supp. Table S2). It means that vortex hardly moves between the two positions. From that point of view the well presented in Fig. 2a is overscaled with respect to the vortex size. Doesn't it require a comment? Is x_0 in the definition of the U_{pin} the same as $2l_0$?

We thank the reviewer for raising this point. We agree that the displacement between the two minima is very small. Indeed, one of the central advantages of the lock-in technique used here is that it allows us to resolve vortex displacements that are much smaller than the apparent width of the vortex in DC magnetic imaging.

The fact that the distance between the two minima is $2l_0 = 7.8\text{ nm}$ for double-well #3 is consistent with the coherence length $\xi = 12\text{ nm}$. The vortex position should be understood as a collective coordinate describing the center of the vortex. This coordinate can shift by a distance smaller than ξ , especially in a smooth or overlapping pinning landscape (see also Ref. 13 and 18). The coherence length sets the characteristic spatial scale over which the superconducting order parameter varies, but it does not impose a lower bound on the displacement of the vortex center between two nearby metastable configurations.

Assuming a Lorentzian potential, the shortest distance between two distinct minima is $2l_0 < \xi/\sqrt{3} \approx 6.9\text{ nm}$, which is smaller than the displacement observed in the strong drive limit. That point was also discussed before in Ref. 13 and in Ref. 18.

Regarding the notation, x_0 and l_0 are not exactly the same quantity. The parameter x_0 enters the calculation of the pinning landscape and controls the separation of the underlying pinning centers used to construct U_{pin} . By contrast, l_0 is obtained from the resulting total potential (sum of two Lorentzian) after minimization and denotes half the distance between the two actual potential minima; the experimentally relevant distance between the two vortex states is therefore l_0 and not x_0 .

In the limit where the two elementary pinning potentials are well separated compared with ξ , the minima of the total potential coincide approximately with the centers of the individual pinning potentials ($x_0 \approx l_0$). However, in the regime relevant here, the two Lorentzian potentials overlap significantly, so the minima of the combined potential are shifted relative to the input parameter x_0 . Therefore x_0 should not be confused with l_0 .

We have revised the method section of the manuscript to clarify this distinction by adding the following phrase:

“Here we distinguish between x_0 the simulation parameter for a given pin and l_0 the distance between the two minima resulting from the sum of the two Lorentzian.”

7. It should be clearly mentioned that Fig.2. is an illustrative drawing of the postulated behavior. None of graphs presented there are measured experimentally or established by a simulation.

We thank the reviewer for pointing this out. We agree that the illustrative nature of Fig. 2 should be stated explicitly. Although the figure was labeled as a toy model, we have now clarified in the caption and in the main text that Fig. 2 is a schematic illustration only. None of the curves shown in this figure correspond to measured experimental data or to the Monte Carlo simulations used for quantitative comparison.

8. It is not clear how authors arrived to the estimation of the “energy penalty” equal to ~ 5 times Landauer’s limit. You notice that too low writing energy cost makes the memory not very reliable. Do you mean that $5 \times$ Landauer limit is necessary to make the memory non-volatile. Please explain.

Yes, we confirm that this is the energy penalty to work with a useful non-volatile memory.

For a functional memory operation, the manipulation must be performed on a timescale shorter than the spontaneous hopping time, i.e., in the regime $\Omega \gg \tau_k$. For the experimentally relevant operating frequency $\frac{\Omega}{2\pi} = 150$ kHz, our analysis gives an average erasure work of approximately $5k_B T \ln 2$. This is what we refer to as the energy penalty: it is the additional work required to operate the vortex as a reliable memory bit.

In my opinion the work is sufficiently original and interesting to appear in Nat. Comm., provided that authors present the additional experimental evidence, mentioned in the report, which will make their “energetic” investigations well-rooted in the convincingly tested experimental system. At the moment I have a feeling that authors did not make their best to give the full credibility to their interpretation and their presentation becomes at times too speculative.

We thank the reviewer for the careful assessment of our revised manuscript and for recognizing the originality and potential interest of the work for *Nature Communications*. We also appreciate the reviewer’s continued effort to help us improve the clarity and credibility of the presentation.

We respectfully disagree, however, that the central interpretation remains speculative or that the additional experiments suggested in the report are necessary prerequisites for the conclusions of the manuscript. In the revised version, the interpretation of the vortex as a stochastic two-state system is supported by several independent experimental observations: the direct observation of telegraphic switching, the appearance of a critical force and nonlinear response, the frequency dependence of the work $W(\Omega)$ and first-harmonic displacement $l_\Omega(\Omega)$, the Lorentzian noise spectrum $B(\omega)$ with independently extracted hopping rates, and the consistency of these results with stochastic-resonance theory and Monte Carlo simulations.

We have also substantially revised the manuscript to make the distinction between measured quantities and model-based interpretation explicit. In particular, the extraction of l_{Ω} and W from the lock-in magnetic response is now described step by step, and the role of the models is clearly limited to interpreting the experimentally measured behavior.

We agree that the additional measurements suggested by the reviewer, such as direct DC imaging of hysteresis or temperature-dependent hopping measurements, could provide useful complementary information in some regimes. However, as discussed in our responses above, these experiments are either not feasible for the relevant low-dissipation double wells, not sufficiently sensitive to the relevant displacement scale, or not a clean test of thermal activation in a superconducting vortex system because changing temperature also modifies the pinning landscape itself.

We have therefore further clarified the scope of our conclusions and avoided overstatement. Our result do not rely on a direct microscopic proof of purely thermal Kramers activation. Rather, we demonstrate experimentally that a single vortex can behave as a stochastic two-state system under controlled drive and that the work associated with its manipulation can be quantified at energy scales close to the Landauer limit. We believe that the revised manuscript is firmly rooted in experimentally measured observables and that the conclusions are appropriately supported by the data presented.

Reviewer #3 (Remarks to the Author):

The authors have revised and improved their manuscript carefully considering all my comments and suggestions. They have provided detailed and well-elaborated responses to my remarks in the first review report.

1. The authors have explicitly clarified a distinction of their continuous sinusoidal driving protocol from the single-shot erasure protocol considered by Landauer and emphasized in the refined Abstract that their numerical analysis demonstrates that the work required for such bit manipulation is within an order of magnitude of Landauer's limit.

2. The authors have convincingly explained that the fluctuations visible in the experimental arise from the finite signal-to-noise ratio of the measurement and become noticeable in the regime of low dissipation, where the measured signal approaches the noise floor of the SQUID-on-tip and the related electronics, and hence reflect instrumental noise rather than intrinsic vortex dynamics. At the same time, their Monte Carlo simulations do not include measurement noise and therefore produce smooth curves. The corresponding statement has been added in the revised manuscript.

3. The authors have performed an additional investigation of the issue that different values of the distance between the two minima are obtained when fitting the displacement and the work in double-well #3. Though the precise origin of this discrepancy is not still fully understood, I find well-grounded the statement of the authors that, as this behaviour is not detected in the six additional double-well systems presented in Supplementary Fig. S5, the discrepancy observed for double-well #3 is not representative of the general behaviour. The corresponding sentence is included in the revised manuscript.

4. The authors have included in the revision an exhaustive explanation of “another factor 2 related to the initial condition of the vortex” in the present protocol as compared to the Landauer's one.

5. Minor remarks concerning several grammatical errors and typos have been carefully taken into account in the revised manuscript.

The revised manuscript is suitable for publication in the Nature Communications in its present form.

We sincerely thank the reviewer for the positive assessment of our revised manuscript and for recommending publication in *Nature Communications*. We are grateful for the reviewer's careful reading and constructive comments, which greatly helped us improve the clarity and quality of the manuscript.