Response to Reviewer #2

> The authors, after a short introduction devoted to the discussion of the phenomenon of superconductivity in heterogeneous mesoscopic systems, focus on an attempt to build a model describing quasi-two- dimensional heterostructures in which the phenomenon of superconductivity in the interface occurs. In particular, the subject of the work is an attempt to describe the phenomenon of superconductivity in disordered quasi-one-dimensional filaments (lattice) near the percolation boundary studying that phenomenon with a kind of a two-fluid model.
> In the further part of the paper, they compare the results of the calculations and simulations with the results obtained, as it results from the text earlier (see below) in other publications, of measurements of the impedance of LAO/STO heterostructures. In conclusion, the authors write that (page 12, third paragraph): ”were able to identify the specific physical effects of [8230;] on the model on macroscopic transport: the resistivity and inductance of the various regions, how rapidly the normal metal becomes superconducting by decreasing T, due to the width of the distribution of temperatures associated with the microscopic disorder, and so on.”.

We thank the reviewer for their careful reading of the manuscript. Below, we provide a detailed answer to all the issues raised.

> In the respectful opinion of this reviewer, this conclusion is not justified. Starting from the logic, from the compliance of the model results for a certain set of parameters with the measurement results, it does not yet follow that the model correctly describes the reality.

We thank the referee for their comment. However, we believe we have provided enough evidence that our model can reproduce the main features experimentally observed in LAO/STO heterostructures. If the referee is rather asking for proof that our model describes the *reality*, in all its aspects, we believe that this issue can be generically raised for any theoretical attempt to understand nature. Indeed, according to the most widely accepted epistemological framework (cf K. Popper, The logic of Scientific Discovery), no theory nor model can aim to correctly describe reality. What we can say is that our model, for a broad range of parameters (each one corresponding to specific features of the data, without fictitious redundancy) is corroborated by the fact the model calculation quantitatively agrees with the data.

> The second problem concerns measurements. The geometry of the measuring system, not discussed at all, but probably identical to the one described in [46], in the language of circuit theory allows only the measurement of the amplitude and phase of the wave reflected from the DUT.

We thank the referee for their comment. The geometry of the measuring system is, indeed, the same as used in G. Singh et al, Nat. Comm. 9(1), 1 (2018) (ref. [46], now ref. [58]). This device (see Fig.R1 attached and Fig. 1 in G. Singh et al, Nat. Comm. 9(1), 1 (2018)) was developed in the experimental group led by N. Bergeal and was inspired by the developments in the field of quantum devices [A. Wallraff et al, Nature volume 431, pages162–167 (2004), Bergeal, N. et al. Nature 465, 64–68 (2010)]. As such, it was already used and described also in Nature Mat. 18, 948 (2019) and Nature Commun.

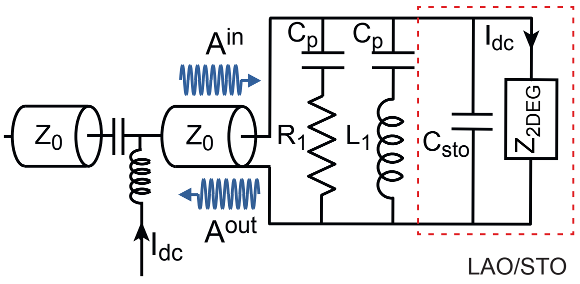

FIG. R1: Microwave set-up.

13, 4625 (2022). Nonetheless, we agree with the referee that we should have stressed this better in our manuscript. In the revised version of the manuscript, we added more details on the experimental set-up, as an Appendix to the main text.

> The authors write that they are limited to a very narrow frequency region near 360 MHz, which in free space corresponds to a wavelength of about 14". This is much more than the 0.1" size of their sample. In practice, it is a resonant RLC circuit with a piece of quasi-2d material as partially L, R, and C, frequency and temperature dependent element, and of course with many other elements which sizes, properties, geometry and linearity contribute to the quality factor and the resonance frequency of the circuit. The VNA (measuring instrument) tests the entire measurement chain, cables, sockets, strip lines, etc. ending with the sample and matching electronics, and the complex impedance, real and imaginary conductivity numbers, shown in pictures are based on a behavioral model of "Device Under Test", inhomogeneous sample in a resonant microwave system, with many distributed and discrete elements, which the authors do not write about. How large are the estimated errors in determining the values $\sigma_1(T,\omega)$ and σ_2?

As pointed out by the referee, the wavelength at the probing frequency is much larger than the size of the resonant circuit that can therefore be treated as a lumped elements circuit.

A calibration of the microwave set-up is done in-situ, by using the gate-controllable impedance of the sample as calibrated impedances. Above Tc, the circuit has a resonance frequency $\omega_0 = \sqrt{\frac{1}{L_1 C_{\mathrm{STO}}(V_{\mathrm{G}})}}$ where L_1 is the inductance of a SMD inductor (temperature independent). C_{STO} is the parallel capacitance due to the STO substrate that depends on the gate voltage V_{G} (since STO dielectric constant is electric-field dependent) but is temperature independent in the range of interest. The measurement of ω_0 above T_c for different values of V_{G} gives therefore a direct access to $C_{\mathrm{STO}}(V_{\mathrm{G}})$. Likewise, the width of the resonance is determined by the resistive part of the resonant circuit comprising a SMD resistor R_1 (temperature independent) in parallel with the resistance of the sample. The resistance of the sample at microwave frequency is therefore extracted from the resonance width for different gate voltage values and compared with the DC values which are measured through a bias Tee. The two measurements agree well (within 10%) as expected for the rather low frequencies used in this work. The small difference can be explained by the fact that the current paths at the injection of the current into the sample are slightly different for DC and AC currents. Using three different values of gate voltage, we can then realize three different complex impedances of the resonant circuit whose elements (R, L, C) are all known. They are used as standards to implement a calibration procedure that allows to move the calibration plane from the VNA to the input of the resonant circuit. Note that all the information on σ_1 and σ_2 is in fact already directly available from the resonance (position and width) and that the calibration procedure is only needed to remove some parasitic signals mainly due to standing waves. The procedure was successfully implemented in the references mentioned above. Uncertainty in the determination of the absolute value of is estimated to be lower than 15% (mainly because of the inductance of PCB lines) and the uncertainty on the absolute value of is estimated to be lower than 10% (mainly because of some losses in the STO substrate). In Nature Commun. 9, 1 (2018) we found that the absolute value of the stiffness extracted at $T = 0$ agrees well with BCS/Mattis Bardeen's prediction for a dirty superconductor. In both cases (σ_1 and σ_2), the relative uncertainty in the temperature dependence is marginal.

> How repeatable are the measurements for different samples? Do the conclusions are supposed to be about a phenomenon? In fact. on page 7 of the manuscript authors write "It is worth noting that the tree peculiar features summarized above are indeed characteristic of the rather disordered sample, whereas in more homogeneous samples some of those peculiarities can be less pronounced or even absent." What is the measure of the "sample homogeneity" and how was it determined? Lastly, Does the model predict an unobvious phenomenon that awaits experimental confirmation?

We thank the referee for letting us clarify this point. The experiment is fully repeatable on the same sample. Measurements on different samples could instead lead to different resistive and superfluid responses since they will have, in general, a different amount of disorder. Let us also stress that the disorder we are referring to is characterized by strong correlations in space since, as we discussed in our work, the geometry of the superconducting forming path plays a major role in the system's response. A measure of this inhomogeneity can be experimentally obtained from the broadening of the resistive transition, i.e. $\Delta T/T_c$. Indeed, for $\Delta T/T_c \gtrsim 1$ it is not possible to account for the broadening only relying on paraconductivity effects with sensible parameters. The emergence of this strong inhomogeneity as well as signatures of filamentary superconductivity has been observed in these LAO/STO heterostructures since the very first publication [N. Reyren et al, Science 2007 doi:10.1126/science.1146006]. Afterwards, similar puzzling experimental features have been observed in other two-dimensional systems such as transition metal dichalcogenides, pointing towards a more generic phenomenology emerging in strongly inhomogeneous superconductors. Our theoretical study sheds light on these "unobvious" experimental observations in terms of SC filamentary structures. Let us highlight again that the beauty of our theoretical approach is that it allows us to discriminate between the role played by the degree of disorder, encoded in the width of the Gaussian distribution $P(T_{c,i})$, and the spatial inhomogeneity, i.e. the filamentary structure of the cluster, which is ultimately what distinguishes the two over- and under-doped regimes.

Finally, let us stress that those effects due to filamentarity are found even in the cleanest samples, whereas in a very inhomogeneous sample, as the one we considered, they are enhanced and this allows for a theoretical understanding of the phenomenon.

Turning to the model, it is worth noting two elements that were almost completely omitted in the discussion by the authors: the RIN model described on page 5 is based on the linear response of the system (in the authors' words, Ohm's law and Kirchhoff's law). The system of even weakly coupled superconductors is a non-linear system, and the phenomenon of proximity may complicate this description even more. Discussion of these elements of the physical system is definitely missing in the work.

It is true that we give a linear description of the system, which is valid in stationary conditions and assuming infinite critical currents between the various superconducting bonds forming the filaments. It is worth noting that our superconducting bonds are not weakly-coupled superconductors à la Josephson but instead, those are coarse-grained mesoscopic regions of superconductors. They are strongly connected and all phase fluctuations are absent.

In experimental terms, typical currents used to probe the linear response in LAO/STO samples are of the order of the nA, whereas non-linearities in the IV characteristics are typically in the μA range.

In the present work, however, we explore only the finite-frequency response of the system in linear response and stationary conditions. In the revised manuscript, this indirect assumption is now more clearly explained and we thank the reviewer for pointing it out.

The last remark concerns percolation phenomena. Author's narration liberally mixes up microscopic and electrodynamic description, using term "optical" for microwave, and in fact not even microwave but radio spectroscopy.

The referee pointed out what we believe is actually the strength of our theoretical investigation, i.e. we are here discussing how from macroscopic measures one can infer the microscopic structure of the superconducting cluster even revealing signatures of filamentary superconductivity.

Besides, since σ_1 accounts for the transport of unpaired electrons, we use the term "optical" in analogy with optical conductivity. That is a quite general terminology used to refer to spectroscopic measurements. In the revised manuscript, for the sake of clarity, we have further clarified it.

The frequency of their probe as it seems, except for the lowest temperatures, is basically indistinguishable from DC probe. The probe energy of 1.5 μeV is much lower than the gap of their superconductor condensate, based on at approx. 0.1K and much lower than kT of their experimental conditions. Does the real part of the conductivity, determined with the Drude model or alike, above T_c matches the DC conductivity value? If not, as it may be the case in disordered systems, how important is AC hopping conductivity in that system?

We thank the referee for giving us the possibility to clarify this point. Experimentally, for $T > T_c$, the resistance of the sample deduced from the microwave measurements matches the DC resistance (Drude resistance) that we measure at the same time through a bias Tee.

On the other hand, the "disorder" discussed in our manuscript does not involve any kind of strong nor even weak localization effects. The system here is metallic, far from the superconductor-insulator transition, and behaves as a very good metal with rather high mobility (100-100 $\mathrm{cm}^2/(\mathrm{V\ s})$). Going towards the metal-to-superconductor transition, a phase separation mechanism likely intervenes in the electronic condensate, segregating the system into puddles of the order of hundreds of nm [see J. Biscaras et al, Nature Materials volume 12, pages542–548 (2013)]. For the reasons above, we always refer to "inhomogeneities" rather than disorder.

> Studies of disordered systems have a long history and by the early 1970s the work of Boris I. Shklovskii and Alex L. Efros was already a classic. During the period of intensive study of high-temperature superconductors, numerous groups dealt with these problems, in Zurich, in Cambridge, in the former USSR, many other places, and a lot is known already about the reasons of T_c broadening in inhomogeneous systems, where not only carrier density but also scattering phenomena may influence (local) T_c.

Indeed, the problem of "T_c broadening" is not new in the context of superconductors. We might emphasize, though, that scattering phenomena caused by impurities act on a microscopic level (the so-called *dirty* limit), whereas here we investigate the presence of disorder on a *mesoscopic* lengthscale (a network of superconductors embedded in a metallic background). Moreover, as already mentioned in the previous comment, our LAO/STO sample behaves as a good metal, while the works of Shklovskii and Efros describe conduction by jumps in poorly conducting systems. All the measurements that we present here are hence not on the verge of the superconducting-insulating transition but well inside the metallic region. The mobility is always larger than 100 $\mathrm{cm}^2/(\mathrm{V\ s})$ and can even reach 1000 at high gate voltage, making the samples much better than many conventional metals in terms of conduction.

If you wish, the random impedance network RIN follows the line of the random resistor network RRN, proposed already by Kirkpatrick (Rev. Mod. Phys. 45, 574, 1973) in the early 70s. Whereas Kirkpatrick proposed the model to deal with "two-phase systems in which one phase is much more conductive", the use and application of such model to inhomogeneous superconductors started in the last 10 years in our group in order to explain, in particular in the context of STO-based heterostructures, the long tails of resistivity measurements, the "anomalous resistivity" at $T = 0$, the large broadening of the metal-to-superconductor transition (see e.g. our ref [21]), the pseudo-gap signatures (see e.g. our ref [23]). All of those features cannot be accounted for by using paraconductivity effects nor the presence of impurities (see also comments below) but rather by the RRN percolative scenario (see also last comment).

> Further technical notes on text, organization, illustrations and references. Appendix B, on page 14 and 16 of the manuscript, is a word for word self-plagiarism of the Methods Sample growth paragraph on page 7 of the paper cited as reference [46]. Does it implies that samples and experimental data shown in this paper are not original, and have been done, and published, before? It is not explained clearly enough.

As mentioned in previous comments, the set-up was indeed the same as the one presented in Nature Mat. 18, 948 (2019), and Nature Commun. 13, 4625 (2022), while the sample is actually the same used in G. Singh et al, Nat. Comm. 9(1), 1 (2018) (ref. [46], now ref. [58]). So Appendix C is necessarily very similar to the methods section of ref. [46]. However, the data presented in this paper were not published anywhere else. Indeed, in G. Singh et al, Nat. Comm. 9(1), 1 (2018) the data were at $T \sim 0$ and various gate voltages with an emphasis on the experimental measurements. This work is instead devoted to the investigation and understanding of the temperature dependencies of $R(T)$ (DC measurements) and $\sigma_{1,2}(T)$ (AC measurements), which clearly do not follow any behaviour expected in a "homogeneous" sample, neither BCS nor BKT. We understand that the experimental details concerning the setup and the sample were not indeed clear to the reader. Hence, we will clarify this point in the main text of our revised manuscript and we are willing to add more experimental details in the dedicated Appendix.

> Next, what is the purpose of a full page picture no. 6. in Appendix C? Not a single word is devoted to this picture. BTW, it would be much clearer for a physicist reader to see the axes of graphs in units that reflect the physics of the problem rather than the engineering, such as the ratio T/T_c for temperature, E/δ for energy, and n/n_c for concentration, instead of the only technically relevant, like voltage, for example gate polarity. Why 4 volts step should be relevant, is the voltage-carrier density relation linear?

We thank the referee for pointing out the fact that we never really addressed Figure 6 (Figure 7 in the revised version) and we apologize for this. The aim of Figure 6 was to exhaustively present the *experimental* data from which our work was based. Thus, the choice of the axis: $\sigma_{1,2}$ in Ω^{-1}, R in Ω, and T in K. The figure was meant to give to the reader a clear vision of the three peculiar features discussed in Section 2 and summarized in Figure 1 and how they change with gate voltage. Instead, the choice of 4 V steps is rather arbitrary: the measurements were actually done changing the gate voltage V_G every 2 Volts. We decided to show the data every 4 Volts because we are convinced that adding more data will have the only outcome to disorder the work (which would be ironic) without really giving much more information to the reader.

The voltage-carrier relation is known to be non-linear in STO-based interfaces. The reason behind this non-linearity has to do with the band structure and the potential well that confines the 2D electron gas at the interface [see Y.-Y. Pai, Reports on Progress in Physics, vol. 81, no. 3, p. 036503, 2018.]. This is a well-known issue in literature, that however goes beyond the scope of the work we presented.

> It may be also worth discussing the role, or lack thereof, of the relationship between the frequency of the AC signal used to measure the impedance and the frequency (energy) of the energy gap associated with the formation of the condensate. Where and if 2Δ is $< 3.5kT$?

We thank the referee for their comment, we will clarify this point in the revised version of the manuscript. From the experimental point of view, the equivalence between the superfluid stiffness J_s and the inductance L_k is valid only in the circuit's frequency regime $\hbar\omega = h\nu \ll \Delta$. From Figure 2 (red curve, right axis), one sees that at optimum doping $\Delta^{\text{opt}} \lesssim 25\,\mu\text{eV}$ (corresponding to $T_c \lesssim 200\,\text{mK}$), giving an upper bound for the angular frequency of about $\nu = \omega/2\pi = 6\,\text{GHz}$. We therefore designed a circuit with a maximum resonant frequency of 500 MHz that satisfies the condition $\omega \ll \Delta$. On the other hand, the bandwidth of microwave components such as the cryogenic amplifier and the directional coupler imposes a low-frequency cut-off around 100MHz, hence our choice to work in the [100MHz-500MHz] range. Indeed, when $\hbar\omega \ll \Delta$, the system behaves as a resonant circuit and one can access the superfluid stiffness J_s directly from its inductive response, being $J_s = \frac{\hbar^2}{4e^2 L_k}$, where L_k is the inductance of the circuit.
The superconducting gap at zero temperature Δ^{exp} shown in Figure 2a (we report it here in Fig. R2 for simplicity) was extracted using the BCS formula for dirty superconductors $\Delta^{\text{exp}} = 4e^2 J_s R_{\text{N}}/\hbar\pi$. In what we call the underdoped (UD) region $2\Delta^{\text{exp}} < 2\Delta^{BCS} = 3.5k_B T_c$. This is an indication that we are outside the BCS scenario expected. In this paper, we explain this odd behavior in terms of filamentary superconductivity.

> Last, but not least, the reviewer's attention was also drawn to the selection of references, especially the percentage of self-citations. Without taking a position in the ongoing debate on this issue, the reviewer believes that 33% of self-citations by a leading author in the reference list is well outside the range considered appropriate. In conclusion I recommend against publication of submitted manuscript in the present form.

The selection of references has been made because the work presented follows a line of research in the context of *percolating superconductors* and *mesoscopic inhomogeneities* that our group has been pursuing for about 15 years now. We did not, for instance, mention the Kirkpatrick paper "Percolation and conduction" because we felt that this was only an inspiration of our work and cited it several times in our previous works. Nevertheless, we see the point of the referee and we will elaborate more about the context in which our paper is inserted, adding references from other groups to our bibliography. This idea of mesoscopic inhomogeneities is in fact becoming more present in literature, even in compounds different from STO-based interfaces.

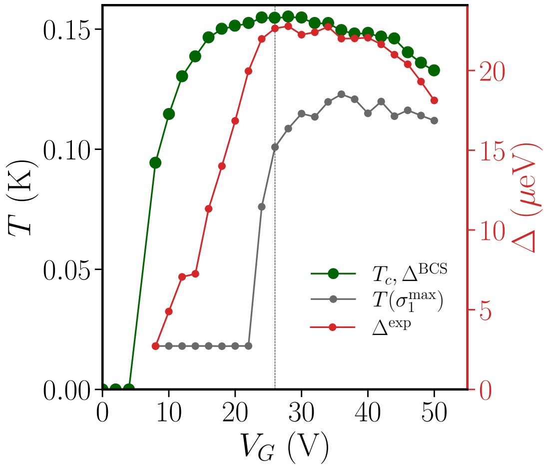

FIG. R2

We are confident that we have clarified all the points raised by the referee, as they will be in the revised version of our manuscript. We hope that the referee will now recommend our paper for publication in SciPost Physics.