

Probing picosecond depairing currents in type-II superconductors

Corresponding Author: Professor Andrea Cavalleri

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

The manuscript by Wang et al. introduces a novel approach to investigate the intrinsic depairing current density (J_c^*) in type-II superconductors using picosecond electrical transport measurements. The authors present experimental demonstrations of picosecond-scale critical current measurements in both NbN and YBCO. Notably, for NbN, a sharp depairing transition is reported at a current density reaching $J_c^* = 2.2 \times J_c$, which the authors claim aligns well with theoretical predictions derived from BCS microscopic dynamics. In contrast, YBCO exhibits a more gradual suppression of superconductivity with increasing current, a characteristic attributed by the authors to its d-wave gap symmetry. Overall, the work effectively highlights the significant potential of ultrafast current pulses as a powerful new tool for accessing intrinsic superconducting properties.

Despite the intriguing nature of the experimental technique, the manuscript, in its current form, exhibits several areas requiring substantial revision before it can be considered for publication in a high-impact journal. My specific concerns are detailed below:

1. The manuscript lacks sufficient direct characterization of the fundamental parameters for the samples under investigation. The reliance on values adopted from Ref. 4 in the Supplementary Information, without adequate justification for their applicability to the specific samples studied here, introduces considerable uncertainty regarding the validity and robustness of the subsequent analysis and conclusions. Directly measured parameters are crucial for establishing the credibility of the reported intrinsic properties.
2. The characterization of YBCO at 110 K as "gapless" is inaccurate and potentially misleading. The pseudogap is a well-established phenomenon in cuprates above T_c , and evidence suggests that density-wave gaps can persist in YBCO to temperatures significantly exceeding T_c . The authors must revise this terminology and incorporate a comprehensive discussion on how these pseudogap and density-wave gap phenomena might influence or relate to the "partial depairing" observed at 110 K. A deeper understanding of these high-temperature phases is essential for a complete interpretation of the experimental observations in YBCO.
3. The manuscript employs a specific model, detailed in the Supplementary Information, to calculate the depairing current, but fails to clearly explain why this model is chosen over the conventional Ginzburg-Landau (GL) framework. Given the widespread use and established validity of G-L theory, a transparent justification for adopting an alternative approach, particularly in the context of Type-II superconductors, is imperative. This justification should highlight the specific advantages or unique insights offered by the chosen model.
4. The observed extremely close agreement between the calculated depairing current values and the experimental data is striking. Given that the theoretical model, while capturing the scales of superfluid density and coherence length, is generally regarded as providing order-of-magnitude estimates, the authors must clarify whether this exceptional agreement is coincidental or if the model indeed possesses a higher degree of precision than typically assumed. A detailed explanation of the theoretical refinements or experimental controls that enable such precise correspondence is needed.
5. The NbN films investigated have a width of $\sim 10 \mu\text{m}$, which is significantly larger than the typical coherence length ξ of NbN. The manuscript should explain why coherence-length effects can be neglected under these conditions. The manuscript currently does not adequately explain why coherence-length effects, which are usually crucial in understanding current distributions and critical phenomena in superconductors with dimensions comparable to ξ , can be neglected under these conditions. A thorough discussion on this point is required.
6. It is highly probable that the NbN films studied operate in the dirty limit, given the material's inherent disorder. However, the formula employed by the authors for $\xi(T)$ appears to correspond to the clean-limit case or, at minimum, does not adequately incorporate the scattering effects that are dominant in the dirty regime. This fundamental discrepancy requires immediate clarification and, most likely, correction to ensure the theoretical model accurately reflects the physical properties of the NbN samples.
7. A significant inconsistency exists between the reported values of J_c . Fig. S5 indicates a critical current density of approximately $\sim 20 \text{ GA/m}^2$ at 7.5 K, whereas Fig. 5a reports a value closer to 100 GA/m^2 at the same temperature. This

substantial discrepancy must be clearly resolved, with a detailed explanation provided for the difference.

8. Similarly, an inconsistency is present in the reported J_c^* values. Fig. S11d shows J_c^* exceeding 200 GA/m² at 8.5 K, while Fig. 5a presents J_c^* slightly below 200 GA/m² at 8.5 K. This disagreement, though less pronounced than for J_c , still requires a clear explanation and reconciliation.

9. At line 100, the manuscript states that the critical current density of YBCO at 50 K is “approximately 40 GA/m²”. According to Fig. 2b, the actual value is closer to 50 GA/m². This numerical discrepancy should be corrected for accuracy.

In summary, while the manuscript presents an intriguing experimental technique with clear potential for advancing the study of intrinsic superconducting properties, the work is significantly weakened by a lack of rigorous sample characterization, questionable assumptions regarding YBCO's high-temperature behavior, an unjustified preference for a specific theoretical model, and multiple unresolved inconsistencies between the figures and the main text. These fundamental issues preclude recommendation for publication in its current form. Thoroughly addressing these points is essential to validate the results and strengthen the manuscript's scientific contribution..

Reviewer #2

(Remarks to the Author)

In the manuscript, the authors investigate the critical current density in type-II superconductors, NbN and YBCO, which are representative of s-wave and d-wave superconductors, respectively. To access the intrinsic depairing current (J_c^*), the authors employ picosecond electrical pulses generated in on-chip waveguides by photoconductive switches. They find that the critical currents probed by picosecond pulses (J_c^*) are larger than those measured by conventional direct current (J_c), because in the former case the electrical pulses interact with the samples on an ultrafast timescale where the contributions from vortices and self-heating are negligible. In addition, the authors observe a gradual suppression of superconductivity in YBCO as a function of current density, which they attribute to the nodal d-wave symmetry of the superconducting gap.

The idea of accessing the intrinsic depairing current through ultrafast nonlinear transport is interesting, and the experiments appear to have been conducted carefully. Therefore, I believe the manuscript deserves publication in some form. However, in terms of impact and broad interest, I am not convinced that it is suitable for Nature Physics. In particular, I am not persuaded that the reported experiment offers a new approach to probing the gap symmetry, since a gradual suppression of superconductivity has also been observed in previous studies of K3C60, which has a distinct gap symmetry from YBCO. Furthermore, I also find several points unclear or inconsistent, which make the interpretation of the results less convincing. Hence, in its present form, I cannot recommend the manuscript for publication in Nature Physics.

Specific questions and comments are provided below:

1. While I find the discussion in lines 207-217 reasonable, the attribution of the discrepancy between J_c^* and J_c to vortex motion or self-heating is not directly supported by the experimental data. On the other hand, a recent study using ultrafast-current pump-probe spectroscopy reported a similar observation, where the large picosecond current density required to suppress superconductivity was explained by the intrinsic ultrafast dynamics of the order parameter, as described by the time-domain Ginzburg-Landau (TDGL) equation. Such a scenario is distinct from the interpretation provided by the present work, where the authors assume that “the order parameter relaxes sufficiently fast that we can treat the dynamics as essentially adiabatic — that is they follow the equilibrium behavior at each point in time as a function of the current.” Is this assumption reasonable? While the GL equation is less valid at sufficiently low temperatures, is it still possible that, even within the BCS framework, the intrinsic J_c^* becomes larger on a picosecond ultrafast timescale compared to the DC regime?

2. In this work, the temperature-dependent depairing current J_c^* is calculated within the BCS framework, which accounts for the influence of the supercurrent but assumes thermal equilibrium. Despite its relatively simple formalism, the calculated result perfectly reproduces the experimental data. My question is, how strictly is this calculation valid (in particular, compared to the theoretical works raised in the main text)? For example, the authors evaluate J_c^* by assuming that, at J_c^* , the quasiparticle energy shift equals the superconducting energy gap. On the other hand, it is known that the superconducting gap remains finite at the depairing current density [shown in Ref. 15, for example]. Is this fact consistent with the authors' formalism? In addition, the assumption of the clean limit of the superconductivity does not hold for NbN, which is in the dirty limit.

3. The authors attribute the lack of a well-defined critical current J_c^* (the gradual suppression of superconductivity as a function of the picosecond current), as shown in Fig. 4c-d, to the nodal d-wave superconducting gap (“gapless” as the authors refer to it). On the other hand, a clear threshold J_c can be identified at a considerable current density of 40-50 GA/m² in the conventional DC measurement, as shown in Fig. 2b. This seems inconsistent, and I cannot find a plausible explanation that reconciles this discrepancy. If there is no intrinsic critical current, a threshold current J_c cannot appear in the DC measurement.

4. The YBCO sample was deposited by dc magnetron sputtering. Therefore, I guess the sample is not a single crystal but rather a polycrystalline film. If so, could this have any influence on the nonlinear transport? For example, the anisotropy originating from the d-wave symmetry might be averaged out if the grains are randomly oriented. Furthermore, is the nature of granular superconductivity, reported by the authors in their previous studies of K3C60, irrelevant to the present work?

5. Throughout the manuscript, the authors only discuss the pulse peak values (such as E_T , peak and J_T , peak). What about the spectral information? Any fingerprint of superconductivity, and any difference between s-wave and d-wave

symmetry?

6. The data in Fig. S5 of the Supplementary information appear to be inconsistent with those in Fig. 2a of the main text. Could the authors comment on this point?

Minor comments:

7. A question regarding the data acquisition procedure: after the transimpedance amplifier, how is the signal measured? Do the authors use a lock-in amplifier or a digital oscilloscope? In addition, the authors mention the calibration procedures in their previous work (Ref. [2] in the Supplementary information). However, according to the equation in S4, it seems that the calibration discussed in Ref. [2] is not necessary, but the peak current amplitude can be directly obtained from the conversion equation between I_{peak} and $V_{\text{signal,peak}}$.

8. I recommend that the authors show the superconducting transition temperature T_c of NbN and YBCO earlier in the manuscript, probably around lines 99-100 or in the caption of Fig. 2.

9. What is the reason that the measurement of the YBCO sample was limited to relatively high temperatures above 50 K?

10. What is the normalization factor for the vertical axes (Norm. Ein) in Figs. 3b,3c?

11. In line 162, the authors state that “The slight leftward deviation observed during the sharp drop can be attributed to the reduction in the transmitted peak current amplitude...” Here, is the word “deviation” appropriate? The authors might consider using another word, such as “tilt” or something else.

12. In Figs. 4a and 4c, the normalized transmitted electric peak field ($E_{\text{T, peak}}$) is plotted as a function of transmitted peak current densities ($J_{\text{T, peak}}$). I assume that these plots correspond to the electric field transmittance depending on the current density flowing in the sample. It would be helpful for readers if the authors could mention this explicitly, because the physical meaning of the quantities is not immediately clear.

13. In line 185, the authors refer to the d-wave superconductor as “gapless.” However, I think this is not appropriate. Rather, d-wave is “nodal”.

14. In S2 (and in Fig. S4) of the Supplementary information, what is the pulse width of the current? I find Fig. S4a ambiguous, as it only shows the “onset” of the applied current. In addition, I cannot catch how the authors safely excluded the heating effect based on Fig. S4b. How do the authors explain the delay-time dependence observed in Fig. S4b?

15. I cannot clearly understand what the authors mean in the last part of S4 of the Supplementary information. Does the sentence “the factor 0.85 is taking into account the average value between the pulses reaching the photoconductive switches and the pulses transmitting through the switches ($\approx 30\%$ reflection due to local impedance mismatch)” mean that the authors have compensated for the reflection loss? The sentence “the correlation effect between the current pulse time profile and the switch response time profile” is also ambiguous. Does “current pulse time profile” refer to the observed data, or to the actual time profile of the real current pulse?

16. What is Z_{T} in equation (9) in the Supplementary information?

17. In the last page in the Supplementary information, the authors say that “In the microscopic model, we have obtained...To do so, we express the current in units of depairing current...” Here, it is not clear what the authors mean by “To do so”.

In summary, substantial revisions are required before the manuscript can be further considered for publication.

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

I have carefully reviewed the revised manuscript, the updated supplemental information, and the authors' detailed point-by-point rebuttal. I would like to begin by commending the authors for the substantial effort invested in this revision. The inclusion of the “notch-strip” experiment is a particularly strong addition, and the direct experimental characterization of the coherence length and penetration depth provides a significantly stronger foundation for the quantitative comparison between theory and experiment.

I am aware that raising further points at this stage may appear stringent. However, to ensure the manuscript fully meets the level of physical rigor expected for Nature Physics, I believe several conceptual aspects would benefit from further clarification before final acceptance.

1. The interpretation of the gradual suppression of superconductivity in YBCO remains largely qualitative, especially when compared to the more rigorous treatment presented for NbN. The revision from the term “gapless” to a description of the gap

evolving from nodal to antinodal regions is appropriate and appreciated. However, the underlying non-equilibrium dynamics of nodal quasiparticles under strong current injection are not yet sufficiently addressed. Unlike the s-wave case, where a sharp threshold behavior is well captured within the BCS/Usadel framework, the YBCO response is currently attributed to gap nodes in a largely descriptive manner. Providing even a simplified quantitative estimate, or a more explicit theoretical narrative describing how the nodal quasiparticle population evolves during the pulse, would significantly strengthen the connection between experiment and interpretation, and help place the two material systems on more equal footing.

2. The claim that the presented approach constitutes a “powerful new probe” of pairing symmetry appears somewhat overstated in its current form. While the contrasting behaviors observed in NbN and YBCO are compelling, the study effectively compares only a single representative of each symmetry class. In the absence of broader material validation or a more general theoretical framework, I recommend moderating this statement. Framing the method as a promising approach demonstrated in representative s-wave and d-wave systems would preserve the impact of the work while maintaining appropriate caution in introducing a new experimental paradigm.

3. While the success of the “adiabatic approximation” in reproducing the experimental J^* values is convincing, the manuscript would benefit from a more explicit discussion of the underlying non-equilibrium physics. Even if the order parameter can adiabatically follow the current pulse, the excitation and relaxation pathways of quasiparticles on picosecond timescales remain central to the physics of high-field transport. A concise discussion addressing relevant energy relaxation timescales and the possible role of non-thermal carrier distributions would substantially broaden the relevance of the work, particularly for the ultrafast and non-equilibrium superconductivity communities.

4. As a technical point, the theoretical curves shown in Fig. 5 (and Fig. R4) appear noticeably piecewise-linear rather than smooth. Given that the abrupt collapse of superconductivity in the s-wave NbN system is a central result, it would be helpful for the authors to clarify whether these features arise from numerical discretization or reflect an intrinsic instability captured by the Usadel/tDGL framework.

In summary, the authors have addressed nearly all of the technical concerns raised in the first round of review. With further refinement of the discussion regarding YBCO's nodal dynamics, and a moderation of the claims concerning general applicability, I believe this work will constitute a definitive and impactful contribution to the field of nonlinear superconducting transport and ultrafast superconductivity.

Reviewer #2

(Remarks to the Author)

The authors have properly addressed most of the questions and comments raised by the Reviewers. However, I still find several points unclear or inconsistent, which should be clarified before publication.

1. In the model of the equivalent circuit of the sample and waveguide, the authors assume $L_{n0} = L_{s0}$. This indicates that the kinetic inductances of the normal carriers and Cooper pairs are identical, which sounds implausible. Is this assumption justified?

2. The authors claim that the current distribution across the samples is uniform because the sample widths are smaller than the Pearl length. However, in this case, a vortex cannot be pinned in the sample, since it induces a non-uniform screening current. Therefore, attributing the smaller critical current in the DC regime to the depinning process does not seem to be applicable. Could the authors comment on this point?

3. In their reply, the authors state: “The NbN film is indeed in the dirty limit. In the revised manuscript we modified the temperature dependence of $\xi(T)$ to the dirty limit. As can be seen the calculated temperature dependence of J_c^* is still consistent with the data. This is because the drop of superfluid density at higher temperatures is dominating the decrease of the depairing current.” I find this explanation misleading. As far as I understand, the authors calculate $J_c^*(T)$ in Fig. 5a in the same way as in the previous version. The change in the revised manuscript appears to arise only from a slight modification of the values of the penetration depth and coherence length. The theoretical improvement resulting from assuming the dirty limit instead of the clean limit should appear in Fig. 5b. However, in Fig. 5b, the theory and experiment show perfect agreement, as was the case in the previous version. This seems to indicate that whether the sample belongs to the dirty or clean limit hardly affects the depairing current. Is this result reasonable?

4. Related to question 3, I am also confused about the consistency between sections S9 and S10.1 in the SI. In S9, the authors calculate the depairing current based on $J^*c = ns$ vs $(2e)$, which appears to be an order-of-magnitude estimate, and the calculated results are plotted in Fig. 5a. On the other hand, in S10.1 the authors present a dynamic model within BCS framework. How were the results derived in S10.1 used in the main text?

Minor questions and comments,

5. The authors state that “the order parameter relaxes sufficiently fast that we can treat the dynamics as essentially adiabatic.” Do the authors assume that quasi-thermal equilibrium is reached in the total system, i.e., not only within the electron subsystem but also including the lattice system?

6. I cannot clearly understand the author's reply to question 7 by Reviewer 2. In the SI, the authors state: “The calibration procedures are discussed in detail in Ref. [2, 3].” In Ref [2], information such as the pulse duration and repetition rate was necessary for the calibration. However, in the present work these parameters do not appear in Eq. (1) in the SI. Could the authors explain the calibration procedure more clearly?

From a more fundamental viewpoint, I find that attributing the different behavior between NbN and YBCO to the difference in

gap symmetry remains speculative. The detailed analysis and theoretical calculations are limited to NbN. Simulations of the depairing current in a d-wave superconductor would be highly desirable to confirm this interpretation.

On the other hand, I find the experimental method presented by this work, evaluating the depairing current by using ultrafast current pulses, to be novel and interesting. Therefore, after the authors have addressed the issues raised above, I think the paper would be suitable for publication in Nature Communications.

Version 2:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

I have read the revised manuscript and the authors' response to my previous comments. I am satisfied that the main points I raised have been addressed.

The authors have toned down the broader claims, added a more careful discussion of the YBCO results, and clarified the numerical origin of the piecewise features in the theoretical curves. The remaining limitations, particularly the lack of a full microscopic treatment of the d-wave case, are now stated appropriately and do not, in my view, prevent publication.

I therefore think the manuscript is now suitable for publication in Nature Physics.

Reviewer #2

(Remarks to the Author)

While the authors have properly addressed some Reviewers' comments, I still have some questions that should be clarified before publication:

1. In their response to Comment 2 by Reviewer 2, the authors might have misunderstood the main issue. In samples with a width smaller than the Pearl length, where the current distribution is nearly uniform, the circulating current around a vortex spreads across the entire sample. Under this condition, a trap potential arising from impurities cannot effectively pin the vortex. Rather, other phenomena, such as vortex entry from the sample edges and the emergence of phase slip centers, will become relevant. This point should be discussed not only in the reply but in the main text or Supplementary information. In addition, I would recommend that the authors include their nice results for the notched NbN strip sample in Supplementary information.

2. I am confused by the authors' reply to Comment 3 by Reviewer 2, "As a result, the main revision concerns the temperature dependence of $\xi(T)$, rather than its absolute value." The calculation in S9. "Calculation of J_c^* ", which was used to obtain the results in Fig. 5, remains unchanged from the originally submitted version. Therefore, the claimed revision by considering the dirty-limit coherence length is not clear. Could the authors clarify this point?

3. I find the authors' reply to Comment 6 by Reviewer 2 helpful, so I recommend that the authors include it in the Supplementary information.

4. Concerning the authors' reply to Comment 1 by Reviewer 1 and the last comment by Reviewer 2: While I understand that a quantitative calculation of the nonequilibrium depairing dynamics in a d-wave superconductor is highly challenging, it is not clear why the tDGL framework is considered to be suitable, at least qualitatively, for describing the dynamics of a d-wave gap system. Do the authors assume that a nodal gap system can be treated as an effectively high-temperature system?

After addressing the issues raised above, the manuscript will be ready for publication.

Version 3:

Reviewer comments:

Reviewer #2

(Remarks to the Author)

The authors have addressed the reviewers' comments raised in the previous round. However, I still find an inconsistency in the authors' claim concerning the uniformity of the supercurrent. The authors state that the screening current density around a vortex varies rapidly over a length scale of 100 nm-1 μ m. On the other hand, they also argue that the current density in their sample is nearly uniform because the Pearl length exceeds the sample's width. How can these two statements be reconciled?

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Reviewer comments:

Reviewer #1 (Remarks to the Author):

The manuscript by Wang et al. introduces a novel approach to investigate the intrinsic depairing current density (J_c^*) in type-II superconductors using picosecond electrical transport measurements. The authors present experimental demonstrations of picosecond-scale critical current measurements in both NbN and YBCO. Notably, for NbN, a sharp depairing transition is reported at a current density reaching $J_c^* = 2.2 \times J_c$, which the authors claim aligns well with theoretical predictions derived from BCS microscopic dynamics. In contrast, YBCO exhibits a more gradual suppression of superconductivity with increasing current, a characteristic attributed by the authors to its d-wave gap symmetry. Overall, the work effectively highlights the significant potential of ultrafast current pulses as a powerful new tool for accessing intrinsic superconducting properties.

Despite the intriguing nature of the experimental technique, the manuscript, in its current form, exhibits several areas requiring substantial revision before it can be considered for publication in a high-impact journal. My specific concerns are detailed below:

We thank the referee for the positive comments. We also realize that many of the issues raised by this referee are appropriate and have tried to improve our manuscript. We expect that the referee will be pleased with these improvements. Below we answer the questions point-by-point.

1. The manuscript lacks sufficient direct characterization of the fundamental parameters for the samples under investigation. The reliance on values adopted from Ref. 4 in the Supplementary Information, without adequate justification for their applicability to the specific samples studied here, introduces considerable uncertainty regarding the validity and robustness of the subsequent analysis and conclusions. Directly measured parameters are crucial for establishing the credibility of the reported intrinsic properties.

Both NbN thin film samples studied here and the ones from Ref. 4 are epitaxially grown on MgO substrates and have similar T_c , so we expect their microscopic parameters to be close to literature values. We have characterized these parameters as requested.

1) Regarding the coherence length $\xi(0)$, we now measured the superconducting transition in a magnetic field. From this we have extracted $B_{c2}(0)$, from which $\xi(0)$ can be directly calculated. The results are shown in Fig. R1. We took the magnetic field when the sample resistance reaches half of the normal state resistance as B_{c2} and follow the Werthamer-Helfand-Hohenberg (WHH) theory [N. R. Werthamer, E. Helfand, and P. C. Hohenberg, Temperature and Purity Dependence of the Superconducting Critical Field, Hc2. III. Electron Spin and Spin-Orbit Effects, Phys. Rev. 147, 295 (1966)], same as Ref. 4.

$$B_{c2}(0) = -0.69T_c \left. \frac{dB_{c2}}{dT} \right|_{T=T_c} \quad (1)$$

$$\xi(0) = \left[\frac{\Phi_0}{2\pi B_{c2}(0)} \right]^{1/2} \quad (2)$$

We extract $B_{c2}(0) = 12 \text{ T}$ and $\xi(0) = 5.24 \text{ nm}$, close to 5.3 nm from Ref. 4.

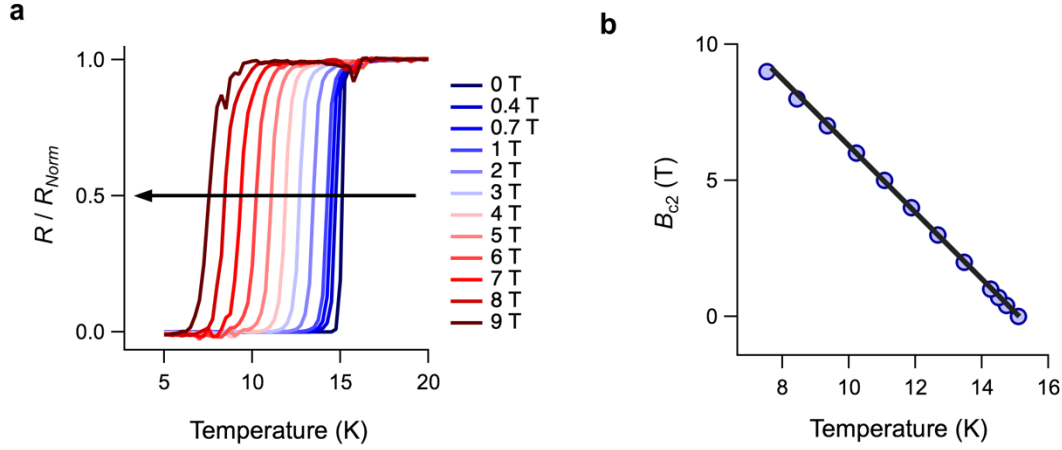


Figure R1| Measurements of B_{c2} at different temperatures. (a) Superconducting transition for various applied magnetic fields. (b) Extracted B_{c2} values versus temperature.

2) Regarding the magnetic penetration depth $\lambda(0)$, we extracted its value from the sample's kinetic inductance from the equation below [R. Meservey, P. M. Tedrow, Measurements of the Kinetic Inductance of Superconducting Linear Structures, J. Appl. Phys. 40, 2028 (1969)].

$$L_{kin} = \frac{m}{2n_s e^2} \cdot \frac{l}{s} = \frac{\mu_0 \lambda^2 l}{s} \quad (3)$$

Where m is the effective mass of the electron; n_s is the superfluid density; e is electron charge; l is the length of strip; s is the cross section of the strip; μ_0 is the vacuum magnetic permeability; and λ is the magnetic penetration depth.

The kinetic inductance of the sample is extracted from the measured reflected pulse shown in Fig. 3b of the main text. Below are the data and the simulated reflected signal with $L_{kin}(0) = 23 \text{ pH}$, with matching amplitude. This corresponds to $\lambda(0) = 350 \text{ nm}$, which is close to the value of 360 nm we took from Ref. 4. The detailed simulation procedure is described below.

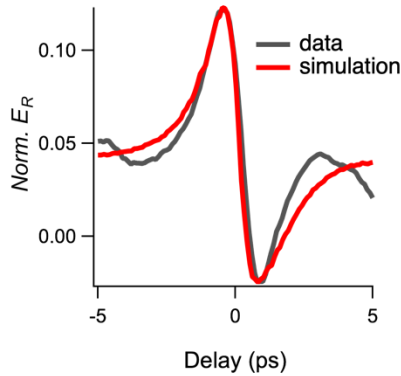


Figure R2| Simulation of the reflected pulse at 7 K.

The equivalent circuit of the sample together with the coplanar waveguide is shown in Fig. R3. The sample is treated using a two-fluid model. R_n and L_n are the resistance and kinetic inductance of the normal channel; L_s is the kinetic inductance of the superconducting channel and Z_0 is the impedance of the coplanar waveguide, which is 50Ω . R_n , L_n and L_s are directly related with the superfluid density as $R_n = R_{n0}/(1 - n_s)$, $L_n = L_{n0}/(1 - n_s)$ and $L_s = L_{s0}/n_s$, where R_{n0} and L_{n0} are the sample's resistance and kinetic inductance when all carriers are normal quasiparticles, and L_{s0} is the sample's kinetic inductance when all carriers are Cooper pairs. We take $R_{n0} = 140 \Omega$, from DC transport measurement. $L_{n0} = L_{s0}$ is treated as a variable.

The reflection and transmission of current pulses at the sample are described by the equations below.

$$I_{in}(t) - I_R(t) = I_s(t) + I_n(t) \quad (4)$$

$$I_T(t) = I_s(t) + I_n(t) \quad (5)$$

$$V_{in}(t) + V_R(t) - V_T(t) = L_s \cdot \frac{dI_s(t)}{dt} \quad (6)$$

$$V_{in}(t) + V_R(t) - V_T(t) = L_n \cdot \frac{dI_n(t)}{dt} + R_n \cdot I_n(t) \quad (7)$$

Where, $V_{in}(t) = I(t) \cdot Z_0$, $V_R(t) = I_R(t) \cdot Z_0$ and $V_T(t) = I_T(t) \cdot Z_0$.

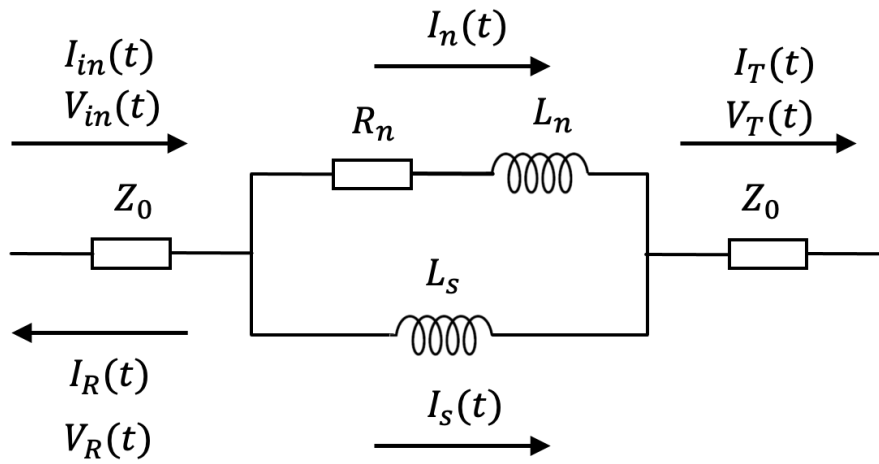


Figure R3 | Circuit model of the sample.

With the incoming current $I_{in}(t)$ shown in Fig. 3b as input, the reflected current $I_R(t)$ is calculated from the differential equations (4 - 7). The calculated curve with $L_{s0} = 23 \text{ pH}$ fits the data best and is shown in Fig. R2

In the modified manuscript we replaced the values of $\xi(0)$ and $\lambda(0)$ with the values extracted above for the calculation of J_c^* . We also added the characterization measurements of the sample described above in the supplementary information section S8.

2. The characterization of YBCO at 110 K as "gapless" is inaccurate and potentially misleading. The pseudogap is a well-established phenomenon in cuprates above T_c , and evidence suggests that density-wave gaps can persist in YBCO to temperatures significantly exceeding T_c . The authors must revise this terminology and incorporate a comprehensive discussion on how these pseudogap and density-wave gap phenomena might influence or relate to the "partial depairing" observed at 110 K. A deeper understanding of these high-temperature phases is essential for a complete interpretation of the experimental observations in YBCO.

As the referee surely appreciates, with the term "gapless", we were loosely referring to the fact that for YBCO at 50 K the gap closes at the nodes. The referee is correct that these are unprecise statements about a material whose normal state is not well understood. As suggested by referee 2 we have changed the simple characterization term "gapless" to the more precise description "superconducting gap evolves from zero at the nodal point to its maximal value at the anti-nodal point" to avoid confusion.

3. The manuscript employs a specific model, detailed in the Supplementary Information, to calculate the depairing current, but fails to clearly explain why this model is chosen over the conventional Ginzburg-Landau (GL) framework. Given the widespread use and established validity of G-L theory, a transparent justification for adopting an alternative approach, particularly in the context of Type-II superconductors, is imperative. This justification should highlight the specific advantages or unique insights offered by the chosen model.

In the following we compare the time-dependent Ginzburg-Landau (tDGL) and the microscopic BCS theory in the dirty limit based on the Usadel equations (K.D. Usadel, *Phy. Rev. Lett.*, 25, 507(1970)). As we can see tDGL cannot capture the sharp drop of transmittance, while the microscopic theory captures the non-linearity much better. This is because, tDGL theory relies on the expansion in the power of the order parameter and is valid only near T_c [V. L. Ginzburg and L. D. Landau, *On the Theory of superconductivity*, *Zh. Eksp. Teor. Fiz.* 20, 1064 (1950); L. P. Gor'kov, *Microscopic derivation of the Ginzburg-Landau equations in the theory of superconductivity*, *Sov. Phys. JETP* 9, 1364 (1959)]. This is not the case for our measurements on NbN performed at 7 K, which is below $0.5 \times T_c$.

Below we also present the simulations based on BCS theory in dirty limit and tDGL theory at $T = 7$ K and 12 K, together with the measurement data. As can be seen, the simulations based on these two theories become similar at $T = 12$ K near T_c .

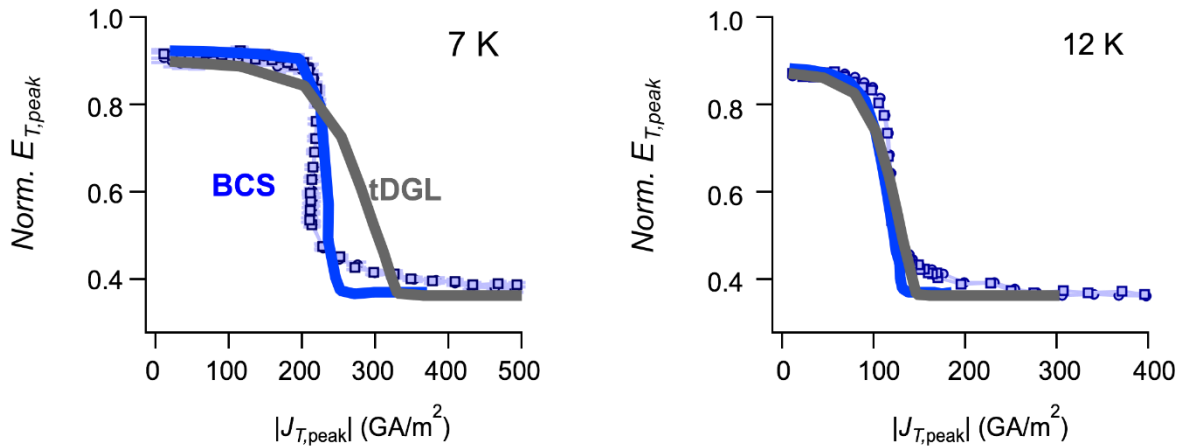


Figure R4. Comparison of simulation results based on BCS theory in dirty limit and tDGL theory. The simulation results based on BCS theory in dirty limit and tDGL theory are plotted in blue and grey colors. The measurement data is plotted in light blue.

4. The observed extremely close agreement between the calculated depairing current values and the experimental data is striking. Given that the theoretical model, while capturing the scales of superfluid density and coherence length, is generally regarded as providing order-of-magnitude estimates, the authors must clarify whether this exceptional agreement is coincidental or if the model indeed possesses a higher degree of precision than typically assumed. A detailed explanation of the theoretical refinements or experimental controls that enable such precise correspondence is needed.

It is difficult to identify key factors when an experiment fits a theory. However, we want to emphasize two points, which in our opinion, are very important and which also took us much effort. First, the accuracy of the calibration method is critical. We have confirmed this by adding an external offset current and then checking the offset of the picosecond current response (see our comments to question 15 of Reviewer #2). Second, it is mandatory to achieve a negligible contact resistance so that the observed response change is solely caused by the sample itself.

In the modified manuscript we have added the validation of the switch calibration method and the discussion of contact resistance in the supplementary information.

5. The NbN films investigated have a width of $\sim 10\text{ }\mu\text{m}$, which is significantly larger than the typical coherence length ξ of NbN. The manuscript should explain why coherence-length effects can be neglected under these conditions. The manuscript currently does not adequately explain why coherence-length effects, which are usually crucial in understanding current distributions and critical phenomena in superconductors with dimensions comparable to ξ , can be neglected under these conditions. A thorough discussion on this point is required.

The current distribution across the cross section of the superconducting film, is determined by the Pearl length $P_L = \frac{2\lambda^2}{d}$, where λ is the magnetic penetration depth and d is the film thickness [Pearl, J. *Current distribution in superconducting films carrying quantized fluxoids. Appl. Phys. Lett.* 5, 65–66 (1964).]. From equation (3) shown above, $P_L = \frac{2w}{\mu_0 l} L_{kin}$, where w and l is the width and length of the strip. With $L_{kin}(0) = 23\text{ pH}$ extracted from sample's kinetic response, we get $P_L(0) = 12.2\text{ }\mu\text{m}$ and $P_L(7\text{ K}) = 13\text{ }\mu\text{m}$, which is larger than sample's width. Therefore, the current distribution across the sample is uniform. This part of the discussion has been added to the supplementary information.

We also note that that the intuition of the reviewer is correct. The depairing current observed here is closely related with the coherence length. From the definition of J_c^* , it corresponds to the situation when the phase gradient along the current direction, is on the order of $\frac{2\pi}{\xi}$. This is an alternative way to understand the depairing current.

6. It is highly probable that the NbN films studied operate in the dirty limit, given the material's inherent disorder. However, the formula employed by the authors for $\xi(T)$ appears to correspond to the clean-limit case or, at minimum, does not adequately incorporate the scattering effects that are dominant in the dirty regime. This fundamental discrepancy requires immediate clarification and, most likely, correction to ensure the theoretical model accurately reflects the physical properties of the NbN samples.

The NbN film is indeed in the dirty limit. In the revised manuscript we modified the temperature dependence of $\xi(T)$ to the dirty limit. As can be seen the calculated temperature dependence of J_c^* is still consistent with the data. This is because the drop of superfluid density at higher temperatures is dominating the decrease of the depairing current.

7. A significant inconsistency exists between the reported values of J_c . Fig. S5 indicates a critical current density of approximately $\sim 20\text{ GA/m}^2$ at 7.5 K, whereas Fig. 5a reports a value closer to 100 GA/m^2 at the same temperature. This substantial discrepancy must be clearly resolved, with a detailed explanation provided for the difference.

This is an oversight on our part. The x-axis value shown in Fig. S5 is the current applied to the sample in mA and is not converted into current density by dividing by the sample's cross section ($20\text{ nm} \times 10\text{ }\mu\text{m}$). We have corrected this in the modified version.

8. Similarly, an inconsistency is present in the reported J_c^* values. Fig. S11d shows J_c^* exceeding 200 GA/m^2 at 8.5 K, while Fig. 5a presents J_c^* slightly below 200 GA/m^2 at 8.5 K. This disagreement, though less pronounced than for J_c , still requires a clear explanation and reconciliation.

The depairing current J_c^* is indeed slightly above 200 GA/m^2 . We took the value of the local minimum point instead of local maximum point. We have corrected this in the modified main text.

9. At line 100, the manuscript states that the critical current density of YBCO at 50 K is “approximately 40 GA/m²”. According to Fig. 2b, the actual value is closer to 50 GA/m². This numerical discrepancy should be corrected for accuracy.

We have corrected this inaccuracy and now write “slightly below 50 GA/m²”.

In summary, while the manuscript presents an intriguing experimental technique with clear potential for advancing the study of intrinsic superconducting properties, the work is significantly weakened by a lack of rigorous sample characterization, questionable assumptions regarding YBCO's high-temperature behavior, an unjustified preference for a specific theoretical model, and multiple unresolved inconsistencies between the figures and the main text. These fundamental issues preclude recommendation for publication in its current form. Thoroughly addressing these points is essential to validate the results and strengthen the manuscript's scientific contribution.

We think we have answered the questions regarding the sample characterization, the partial depairing observed in YBCO below T_c , the justification of our model, and corrected the inconsistencies. We hope that the reviewer will provide an unreservedly positive assessment.

Reviewer #2 (Remarks to the Author):

In the manuscript, the authors investigate the critical current density in type-II superconductors, NbN and YBCO, which are representative of s-wave and d-wave superconductors, respectively. To access the intrinsic depairing current (J_c^*), the authors employ picosecond electrical pulses generated in on-chip waveguides by photoconductive switches. They find that the critical currents probed by picosecond pulses (J_c^*) are larger than those measured by conventional direct current (J_c), because in the former case the electrical pulses interact with the samples on an ultrafast timescale where the contributions from vortices and self-heating are negligible. In addition, the authors observe a gradual suppression of superconductivity in YBCO as a function of current density, which they attribute to the nodal d-wave symmetry of the superconducting gap.

The idea of accessing the intrinsic depairing current through ultrafast nonlinear transport is interesting, and the experiments appear to have been conducted carefully. Therefore, I believe the manuscript deserves publication in some form. However, in terms of impact and broad interest, I am not convinced that it is suitable for Nature Physics. In particular, I am not persuaded that the reported experiment offers a new approach to probing the gap symmetry, since a gradual suppression of superconductivity has also been observed in previous studies of K₃C₆₀, which has a distinct gap symmetry from YBCO. Furthermore, I also find several points unclear or inconsistent, which make the interpretation of the results less convincing. Hence, in its present form, I cannot recommend the manuscript for publication in Nature Physics.

We thank the referee for the positive comment on our manuscript and will address all the issues raised in the report. We expect a positive recommendation to result from this rebuttal and changes to the manuscript.

Indeed, a gradual suppression of superconductivity above a threshold has been observed in previous studies on **polycrystalline** K₃C₆₀ thin films (see AFM measurement shown in Supplementary information Fig. S14 in *Nat. Comm.* 14, 7233 (2023)). As discussed in the supplementary information section S6.3 of that paper, the nonlinear current response is interpreted as indicative of current-induced phase slips at the inter-domain boundaries.

However, granular superconductivity does not play an important role in electrical transport properties of the thin films studied here. Specifically, we would like to point out three important considerations.

First, the granular nature of superconductivity in K_3C_{60} is underscored by the temperature dependance of the resistivity, which exhibits a broad transition from the superconducting to the normal state (see Fig. R5). In contrast, both the NbN and YBCO thin films studied here exhibit a sharp transition from the superconducting to the normal state (see Fig. R5). Already at this level, one does not expect granularity to play an important role in either material.

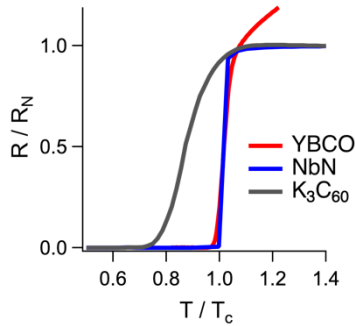


Figure R5] Comparison of the superconducting transition in polycrystalline K_3C_{60} , and monocrystalline NbN and YBCO.

Second, this is further supported by x-ray diffraction measurements, which show that NbN and YBCO —epitaxially grown on MgO and r-cut Al_2O_3 (with 10 nm CeO_2 buffer layer) substrates, respectively— are **monocrystalline** (see Figs. R6 and R7).

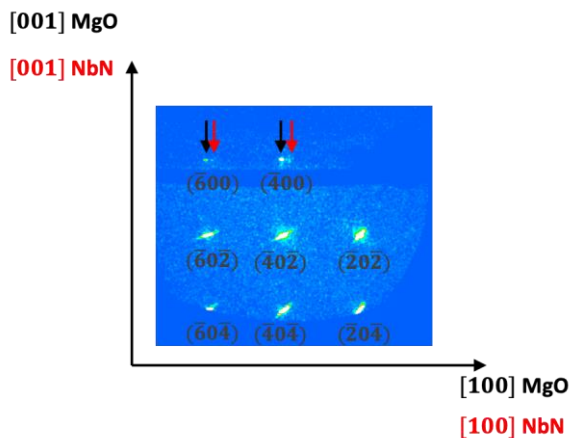


Figure R6] XRD pattern of the NbN film grown on MgO. A clear separation between the Bragg peaks of NbN and MgO can be observed at $(\bar{6}00)$ and (400) . As NbN and MgO both have cubic lattice structure but with different lattice constant ($a_{NbN} \approx 4.4 \text{ \AA}$ and $a_{MgO} \approx 4.2 \text{ \AA}$), the other Bragg peaks cannot be clearly separated. Instead, these Bragg peaks are stretched towards the origin as expected for such a configuration.

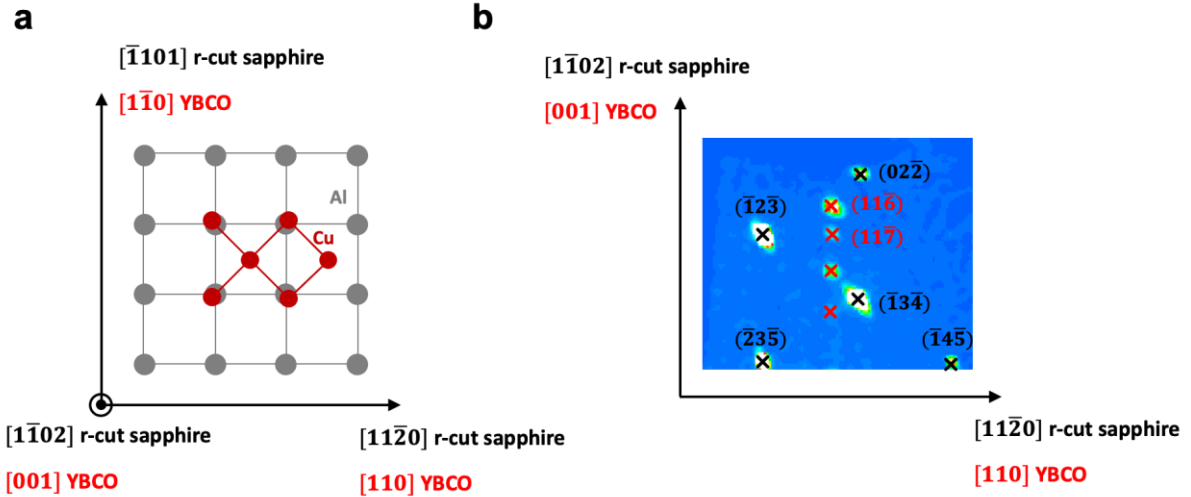


Figure R7| XRD pattern of the YBCO film grown on r-cut sapphire with CeO_2 buffer layer. (a) Illustration of the arrangement of Cu atoms from YBCO on Al atoms from Al_2O_3 . (b) Bragg peaks in $(1\bar{1}0)$ plane of YBCO. The peaks indicated in black correspond to the r-cut sapphire while the red peaks relate to YBCO. Because the thickness of the CeO_2 buffer layer is much thinner than YBCO, its pattern is very weak and not observable here.

Third, in polycrystalline K_3C_{60} the same critical current threshold is observed for DC and picosecond measurements (see Fig. R8). The only difference is how steep the current dependent suppression is. This is an indication that the critical current behavior is not related to vortex motion, but rather to current-induced phase slips both for DC and short pulses. In contrast, for monocrystalline YBCO thin film, the DC critical current and the picosecond critical current response are different, indicating different physics. In particular, the nonlinear transmittance of the picosecond current pulse, which is a proxy for a measurement of the superfluid density at different pulse amplitudes, is reduced already at low current densities — well below the DC critical current density. This has been already observed [D. E. Oates et al., *Phys. Rev. Lett.*, 93, 197001(2004)] and interpreted as an indication of the presence of a node in the momentum-dependent superconducting gap. In this condition, the DC resistance remain zero up to current densities at which vortices get depinned.

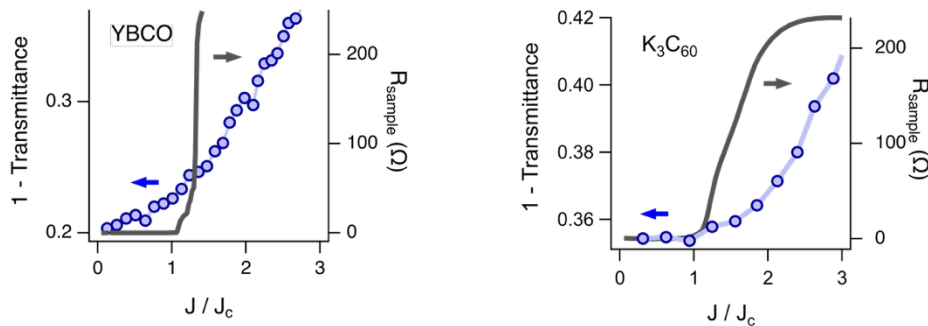


Figure R8 | Comparison of DC and picosecond responses of YBCO at $T = 50$ K and K_3C_{60} at $T = 8$ K. (a) DC and picosecond response of YBCO. Here transmittance is defined as peak electric field of transmitted pulse normalized by that of incoming pulse. $1 - \text{Transmittance}$ is plotted for clearer comparison with DC measurement. (b) DC and picosecond response of K_3C_{60} .

Specific questions and comments are provided below:

1. While I find the discussion in lines 207-217 reasonable, the attribution of the discrepancy between J_{c^*} and J_c to vortex motion or self-heating is not directly supported by the experimental data.

It is established knowledge that in type-II superconductors, the critical current density J_c is attributed to vortex motion after depinning by the Lorentz force, in both s-wave and d-wave type-II superconductors. This is why well-defined DC critical currents can be observed as shown in our experiments (Fig.2).

We have taken additional data to support this notion in the present case, which we expect the referee will be pleased with.

We fabricated a NbN strip with notches on one side, and measured J_c and J_c^* along opposite directions under an external magnetic field.

When the magnetic field generated by the bias current aligns with applied magnetic field, the critical current is suppressed. This is interpreted as local magnetic field enhancement which facilitates vortex penetration and motion. Opposite effect is observed when the two fields are anti-aligned (see Fig. R9b).

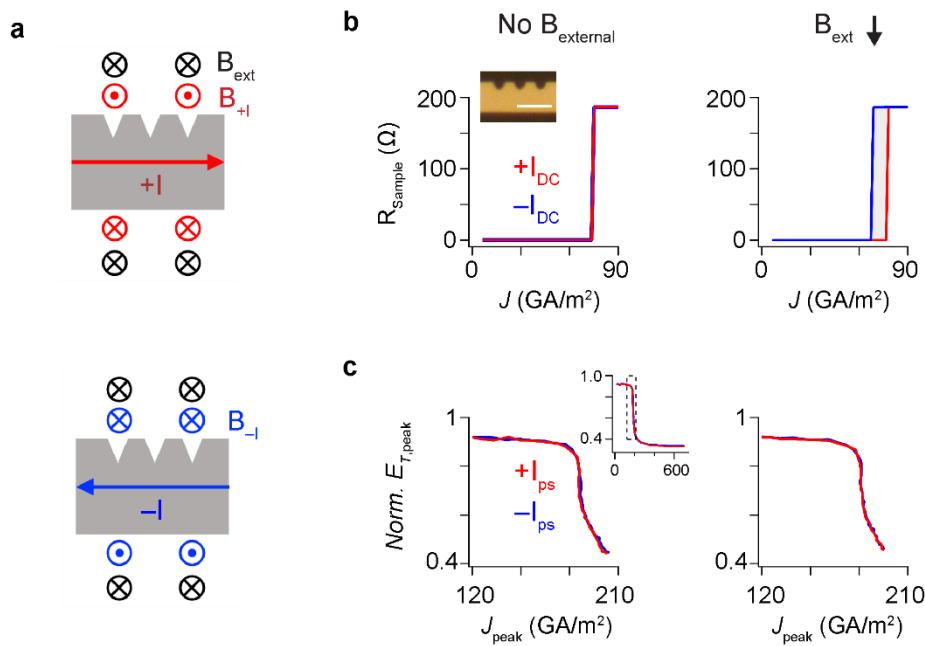


Figure R9 | Comparison of magnetic field effect on critical and depairing currents. (a) Illustration of external magnetic field (B_{ext}) and the magnetic field from current ($B_{\pm I}$). (b) Critical currents along both directions with zero external magnetic field B_{ext} and downward pointing external magnetic field B_{ext} . (c) Depairing currents along both directions with zero external magnetic field B_{ext} and downward pointing external magnetic field B_{ext} .

On the other hand, when the critical current threshold is measured with picosecond pulses, the response is independent on the relative alignment of externally applied and current generated magnetic fields (see Fig. R9c). This is readily interpreted as a confirmation of what is discussed above, that J_c^* does not result from vortex penetration but from microscopic interactions, such as depairing.

On the other hand, a recent study using ultrafast-current pump-probe spectroscopy reported a similar observation, where the large picosecond current density required to suppress superconductivity was explained by the intrinsic ultrafast dynamics of the order parameter, as described by the time-domain Ginzburg-Landau (TDGL) equation.

The reviewer is referring to *Nano Letters* 25, 13764 (2025). This seminal work clearly opened the way to studies of nonlinear superconducting electronics.

However, we believe that in that paper the authors were not in the position to see the effect discussed here. This is because the contact resistance in that study (as stated by the author themselves) is of 400 Ω, i.e., three times the sample resistance of 140 Ω. With such large contact resistance, the DC measurement of the critical current in two-point geometry is subject to

additional confounding factors. A clear problem is heating at the contacts. Furthermore, with such a large contact resistance the authors could not determine which fraction of the picosecond changes originated from the contacts and which from the sample.

In our work, significant effort was dedicated to achieving a contact resistance of $6\ \Omega$. With this, we can exclude the heating effect (see our reply to comment 14) and the uncertainty of the origin of sample's nonlinear response ($\Delta R \sim 140\ \Omega \gg 6\ \Omega$).

We also preferred not to start from J_c as input parameter for tDGL to simulate the **intrinsic** ultrafast dynamics of the order parameter. Indeed, a qualitative description of the effect could be achieved in this fashion. However, for quantitative analysis, this is not accurate enough. This is because J_c is not an intrinsic microscopic parameter of type-II superconductors, as it is determined by the depinning force, which depends on the defects in the sample. We argue that instead J_c^* should be used as the input parameter for tDGL, which gives quantitative consistency with the observed nonlinear picosecond response near T_c (see Fig. R4).

Such a scenario is distinct from the interpretation provided by the present work, where the authors assume that “the order parameter relaxes sufficiently fast that we can treat the dynamics as essentially adiabatic — that is they follow the equilibrium behavior at each point in time as a function of the current.” Is this assumption reasonable? While the GL equation is less valid at sufficiently low temperatures, is it still possible that, even within the BCS framework, the intrinsic J_c^* becomes larger on a picosecond ultrafast timescale compared to the DC regime?

The validity of adiabatic approximation is in our case justified, and in fact supported by the consistency between theoretical calculation and experimental observations. Based on the experimentally extracted microscopic parameters — coherence length and the normal-state resistivity (or magnetic penetration depth), the theory directly yields the depairing current. The calculated values of J_c^* from theory are indeed very close to the experimentally determined values. This gives us confidence that: 1) the experiment indeed can access J_c^* on picosecond timescale and 2) the adiabatic approximation in our microscopic theory describes this experimental situation properly. We have added relevant discussions in the supplementary information.

2. In this work, the temperature-dependent depairing current J_c^* is calculated within the BCS framework, which accounts for the influence of the supercurrent but assumes thermal equilibrium. Despite its relatively simple formalism, the calculated result perfectly reproduces the experimental data. My question is, how strictly is this calculation valid (in particular, compared to the theoretical works raised in the main text)? For example, the authors evaluate J_c^* by assuming that, at J_c^* , the quasiparticle energy shift equals the superconducting energy gap. On the other hand, it is known that the superconducting gap remains finite at the depairing current density [shown in Ref. 15, for example]. Is this fact consistent with the authors' formalism? In addition, the assumption of the clean limit of the superconductivity does not hold for NbN, which is in the dirty limit.

The formalism discussed in the main text is intended to give only a qualitative estimate of the depairing current, for illustrative purposes.

However, the referee is right that the depairing can set in before the quasiparticle energy shift equals the superconducting energy gap, for example, due to thermal activation. In the quantitative theoretical simulations under microscopic BCS framework, reported in our manuscript, the effect of thermal activation is taken into account (see described in the supplementary information). As shown in Fig. S19, the simulation is qualitatively consistent with what is shown in Ref. 15.

We note that the effect appears to be marginal, as the experimentally extracted J_c^* values are slightly smaller than ones calculated from the formalism (see Fig. 5a).

To simulate the depairing process in NbN within the BCS framework, indeed the dirty limit needs to be applied. We have modified the theory accordingly. Using the more accurate framework we see a great agreement between the theory and the experimental data, using the value of depairing current estimate from the coherence length and normal state resistance.

3. The authors attribute the lack of a well-defined critical current J_c^* (the gradual suppression of superconductivity as a function of the picosecond current), as shown in Fig. 4c-d, to the nodal d-wave superconducting gap ("gapless" as the authors refer to it). On the other hand, a clear threshold J_c can be identified at a considerable current density of 40-50 GA/m² in the conventional DC measurement, as shown in Fig. 2b. This seems inconsistent, and I cannot find a plausible explanation that reconciles this discrepancy. If there is no intrinsic critical current, a threshold current J_c cannot appear in the DC measurement.

Indeed, this issue, has not been explained as precisely as it should have.

The two experiments (DC resistance and picosecond current transmittance) measure two different properties of the superconducting state.

The transmittance of picosecond pulses is a measure of the finite-frequency inductive response and hence is indicative of the superfluid density. This quantity starts to drop when very low current pulses are applied. This is attributed to the depairing process at nodal point of superconducting gap as we discussed above.

On the other hand, for the DC measurements the superconductor always exhibits zero resistance with finite superfluid density regardless of the exact fraction (as long as there is a connected path between two electrodes). In DC measurements, type-II superconductors exhibit finite resistance when applied current is above the critical current threshold J_c . As we discussed above, this is well-known attributed to the depinning of vortices.

4. The YBCO sample was deposited by dc magnetron sputtering. Therefore, I guess the sample is not a single crystal but rather a polycrystalline film. If so, could this have any influence on the nonlinear transport? For example, the anisotropy originating from the d-wave symmetry might be averaged out if the grains are randomly oriented. Furthermore, is the nature of granular superconductivity, reported by the authors in their previous studies of K3C60, irrelevant to the present work?

See discussion above. The YBCO thin films are epitaxially grown on r-cut Al₂O₃ (with 10 nm CeO₂ buffer layer) substrates and are therefore not polycrystalline. Also as already remarked above, the granular superconductivity is irrelevant to the observed nonlinear behavior in picosecond measurements.

5. Throughout the manuscript, the authors only discuss the pulse peak values (such as $E_{T, \text{peak}}$, peak and $J_{T, \text{peak}}$). What about the spectral information? Any fingerprint of superconductivity, and any difference between s-wave and d-wave symmetry?

In a THz measurement, the normalized peak of transmitted pulse $E_{T, \text{peak}}$ is related to the spectrally integrated response and can track the superfluid density.

It would surely be nice to have more information and detailed spectral responses, as the referee rightly suggests. However, the full-time trace to perform Fourier Transform for each data point shown in Fig. 4a and Fig. S11, would lead to impractically long acquisition times.

We calculated the real and imaginary optical conductivities vs. frequency for NbN at 7 K and YBCO at 50 K under small ps current pulses, corresponding to the data shown in Fig. 3b and Fig. S10b. The detailed procedure is presented below.

The transmittance of pulses in the frequency domain is:

$$\hat{T}(f) = \frac{2Z_0}{2Z_0 + \hat{Z}_{sample}} \quad (8)$$

where $Z_0 = 50\Omega$ is the impedance of coplanar waveguide and $\hat{Z}_{sample}$ is the complex impedance of sample. Because here, we don't have a reference measurement w/o sample plugged in the waveguide, we use the measurement above T_c as a reference.

$$\frac{\hat{T}(f, T=7K)}{\hat{T}(f, T=20K)} = \frac{2Z_0 + \hat{Z}_{sample}(T=20K)}{2Z_0 + \hat{Z}_{sample}(T=7K)} \quad (9)$$

With $\hat{Z}_{sample}(T = 20K) = R_n - i2\pi f L_n$, where $R_n = 140\Omega$ and $L_n = L_s = 23$ pH (for NbN, $R_n = 250\Omega$ and $L_n = 32$ pH for YBCO) assuming the effective mass of the Cooper pairs is double the mass of normal carriers. The evaluation procedure of kinetic inductance of YBCO is the same as the one in the answer to question #1 of reviewer #1. This procedure has also been added to supplementary information.

From $\hat{\sigma}_{sample} = \frac{l}{\hat{Z}_{sample} \cdot s}$, where l and s is the length and cross section of sample, the real and imaginary optical conductivities for NbN at 7 K and YBCO at 50 K are extracted and shown below. Although the imaginary part σ_2 indeed shows diverging behavior, σ_1 is slightly below zero with quasiparticle contribution near zero frequency. We think the analysis in frequency domain needs to be further improved by implementing a reliable reference measurement. Yet this is out of the scope of the current work.

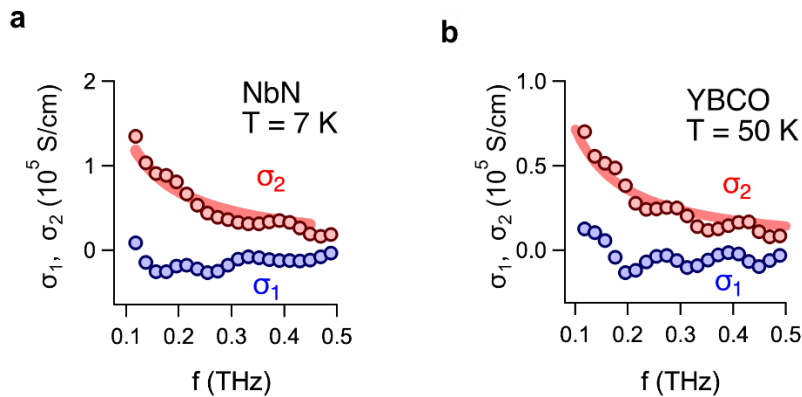


Figure R10| σ_1 and σ_2 of NbN at 7 K and YBCO at 50 K. (a) σ_1 (blue) and σ_2 (red) of NbN at 7 K. Red lines is the fitting with $1/f$ behavior. (b) σ_1 (blue) and σ_2 (red) of YBCO at 50 K. Red lines is the fitting with $1/f$ behavior.

6. The data in Fig. S5 of the Supplementary information appear to be inconsistent with those in Fig. 2a of the main text. Could the authors comment on this point?

This is an oversight on our part. The x-axis value shown in Fig. S5 is the current applied to the sample in mA and is not converted into current density by dividing by the sample's cross section ($20 \text{ nm} \times 10 \text{ um}$). We have corrected this in the modified version.

Minor comments:

7. A question regarding the data acquisition procedure: after the transimpedance amplifier, how is the signal measured? Do the authors use a lock-in amplifier or a digital oscilloscope?

Yes, the signal after the transimpedance amplifier is measured with a lock-in amplifier. We have added this information in supplementary information.

In addition, the authors mention the calibration procedures in their previous work (Ref. [2] in the Supplemental information). However, according to the equation in S4, it seems that the calibration discussed in Ref. [2] is not necessary, but the peak current amplitude can be directly obtained from the conversion equation between I_{peak} and $V_{\text{signal,peak}}$.

Yes, these two calibration methods have been confirmed to be consistent in an earlier work [J. McIver et al, *Nat. Phys.* 16, 38 (2020)]. Therefore, we directly calibrate the peak current amplitude from the ratio between I_{peak} and $V_{\text{signal,peak}}$. All time traces need to be measured in order to perform another calibration procedures as discuss in Ref.2, which makes the measurement time impractically long. As we will demonstrate below in our reply to comment 15, we indeed validated the calibration results.

8. I recommend that the authors show the superconducting transition temperature T_c of NbN and YBCO earlier in the manuscript, probably around lines 99-100 or in the caption of Fig. 2.

We followed the recommendation of the reviewer and added the description of T_c in the caption of Fig. 2 in the main text.

9. What is the reason that the measurement of the YBCO sample was limited to relatively high temperatures above 50 K?

This is to avoid burning of the sample during measurement of J_c . We have observed before, when J_c is too high, the sample got burned immediately after J is above J_c , because of the immediate onset of dissipation.

10. What is the normalization factor for the vertical axes (Norm. E_{in}) in Figs. 3b,3c?

The peak of the incoming pulse. We have clarified this in the modified manuscript.

11. In line 162, the authors state that “The slight leftward deviation observed during the sharp drop can be attributed to the reduction in the transmitted peak current amplitude...” Here, is the word “deviation” appropriate? The authors might consider using another word, such as “tilt” or something else.

We have followed the advice of the reviewer.

12. In Figs. 4a and 4c, the normalized transmitted electric peak field ($E_{\text{T, peak}}$) is plotted as a function of transmitted peak current densities ($J_{\text{T,peak}}$). I assume that these plots correspond to the electric field transmittance depending on the current density flowing in the sample. It would be helpful for readers if the authors could mention this explicitly, because the physical meaning of the quantities is not immediately clear.

We have modified the manuscript to explicitly define all abbreviations properly.

13. In line 185, the authors refer to the d-wave superconductor as “gapless.” However, I think this is not appropriate. Rather, d-wave is “nodal”.

We have changed the simple description term “gapless” to “superconducting gap evolves from zero at the nodal point to its maximal value at the anti-nodal point” to avoid confusion.

14. In S2 (and in Fig. S4) of the Supplemental information, what is the pulse width of the current? I find Fig. S4a ambiguous, as it only shows the “onset” of the applied current. In addition, I cannot catch how the authors safely excluded the heating effect based on Fig. S4b. How do the authors explain the delay-time dependence observed in Fig. S4b?

The data in Fig. S2 was measured with a current pulse with a width of 0.9 ms. In both cases, the voltage measurements started after tunable time delays and the duration of measurement was fixed to 0.4 ms. If the heating effect undermines the value of critical current in measurements, we expect the measured values of critical current decrease with longer delay time, as the sample temperature rises more. Instead, in our device, the measured critical current values stay the same with increasing the delay times. Here we observed that once the current was above the critical value, the resistance increased faster with longer delay. This is because, when the sample becomes resistive after the current is above the critical value, sample's temperature increases more with longer delay due to self-heating.

15. I cannot clearly understand what the authors mean in the last part of S4 of the Supplemental information. Does the sentence “the factor 0.85 is taking into account the average value between the pulses reaching the photoconductive switches and the pulses transmitting through the switches ($\approx 30\%$ reflection due to local impedance mismatch)” mean that the authors have compensated for the reflection loss?

The reviewer is right, the factor compensates for reflection loss.

The sentence “the correlation effect between the current pulse time profile and the switch response time profile” is also ambiguous. Does “current pulse time profile” refer to the observed data, or to the actual time profile of the real current pulse?

The “current pulse time profile” refers to the actual time profile of the real current pulse. Because the bandpass frequency (~ 0.7 THz at -3 dB, see Ref. [2]) of our coplanar waveguide is large enough, considering the most spectral weight of our current pulses (FHMW ≈ 2 ps) is limited up to 0.5 THz (at -30 dB). The damping of the current pulse when propagating along the waveguide is negligible. Therefore, it is reasonable to assume the actual current pulse profile and the switch response time profile are identical.

To avoid confusion, we have modified this paragraph. We show below how we validated the calibration method, which also has been added to the supplementary information.

Here we applied a DC bias current $I_{\text{bias}} \approx 19.25$ mA — slightly below J_c , and did the same measurement to see how much the sudden drop of transmittance is shifted when increasing the peak current of picosecond pulses. As can be seen, the shift is around 19.45 mA, close to the value of bias current. This validates our calibration of the peak current of the picosecond pulse.

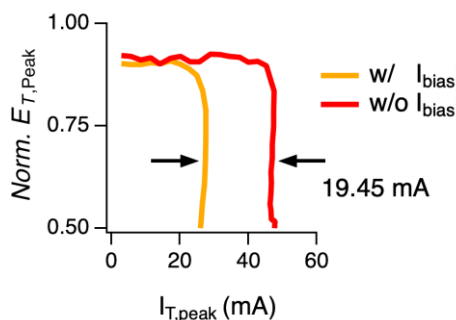


Figure R11 | Offset of the depairing process by applying the bias current I_{bias} .

16. What is Z_T in equation (9) in the Supplemental information?

It is the impedance Z_0 of the coplanar waveguide. We made this consistent in the modified manuscript.

17. In the last page in the Supplemental information, the authors say that “In the microscopic model, we have obtained...To do so, we express the current in units of depairing current...” Here, it is not clear what the authors mean by “To do so”.

We have updated the theoretical approach to include the relevant effects in dirty superconductors. The supplementary information now contains a clarified version of the text, clearly explaining the fitting procedure of theory and experiment.

In summary, substantial revisions are required before the manuscript can be further considered for publication.

Reviewer comments:

Reviewer #1 (Remarks to the Author):

I have carefully reviewed the revised manuscript, the updated supplemental information, and the authors' detailed point-by-point rebuttal. I would like to begin by commending the authors for the substantial effort invested in this revision. The inclusion of the "notch-strip" experiment is a particularly strong addition, and the direct experimental characterization of the coherence length and penetration depth provides a significantly stronger foundation for the quantitative comparison between theory and experiment.

I am aware that raising further points at this stage may appear stringent. However, to ensure the manuscript fully meets the level of physical rigor expected for Nature Physics, I believe several conceptual aspects would benefit from further clarification before final acceptance.

We thank the referee for the positive assessment of our revised manuscript and for recognizing the value of the added notch-strip experiment and direct characterization of the coherence length and penetration depth. We also appreciate the remaining comments, which we agree are helpful for further clarifying the physical basis of our interpretation. We have revised the manuscript and Supplementary Information accordingly, and address each point in detail below.

1. The interpretation of the gradual suppression of superconductivity in YBCO remains largely qualitative, especially when compared to the more rigorous treatment presented for NbN. The revision from the term "gapless" to a description of the gap evolving from nodal to antinodal regions is appropriate and appreciated. However, the underlying non-equilibrium dynamics of nodal quasiparticles under strong current injection are not yet sufficiently addressed. Unlike the s-wave case, where a sharp threshold behavior is well captured within the BCS/Usadel framework, the YBCO response is currently attributed to gap nodes in a largely descriptive manner. Providing even a simplified quantitative estimate, or a more explicit theoretical narrative describing how the nodal quasiparticle population evolves during the pulse, would significantly strengthen the connection between experiment and interpretation, and help place the two material systems on more equal footing.

We thank the referee for their broadly positive evaluation.

As correctly noted, the microscopic theory of cuprate superconductors is not yet well established even in equilibrium, and remains an active area of research. As a result, there is currently no widely accepted theoretical framework that allows a quantitative description of non-equilibrium depairing dynamics in *d*-wave systems comparable to the BCS/Usadel approach used for s-wave superconductors. This limitation makes it challenging to construct a fully microscopic model for the YBCO response under strong current injection.

Nevertheless, the gradual suppression of superconductivity observed in YBCO is qualitatively consistent with the presence of gap nodes, where quasiparticles can be excited at arbitrarily low energies. Within a phenomenological framework, this can be qualitatively captured by the GL theory — $n_s \propto 1 - \frac{J^2}{J_c^2}$. Therefore, we have employed time-dependent Ginzburg–Landau (tDGL) simulations (Section 10.2), which reproduce the gradual suppression of superconductivity under picosecond current excitation, as shown below.

We emphasize that while this approach captures the overall trend, it does not fully resolve the microscopic dynamics. A more complete theoretical treatment would require a robust microscopic description of the *d*-wave superconducting state under non-equilibrium conditions, which is currently lacking and is certainly an exciting area of future research. We have added these points in the main text and expanded the discussion in the Supplementary Information. We agree that developing such a quantitative framework would be highly valuable and could further advance the understanding of pairing mechanisms in cuprate superconductors. This remains an important direction for future work.

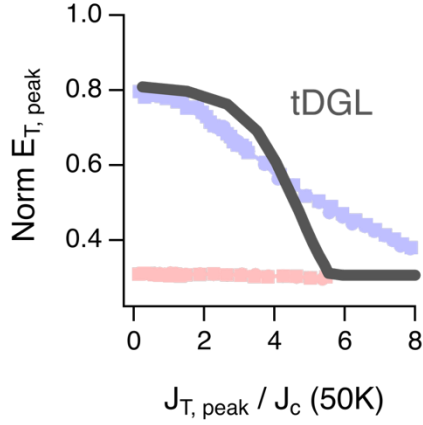


Fig. R1: Comparison of tDGL simulation results and measurements on YBCO at 50 K. The simulation results based on tDGL theory are plotted in dark grey. The measurement data is plotted in light blue (50 K) and light red (110 K).

2.The claim that the presented approach constitutes a “powerful new probe” of pairing symmetry appears somewhat overstated in its current form. While the contrasting behaviors observed in NbN and YBCO are compelling, the study effectively compares only a single representative of each symmetry class. In the absence of broader material validation or a more general theoretical framework, I recommend moderating this statement. Framing the method as a promising approach demonstrated in representative s-wave and d-wave systems would preserve the impact of the work while maintaining appropriate caution in introducing a new experimental paradigm.

Our original phrasing may have been too strong. Accordingly, we have moderated the statement in the revised manuscript and now describe the method as “offer a new probe” of pairing symmetry. We believe this revised wording more appropriately reflects the present level of validation while preserving the significance of the demonstrated approach.

3.While the success of the “adiabatic approximation” in reproducing the experimental J^* values is convincing, the manuscript would benefit from a more explicit discussion of the underlying non-equilibrium physics. Even if the order parameter can adiabatically follow the current pulse, the excitation and relaxation pathways of quasiparticles on picosecond timescales remain central to the physics of high-field transport. A concise discussion addressing relevant energy relaxation timescales and the possible role of non-thermal carrier distributions would substantially broaden the relevance of the work, particularly for the ultrafast and non-equilibrium superconductivity communities.

We thank the referee for this insightful comment. We agree the relaxation channels involved in the depairing dynamics is an important aspect.

Experimentally, we find that for stronger pulses above the depairing threshold, the transmittance gradually approaches the normal-state response. However, this slow suppression is not captured within the current adiabatic theoretical framework. We attribute this discrepancy to non-equilibrium effects and additional relaxation mechanisms beyond our adiabatic model.

Several processes can contribute on relevant timescales. First, both cold-atom experiments and BCS theory suggest that, following a quench, the superconducting gap relaxes on a timescale set by the gap magnitude (e.g., Phys. Rev. Lett. 106, 050402; arXiv:0908.3483v2; Phys. Rev. Lett. 111, 057002), which for $T_c \sim 14.5$ K corresponds to the picosecond regime. Second, phonon-mediated relaxation processes occur on comparable timescales. Third, in the dirty limit applicable to NbN, elastic scattering introduces additional fast relaxation channels.

These mechanisms can jointly contribute to the observed gradual suppression, including gap retardation effects and quasiparticle relaxation. While our experimental platform provides access to these ultrafast dynamics, a quantitative disentanglement of the individual contributions would require a more complete non-equilibrium theoretical framework, which is beyond the scope of the present work.

Importantly, the inability of the adiabatic model to reproduce the observed tail already highlights the sensitivity of our experimental approach to microscopic relaxation processes, underscoring

its potential as a probe of non-equilibrium superconducting dynamics. We have added a discussion of these points in the revised manuscript and Supplementary Information.

4. As a technical point, the theoretical curves shown in Fig. 5 (and Fig. R4) appear noticeably piecewise-linear rather than smooth. Given that the abrupt collapse of superconductivity in the s-wave NbN system is a central result, it would be helpful for the authors to clarify whether these features arise from numerical discretization or reflect an intrinsic instability captured by the Usadel/tDGL framework.

We thank the referee for this observation. The piecewise-linear features arise from numerical discretization in the simulations and do not reflect a physical effect.

To verify this, we performed simulations with finer discretization (Fig. R2). With increasing sampling density, the overall behavior remains unchanged, while the apparent piecewise structure becomes progressively weaker, though still faintly visible. These residual variations arise from numerical sensitivity near the depairing threshold.

Further increasing the numerical precision would require prohibitively large computational resources and does not lead to any qualitative change in the results.

We have accordingly replaced the data shown in Fig. 5 with higher-resolution results and clarified this point in the theory section of the Supplementary Information.

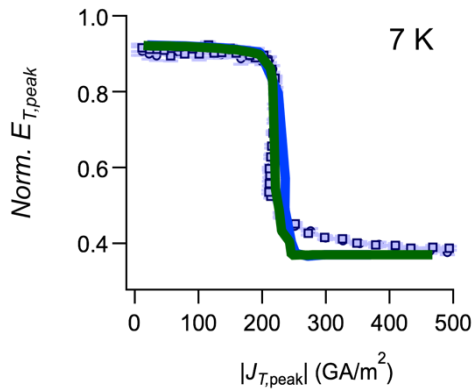


Fig. R2: Comparison of simulation results with more sampling points (green) with the simulation result shown in last-version main text (blue).

In summary, the authors have addressed nearly all of the technical concerns raised in the first round of review. With further refinement of the discussion regarding YBCO's nodal dynamics, and a moderation of the claims concerning general applicability, I believe this work will constitute a definitive and impactful contribution to the field of nonlinear superconducting transport and ultrafast superconductivity.

We thank the referee for the positive assessment and constructive feedback. We have carefully addressed the remaining points, including refining the discussion of YBCO nodal dynamics and moderating the scope of our claims. We hope that, with these revisions, the manuscript now meets the standards for publication in *Nature Physics*.

Reviewer #2 (Remarks to the Author):

The authors have properly addressed most of the questions and comments raised by the Reviewers. However, I still find several points unclear or inconsistent, which should be clarified before publication.

We thank the referee for the careful evaluation of our revised manuscript. We appreciate the remaining points raised and have clarified them below.

1. In the model of the equivalent circuit of the sample and waveguide, the authors assume $L_{n0} = L_{s0}$. This indicates that the kinetic inductances of the normal carriers and Cooper pairs are identical, which sounds implausible. Is this assumption justified?

The assumption follows from the definition of kinetic inductance which is an indication of the quasiparticle inertia. The kinetic inductance of a thin film is given by [J. Appl. Phys. 40, 2028 (1969)],

$$L_k = \frac{m^* l}{n(e^*)^2 A},$$

where m^* is the effective mass, l the length of thin film, n the carrier density, e^* the carrier charge and A the cross-section area.

In the normal state, all carriers are electrons, giving

$$m^* = m_e; n = n_0; e^* = e, \text{ which defines } L_{n0}.$$

When all carriers condense into Cooper pairs, the parameter become

$$m^* = 2m_e, n = n_0/2 \text{ and } e^* = 2e. \text{ Substituting these values into the expression above yields } L_{s0} = L_{n0}.$$

This result relies on the assumption that the effective mass of a Cooper pair is twice that of a single electron. To avoid ambiguity, we have clarified this assumption in the revised supplementary information.

We also note that even if this assumption is relaxed, variations in L_{n0} have a negligible effect on the extracted kinetic inductance of the superconducting state from which the magnetic penetration depth λ is calculated. At 7 K, the superfluid fraction is approximately 95%, and the impedance of the normal channel ($R_n + i\omega L_n$) is two orders of magnitude larger than that of the superconducting channel. Specifically, $R_n = R_{n0}/(1 - n_s) \approx 2800 \Omega$, while $i\omega L_s \approx 24 \Omega$ at 1 THz, even without considering the contribution from L_n . Therefore, the sample response is dominated by the superconducting channel, making the precise value of L_{n0} uncritical. To illustrate this, we provide simulations in Fig. R3 below showing the effect of varying L_{n0} .

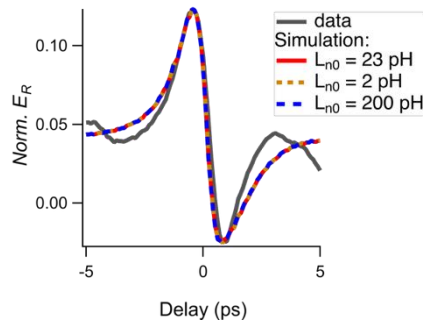


Fig. R3: Response simulation of NbN at 7K, with $L_{s0} = 23 \text{ pH}$ and $L_{n0} = 2, 23 \text{ and } 200 \text{ pH}$.

2. The authors claim that the current distribution across the samples is uniform because the sample widths are smaller than the Pearl length. However, in this case, a vortex cannot be pinned in the sample, since it induces a non-uniform screening current. Therefore, attributing the smaller critical current in the DC regime to the depinning process does not seem to be applicable. Could the authors comment on this point?

While a vortex indeed induces a local screening current, this perturbation remains small in our case and does not invalidate the assumption of an approximately uniform transport current across the sample.

The screening current density around a vortex decays with distance r from the vortex core as

$$J(r) = \frac{\Phi_0}{4\pi\mu_0\lambda^2 r}$$

(J. Pearl, *Appl. Phys. Lett.* 5, 65–66 (1964)). Using representative parameters, the screening current density at $r = 1\mu\text{m}$ is on the order of $\sim 1\text{ GA/m}^2$, which is two orders of magnitude smaller than the critical current density of our NbN films $\sim 100\text{ GA/m}^2$. Therefore, the vortex-induced screening current constitutes only a minor local perturbation and does not significantly modify the overall uniform current distribution.

Regarding the referee's concern that vortices cannot be pinned due to the non-uniform screening current, we note that pinning does not require a perfectly uniform background current. Rather, what matters is the relative magnitude of the Lorentz force the vortex experiences compared to the pinning potential.

A different regime may arise when the sample width becomes much smaller than the Pearl length, such that the vortex screening currents extend across the entire width and become comparable with the applied current. This can lead to extra strong attractive interactions between vortices entering from opposite edges. This will suppress the vortex pinning and might lead to other phenomena, for example, phase slip center. As discussed above, our device (width $\sim 10\mu\text{m}$) is not in this region.

We also want to note that regardless of the exact mechanism responsible for the DC resistance, the dynamics are irrelevant for the picosecond dynamics observed in our experiments. This is evidenced by the DC and picosecond transport measurements on notched NbN strips included in the last response.

3. In their reply, the authors state: "The NbN film is indeed in the dirty limit. In the revised manuscript we modified the temperature dependence of $\xi(T)$ to the dirty limit. As can be seen the calculated temperature dependence of J_c^* is still consistent with the data. This is because the drop of superfluid density at higher temperatures is dominating the decrease of the depairing current." I find this explanation misleading. As far as I understand, the authors calculate $J_c^*(T)$ in Fig. 5a in the same way as in the previous version. The change in the revised manuscript appears to arise only from a slight modification of the values of the penetration depth and coherence length.

First, as noted by the referee, we updated the values of the penetration depth and coherence length using parameters extracted from our measurements. These values differ slightly from those previously taken from the literature and lead to small quantitative adjustments.

Second, we explicitly adopt the dirty-limit description appropriate for NbN. In this regime, the effective coherence length is given by

$$\xi = \sqrt{\xi_0 l},$$

where ξ_0 is the intrinsic coherence length and l is the mean free path. Importantly, the coherence length values used in the original manuscript already correspond to this effective (dirty-limit) coherence length (the value is taken from literature). As a result, the main revision concerns the temperature dependence of $\xi(T)$, rather than its absolute value.

As shown in Fig. 5a of the revised manuscript, incorporating the dirty-limit temperature dependence results in only minor changes to the calculated $J_c^*(T)$. This weak sensitivity arises because the temperature dependence of the depairing current is dominated by the reduction of the superfluid density at elevated temperatures, while the contribution from $\xi(T)$ plays a comparatively smaller role, see Fig. R4.

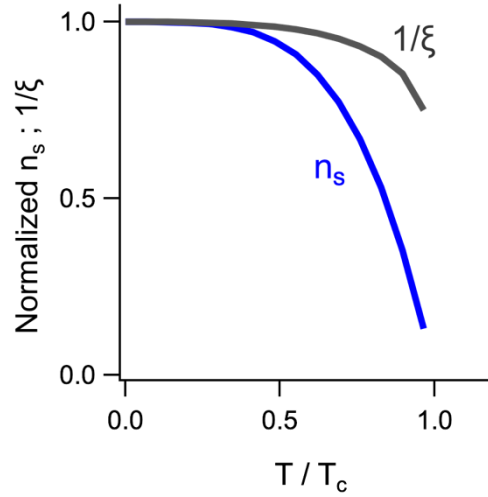


Fig. R4: Comparison of the temperature dependence of the superfluid density n_s and the inverse of the coherence length $1/\xi$.

The theoretical improvement resulting from assuming the dirty limit instead of the clean limit should appear in Fig. 5b. However, in Fig. 5b, the theory and experiment show perfect agreement, as was the case in the previous version. This seems to indicate that whether the sample belongs to the dirty or clean limit hardly affects the depairing current. Is this result reasonable?

As discussed above, adopting the dirty-limit description leads only to a small change in the depairing current value, because the coherence length used in the previous version already corresponded to the effective dirty-limit coherence length $\xi(T)$. Therefore, the overall agreement between theory and experiment in Fig. 5b remains largely unchanged.

The main difference between the clean- and dirty-limit descriptions appears not in the absolute value of the depairing current, but in the detailed dynamics of the suppression of superconductivity once the current approaches the depairing threshold. In particular, the dirty-limit model yields a sharper transition, which captures the experimental behavior better.

To illustrate this point, we compare simulations performed using the clean-limit assumption (previous version) with those using the dirty-limit description (revised manuscript) in Fig. R5. The dirty-limit simulation provides improved agreement with the experimental data, particularly in the shape of the transition near the depairing threshold.

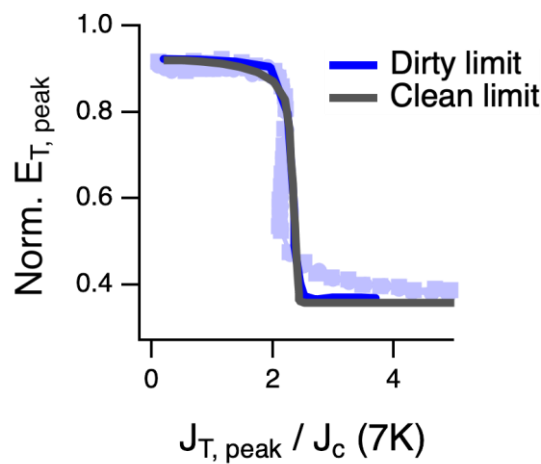


Fig. R5: Comparison between simulation results at 7 K using BCS theory in dirty and clean limit.

4. Related to question 3, I am also confused about the consistency between sections S9 and S10.1 in the SI. In S9, the authors calculate the depairing current based on $J_c^* = n_s v_s (2e)$, which appears to be an order-of-magnitude estimate, and the calculated results are plotted in Fig. 5a. On the other hand, in S10.1 the authors present a dynamic model within BCS framework. How were the results derived in S10.1 used in the main text?

The two sections address complementary aspects of the depairing process.

As noted by the referee, Fig. 5a (based on Section S9) provides an order-of-magnitude estimate of the depairing current using $J_c^* = n_s v_s (2e)$. This calculation is intended to capture the overall temperature dependence of J_c^* , rather than the detailed dynamics of the depairing process.

In contrast, Section S10.1 introduces a dynamic model within the BCS framework to describe how superconductivity is suppressed once the current approaches the depairing limit. This model is used to simulate the evolution of the transmitted signal and is directly compared with the experimental data. The agreement between the measured transmittance versus peak-current dependence and the simulations shown in Fig. 5b demonstrates that the abrupt suppression observed experimentally is consistent with a depairing-driven process.

Minor questions and comments,

5. The authors state that “the order parameter relaxes sufficiently fast that we can treat the dynamics as essentially adiabatic.” Do the authors assume that quasi-thermal equilibrium is reached in the total system, i.e., not only within the electron subsystem but also including the lattice system?

In the present theoretical framework, we ignore the interaction between electron and lattice system, and only assume quasi-thermal equilibrium of the electron subsystem.

6. I cannot clearly understand the author’s reply to question 7 by Reviewer 2. In the SI, the authors state: “The calibration procedures are discussed in detail in Ref. [2, 3].” In Ref [2], information such as the pulse duration and repetition rate was necessary for the calibration. However, in the present work these parameters do not appear in Eq. (1) in the SI. Could the authors explain the calibration procedure more clearly?

Two calibration methods can be used to determine the peak current of the picosecond pulses, as described below.

(a) DC-bias cross-correlation calibration

The first method uses the signal from the photoconductive switch under a DC bias. In our experiments, the transimpedance amplifier measures the cross-correlation between the propagating voltage pulse and the switch response function (namely, the time profile of the conductance of the switch). During calibration, the signal line is biased with a DC voltage source (Fig. R6a), and the measured time profile corresponds to the cross-correlation of the DC voltage with the switch response. As discussed in Section S5, the voltage pulse and switch response share similar temporal profiles, leading to a peak value reduced by a factor of $\frac{1}{\sqrt{2}} \sim \frac{1}{1.414}$ compared to the DC case. This yields the prefactor used in the calibration. The method requires only that the laser repetition rate remains unchanged between calibration and measurement.

(b) Charge-integration calibration

The second method determines the injected charge from the photoconductive switch, yielding

$$I_{\text{peak}} = \frac{1}{2} \cdot \frac{2\sqrt{2}}{1.273} \cdot \frac{V_{\text{Lock-in}}}{A_{\text{TIA}}} \cdot \frac{1}{f_{\text{rep}}} \cdot \frac{1}{\int \text{pulse}_{\text{norm}}(t) dt}$$

Here, I_{peak} is the peak current, $A_{\text{TIA}} (= 2 \cdot 10^9 \frac{\text{V}}{\text{A}})$ is the gain of the transimpedance amplifier, $f_{\text{rep}} (= 50 \text{ kHz})$ is the laser repetition rate, and $\int \text{pulse}_{\text{norm}}(t) dt$ is the temporal integral of the normalized electrical pulse. The factor $1/2$ accounts for equal pulses propagating in opposite

directions. The factor $2\sqrt{2}/1.273$ converts between the net charge amplitude and the root mean square (RMS) value measured by the lock-in amplifier at the chopping frequency.

- (c) Both calibration approaches are discussed in Ref. [3] and have been shown to be consistent. In our work, we adopt the DC-bias cross-correlation calibration because acquiring full temporal profiles for all current amplitudes using the second method would be impractically time-consuming. We further verified in Section S5 that this calibration procedure is valid for our measurements.

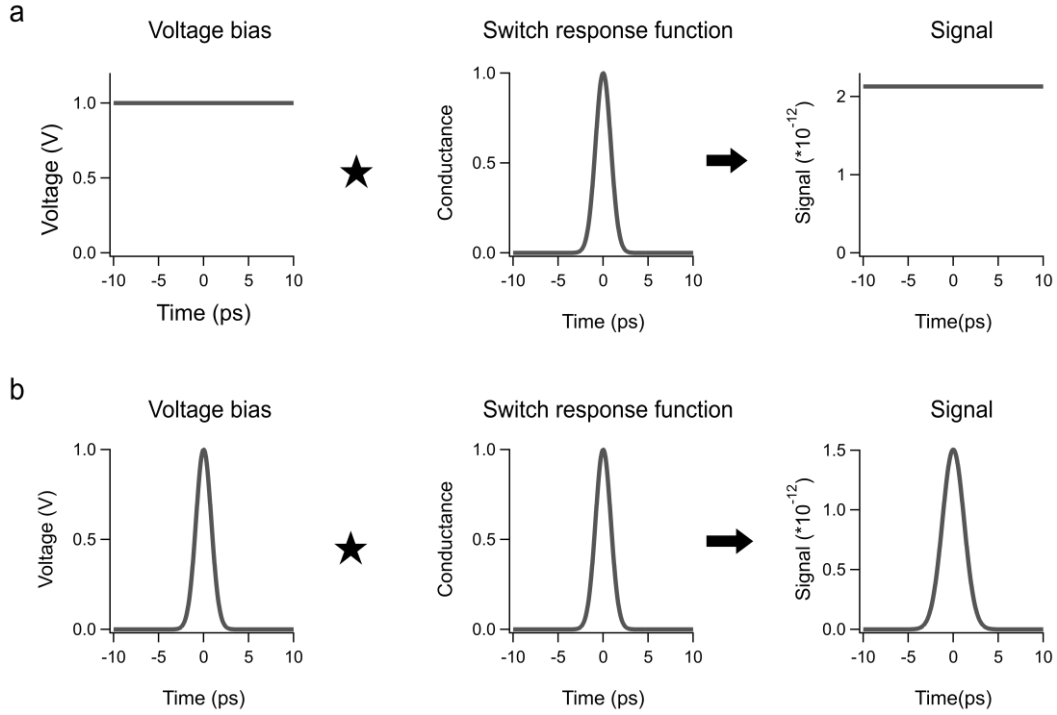


Fig.R6: Illustration of measured signal from photoconductive switch under (a) DC and (b) picosecond voltage bias. “★” denotes the cross-correlation operation.

From a more fundamental viewpoint, I find that attributing the different behavior between NbN and YBCO to the difference in gap symmetry remains speculative. The detailed analysis and theoretical calculations are limited to NbN. Simulations of the depairing current in a d-wave superconductor would be highly desirable to confirm this interpretation.

Our interpretation is primarily grounded in the experimental observations and their internal consistency. The qualitatively different responses observed in NbN and YBCO—namely, an abrupt depairing transition in NbN and a gradual suppression in YBCO—are robust features of the data. From an experimental perspective, we have carefully examined alternative explanations and performed consistency checks to support this interpretation.

As also noted by Referee #1, further verification across a broader range of superconducting systems would be valuable. However, the technique is new, the experiments are demanding and hence a systematic study is definitely beyond the scope of the present work. We do consider this investigation as an important direction for future studies, while the current results already establish the proposed mechanism and its experimental signatures.

Following the suggestion of Referee #1, we have moderated our claim and remained cautious. The exact details of non-equilibrium depairing in cuprates clearly need further work.

More specifically, the microscopic theory of cuprate superconductors is not yet well established even in equilibrium, and remains an active area of research. As a result, there is currently no widely accepted theoretical framework that allows a quantitative description of non-equilibrium

depairing dynamics in *d*-wave systems comparable to the BCS/Usadel approach used for *s*-wave superconductors. This limitation makes it challenging to construct a fully microscopic model for the YBCO response under strong current injection.

Nevertheless, the gradual suppression observed in YBCO is qualitatively consistent with the presence of nodes in the *d*-wave gap, where quasiparticles can be excited at arbitrarily low energies. Regarding this aspect, tDGL theory should be able to capture partially the features, in which the superconducting order parameter decreases following $1 - (\frac{J}{J_c})^2$. We demonstrate the simulation results in Fig. R1 in our response to Referee #1.

We emphasize that while the simulations capture the overall trend, it does not fully resolve the microscopic dynamics. A more complete theoretical treatment would require a robust microscopic description of the *d*-wave superconducting state under non-equilibrium conditions, which is currently lacking. We have clarified these points in the main text and expanded the discussion in the Supplementary Information.

We agree that developing such a quantitative framework would be highly valuable and could further advance the understanding of pairing mechanisms in cuprate superconductors. This remains an important direction for future work.

On the other hand, I find the experimental method presented by this work, evaluating the depairing current by using ultrafast current pulses, to be novel and interesting. Therefore, after the authors have addressed the issues raised above, I think the paper would be suitable for publication in Nature Communications.

We thank the referee for the broadly positive assessment of our work and believe that the choice of publication venue rests with the Nature Physics editor.

Reviewer comments:

Reviewer #1 (Remarks to the Author):

I have read the revised manuscript and the authors' response to my previous comments. I am satisfied that the main points I raised have been addressed.

The authors have toned down the broader claims, added a more careful discussion of the YBCO results, and clarified the numerical origin of the piecewise features in the theoretical curves. The remaining limitations, particularly the lack of a full microscopic treatment of the d-wave case, are now stated appropriately and do not, in my view, prevent publication.

I therefore think the manuscript is now suitable for publication in Nature Physics.

We thank the Reviewer for their thoughtful remarks throughout the entire review process and for this recommendation.

Reviewer #2 (Remarks to the Author):

While the authors have properly addressed some Reviewers' comments, I still have some questions that should be clarified before publication:

1. In their response to Comment 2 by Reviewer 2, the authors might have misunderstood the main issue. In samples with a width smaller than the Pearl length, where the current distribution is nearly uniform, the circulating current around a vortex spreads across the entire sample. Under this condition, a trap potential arising from impurities cannot effectively pin the vortex. Rather, other phenomena, such as vortex entry from the sample edges and the emergence of phase slip centers, will become relevant. This point should be discussed not only in the reply but in the main text or Supplementary information. In addition, I would recommend that the authors include their nice results for the notched NbN strip sample in Supplementary information.

The screening current density around a vortex at distance r from the vortex core is given by

$$J(r) = \frac{\Phi_0}{4\pi\mu_0\lambda^2 r}$$

(Pearl, *Appl. Phys. Lett.* **5**, 65 (1964)), and thus decays rapidly away from the vortex core — ~ 10 GA/m² at 100 nm from vortex core and ~ 1 GA/m² at 1 μ m. Therefore, it is not totally justified to state that the circulating current around a vortex spreads across the entire 10- μ m wide sample. However, with our sample width of ~ 10 μ m, which is smaller than the Pearl length, indeed we cannot totally exclude the possibility of other effects — phase slip centers (lines) or a vortex-antivortex pair entering from opposite edges — in DC critical current experiments. We have added the relevant discussions in the Supplementary information.

We note, however, that the precise mechanism responsible for the DC critical current lies beyond the scope of the present work. As demonstrated by the DC and picosecond transport measurements on the notched NbN strip, the picosecond dynamics reported here remain robust against the exact mechanism of the DC critical currents.

As suggested by the reviewer, we have also included the results on the notched NbN strip in the Supplementary information.

2. I am confused by the authors' reply to Comment 3 by Reviewer 2, "As a result, the main revision concerns the temperature dependence of $\xi(T)$, rather than its absolute value." The calculation in S9. "Calculation of J_c^* ", which was used to obtain the results in Fig. 5, remains

unchanged from the originally submitted version. Therefore, the claimed revision by considering the dirty-limit coherence length is not clear. Could the authors clarify this point?

Below we clarify the treatment of the coherence length in the original and revised versions of the manuscript.

In the original version, we took the value of $\xi(0) = 5.3$ nm from the literature (Zhang, Q., et al., Superconductivity Dependence on Epitaxial NbN Film Thickness. *IEEE Trans. Appl. Supercond.*, **29**, 1 (2019)). We used the formula below for calculating ξ at different temperatures, which actually only applies to BCS theory in the clean limit.

$$\xi(T) = \frac{\xi(0)}{\sqrt{\tanh(1.74\sqrt{\frac{T_c}{T}} - 1)}}$$

For BCS theory in the dirty limit, the effective coherence length $\xi_{eff}(0) = \sqrt{\xi(0)l}$ should be used, where ξ is the intrinsic coherence length and l is the mean free path. Therefore, below we adopt the notion ξ_{eff} instead of ξ . In the revised version, as suggested by Reviewer 1, we measured the temperature dependence of B_{c2} and extracted the value of $\xi_{eff}(0) \sim 5.23$ nm, which is very close to 5.3 nm which we took from the reference in the original version. For BCS theory in the dirty limit, ξ_{eff} at different temperatures is given by

$$\xi_{eff}(T) = \frac{\xi_{eff}(0)}{\sqrt{\tanh(1.74\sqrt{\frac{T_c}{T}} - 1)}}$$

Between the original and the revised version, the main change is the temperature dependence as can be seen by comparison of the denominators.

Below we show the comparison between the temperature dependence of J_c^* in the original and revised versions of the manuscript. The results indeed demonstrate small changes.

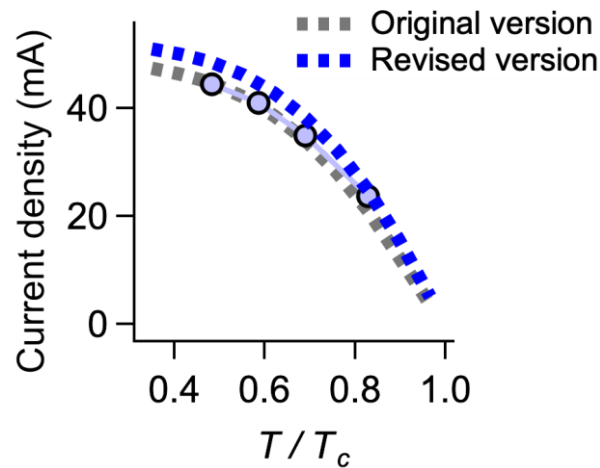


Fig. R1| Comparison between the calculated J_c^* at different temperatures in the original and revised versions of manuscript.

3. I find the authors' reply to Comment 6 by Reviewer 2 helpful, so I recommend that the authors include it in the Supplementary information.

We have added this part into the Supplementary information.

4. Concerning the authors' reply to Comment 1 by Reviewer 1 and the last comment by Reviewer

2: While I understand that a quantitative calculation of the nonequilibrium depairing dynamics in a d-wave superconductor is highly challenging, it is not clear why the tDGL framework is considered to be suitable, at least qualitatively, for describing the dynamics of a d-wave gap system. Do the authors assume that a nodal gap system can be treated as an effectively high-temperature system?

In a d-wave superconductor, even with a small bias current, the induced Doppler energy shift excites quasiparticles at the nodal point in momentum space. This means the superfluid density starts to drop under a small picosecond current drive, which is exactly what we observed in our measurements on YBCO.

In Ginzburg-Landau theory, the superconducting order parameter under current drive is given by

$n_s = 1 - \left(\frac{J}{J_c}\right)^2$, which indicates that with a small bias current, the superfluid density drops.

From this specific comparison, we think the tDGL theory could partially capture the dynamics of a d-wave superconductor. To fully capture the dynamics of YBCO, a more sophisticated microscopic theory needs to be developed.

We do not fully understand the last question “Do the authors assume that a nodal gap system can be treated as an effectively high-temperature system?”. We want to clarify that our analysis or speculations are based on the gap symmetries, not on low or high superconducting transition temperatures.

After addressing the issues raised above, the manuscript will be ready for publication.

Reviewer comments:

Reviewer #2:

Remarks to the Author:

The authors have addressed the reviewers' comments raised in the previous round. However, I still find an inconsistency in the authors' claim concerning the uniformity of the supercurrent. The authors state that the screening current density around a vortex varies rapidly over a length scale of 100 nm-1 μ m. On the other hand, they also argue that the current density in their sample is nearly uniform because the Pearl length exceeds the sample's width. How can these two statements be reconciled?

The two statements are not contradictory, because they refer to different contributions to the current distribution. The condition that the Pearl length exceeds the sample width implies that the externally driven transport supercurrent is approximately uniform across the strip. A vortex, if present, introduces an additional local circulating screening current around its core, which produces only a spatially localized perturbation to this otherwise nearly uniform background current.

Quantitatively, the vortex-induced screening current decreases rapidly with distance from the vortex core. For the parameters of our NbN films, its magnitude is approximately 10 GA/m² at 100 nm from vortex core and approximately 1 GA/m² at 1 μ m. These values are substantially smaller than the depairing current density of order 100 GA/m². Although the screening current can therefore produce appreciable local nonuniformity very close to a vortex core, this perturbation is confined to a small region and does not substantially modify the current distribution over the majority of the sample.

In addition, our picosecond measurements show the disappearance of the superconducting vortex-diode response (see Sec. 10 of the Supplementary Information), consistent with vortex motion being strongly suppressed on the picosecond timescale. The vortices therefore remain effectively confined near their nucleation sites at the sample edges during the current pulse. Consequently, when the bulk of the NbN strip approaches the depairing threshold, its response is governed predominantly by the approximately uniform transport supercurrent rather than by vortex-induced current redistribution.

We have added this clarification to the Supplementary Information.